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CONTENTS

CONTENTS

CONTENTS

CONTENTS

CONTENTS

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By DOROTHY M. WRINCH, Fellow of Girton College, Cambridge, Lecturer at University College, London.

PROF. HOBSON (*Proc. Lond. Math. Soc.*, 2, xviii, part 4, p. 249) has made an important contribution to the theory required for the further development of integral equations. His paper deals with the so-called Hellinger's integral, which arose primarily in connection with the theory of quadratic forms involving an infinite number of variables. Hellinger regarded them as a new species of limit, but Hahn reduced them to the ordinary integrals associated with Lebesgue. Prof. Hobson makes a simpler reduction by a more concise and simple method, and extends the whole theory to a wider class of integrals, of which these are, in fact, only special cases.

In order to define the nature of a Hellinger's integral, we suppose an interval $\beta-\alpha$ to be divided into n parts, α to y_1, y_1 to y_2, and so forth, the last being y_{n-1} to β. If $g(y)$ is a continuous monotone function of y defined for the whole interval, and such that $g(y)$ never decreases as y increases, while $f(y)$ is a continuous function defined for the interval, and constant in any part of it in which $g(y)$ is constant, then the sum

$$\sum_{r=1}^{r=n} \frac{\{f(y_r)-f(y_{r-1})\}^2}{g(y_r)-g(y_{r-1})}$$

when it has a finite upper limit, independent of the mode of division of the interval, is called a Hellinger's integral. Hellinger proposed that it be denoted symbolically by

$$\int_\alpha^\beta \frac{[df(y)]^2}{dg(y)}$$

in his paper in *Crelle's Journal*, vol. cxxxvi (1909). It is clear that a very difficult existence theorem is involved in the definition, and the fundamental result in this connection, derived by the author, is as follows:

If x is a variable equal to $g(y)$, and if $f(y) = F(x)$ when expressed in terms of this variable, then the necessary and

sufficient condition for the existence of the integral (*i.e.* of the limit defined above) is that $F(x)$ should be the indefinite integral of a function $\phi(x)$ whose square is summable in the interval (a, b) of x, which of course corresponds to the interval (α, β) of y.

Moreover, the Lebesgue integral to which the Hellinger limit is equivalent is merely

$$\int_a^b [\phi(x)]^2 dx$$

We have somewhat expanded the statement of the theorem given by Prof. Hobson himself.

The author proceeds, after establishing this very important result completely, to some generalisations. For example, if $f_1(y)$, $f_2(y)$ are two functions with the property of $f(y)$ in the preceding statement, then the sum

$$\sum_{r=1}^{r=n} \frac{[f_1(y_r)-f_1(y_{r-1})]\ [f_2(y_r)-f_2(y_{r-1})]}{g(y_r)-g(y_{r-1})}$$

denoted by Hellinger as

$$\int_\alpha^\beta \frac{df_1(y)df_2(y)}{dg(y)}$$

when the limit exists, is such as to possess the necessary limit

$$\int_a^b \phi_1(x)\phi_2(x)dx$$

as a Lebesgue integral if ϕ_1 and ϕ_2 are summable in the interval (a, b), and have F_1 and F_2 as their indefinite integrals. Much more extensive and fundamental generalisations follow, including, for instance, types of Hellinger integrals which converge to Lebesgue integrals of the form

$$\int_a^b f^n dx$$

where n is any power not restricted to integer values. For these we must refer the reader to Prof. Hobson's paper. The investigation appears likely to be of considerable utility in applied mathematics, where limits of the type defined are of not infrequent occurrence.

Other papers of interest in the same *Proceedings* (parts 4, 5, 6) are noticed more briefly below:

H. J. Priestley, part 4, p. 226, publishes a note on the values of n, which make the function

$$\frac{d}{dx} \cdot P_n^{-m}(x)$$

vanish at a point $x = a$. The problem arose in relation to the scattering of sound waves by a cone, and is also of interest for the pure mathematician. Prof. Carslaw called attention to the need for a proof of the fact that all the roots of this equation are real and separate. Prof. Priestley now supplies a proof derived in a very elegant manner from the theory of a Homogeneous Integral Equation.

J. Hodgkinson, part 4, p. 268, continues the problem of conformal representation of a curvilinear triangle, in some aspects previously developed in an incomplete manner.

W. P. Milne, part 4, p. 274, discusses the determinant systems of co-apolar triads on a cubic curve. The configuration of these triads had already been determined by a method which did not indicate the symmetry of its form, or any convenient method of visualising its structure as a geometrical unit. The paper now under notice establishes the symmetry of the configuration.

H. S. Carslaw, part 4, p. 291, discusses the diffraction of waves by a wedge of any angle, in which the older method of Sommerfeld and Carslaw has been superseded by the later methods of Macdonald, Bromwich, and others. The author shows that the solutions, which are already known, can nevertheless be obtained readily from the older method.

W. H. Young, part 4, p. 307, publishes a part of a long memoir on non-harmonic Fourier Series, which is concluded in part 5. It is related somewhat closely to a memoir which was noticed at some length in Science Progress, April 1920, and we shall state only the main problem, which is the possibility of expanding a function $f(x)$ in a series of the form

$$\sum_{n-\alpha}^{+\alpha} a_n cos(k+n)x$$

where k is not a whole number. Conditions for the existence of such an expansion, together with the equations for determining the coefficients, are discussed with some completeness, and some important applications to the series of Bessel functions follow.

W. H. Young, part 5, p. 339, discusses the formula for an area, familiar in elementary integral calculus, and submits it to an exhaustive examination. It has been usual to suppose the curve, whose area is discussed, to be a simple Jordan curve, or a curve defined by a continuous (1, 1) correspondence with a straight line or circle. It thus possesses an "inside" and an "outside," the former being a simply-connected region. The area can be defined as the common limit of the areas of two sets of polygons, one set inscribed and one set circumscribed, whose sides increase in number. This limit is the content of

the set of points inside the curve, with or without the points of the curve itself.

But the formula

$$\frac{1}{2}\int xdy - ydx$$

which thus gives the area when it exists, leads, when the integral is taken round a looped curve, only to the sum of the areas of the loops. Area is, in fact, a directed quantity—positive when the point describing it has the inside of the loop on its left.

The most important matter, in which more attention to directed area is needed, arises in the transformation of the variables in a multiple integral. In such transformations, as ordinarily presented, the variables are regarded as steadily increasing or decreasing—a supposition which fails in relation to functions of bounded variation, which will not usually be monotone or of uniform sign. Prof. Young takes up the problem of establishing the formula

$$\iint dxdy = \iint \frac{d(x, y)}{d(u, v)}\, dudv$$

under conditions which have no reference to the sign of the Jacobian, or even of the partial derivates, area being suitably defined. We may quote the author's suggested definition. Inscribe in a curve, supposed to be closed, a set of polygons as usual, of which the perimeters approximate to the length when the curve is rectifiable. Imagine vectors to be represented in magnitude, sense, and line of action by the sides of the polygon, supposed described in the sense in which a parameter, u, increases. Take the moments of these vectors about any point O in the plane. Then their sum is independent of O, and equal to twice the area of the polygon as usual when the polygonal line does not cut itself. Moreover, the sum has a unique limit $2A$ when the number of sides of the polygon is increased, so that the perimeter is the length of the curve if this be rectifiable. We define A as the area of the curve.

With this definition, complete precision appears to be attained in the statement of all the fundamental results of the calculus relating to areas. We shall not, however, quote the necessary modifications in their statements. The author has done considerable service in this exposition of some of the difficulties which even elementary students must have felt in regard to this rather neglected subject.

W. P. Milne and D. G. Taylor (part 5, p. 375) discuss the significance of apolar triangles in elliptic function theory. Much work has been done on this subject recently, but apolarity has not been applied to a great extent to curves which are not

rational. Some interesting applications to elliptic function theory are given by the authors.

D. M. Y. Somerville (part 5, p. 385) treats the singularities of the algebraic trochoids. He determines the number of fundamental singularities for the various types of curves, and also analyses the compound singularities at infinity.

E. K. Wakeford (part 6, p. 403) writes on canonical forms. The paper is posthumous, and formed the second part of a dissertation of the author. Being almost complete in itself, the Society has published it separately. The writer's object was to establish the possibility of reduction to canonical form rather than to find the reducing process.

In the *American Journal of Mathematics*, xlii, 1, p. 11, H. D. Frary publishes an interesting note on the Green's function for a plane contour. Its object is rather the practical than the exact solution of this problem, and a method is developed which should be of considerable service to the physicist in special cases. The Green's function depends on two points and on assigned conditions at a boundary of specified form, while satisfying a differential equation—usually, as in this instance, that of Laplace. The function G occurring most frequently in the applications is zero over the boundary, and if this can be found, Dirichlet's problem is soluble for any other condition of the boundary within wide limits. For if $w = \nu\ (\theta)$ on the boundary the solution is

$$2\pi w = \int \nu \frac{\delta G}{\delta n} ds$$

The ordinary methods of finding this function are of very limited application. There is the method of images, which can deal with the circle, semicircle, infinite strip, half plane, and certain triangles, but for other contours, hyperelliptic integrals are usually involved. The Schwarzian method, by the conformal representation of a polygonal boundary on a unit circle, has not been extended very effectively beyond the point at which Schwarz left it, for it again leads soon to abelian and hyperelliptic integrals, though for regular polygons the solution is capable of arithmetical evaluation and can be made practical.

The author follows a new method, with obvious relations to that of Fourier and Neumann. An infinite set of functions, linearly independent, satisfying the differential equation severally, is selected, and the Green's function expanded in a series of them with coefficients determined by the boundary conditions. To find these coefficients, we have an infinite set of equations in an infinity of unknowns, and the solution of these equations is possible in a variety of cases.

The plane contour used by the author means any closed plane curve regular in Osgood's sense, or composed of a finite number of analytic arcs or straight segments. The polygon is a special case. The Green's function in polars is

$$G(r, \theta) = -\log r + u(r, \theta)$$

where u satisfies Laplace's equation and has no singularities in the region concerned. The solution of the resulting equations can be obtained in a formal manner by integral equations, but is only formal, and it is necessary to use the author's method of direct solution of the equations, which, however, does not often give series with a general coefficient capable of recognition —this is not a matter of concern in the applications. The square contour is selected as a convenient illustration of the analysis.

W. G. Simon, in the same number, discusses the solution of some types of linear differential equations in an infinite number of variables, mainly with a view to proving the existence of certain forms of solution, chiefly exponential. His starting-point is an existence theorem of von Koch, and a generalisation of a theorem of Poincaré regarding the development of solutions of differential equations in power series of a parameter μ, when the functions appearing in the differential equations are themselves power series in μ. Considerable discussion of some forms of infinite determinants is included.

D. Buchanan, in the same Journal, discusses some interesting cases of Periodic Orbits on surfaces of revolution, the orbits being those of a particle under gravity, with the axis of the surface vertical.

F. Riesz (*Acta Math.*, xlii, 3, p. 192) discusses Lebesgue's integral. The memoir is a continuation of a note in the *Comptes Rendus* of 1912, in which the idea of the integral was introduced independently of the theory of measure, following a suggestion of Borel. The author's point of view is, however, quite different from that of Borel.

P. Lévy (*Acta Math.*, xlii, 3, p. 207) deals with Green's and Neumann's functions. The main initial problems proposed are:

(1) If ϕ_B^A is one of these functions, to determine ψ_B^A in such a way that $\phi_B^A - \psi_B^A$ is a holomorphic function of the points A and B.

(2) To form a function ψ_B^A such that the difference $\phi_B^A - \psi_B^A$ may be finite, with all its derivatives, to a given order.

These functions ψ are treated like the Green and Neumann functions in the sense that, once obtained, they are capable of

simple verification. Many interesting general theorems on harmonic functions are developed.

The same Journal contains valuable accounts of the work of D. Hilbert and G. Darboux.

G. Fubini (*Rendiconti di Palermo*, xliii, 1, p. 1) discusses the fundamentals of projective differential geometry. The fundamental differential equations are deduced in curvilinear coordinates by a purely projective method.

C. Bonomi (p. 46), in the same number, describes the theory of a special type of hyperelliptic surface.

F. Gerbaldi (p. 78) treats the continued fractions of Halphen in relation with (2,2) correspondences and the Poncelet polygons.

P. Nalli (p. 105) makes an interesting contribution to the theory of integral equations with a symmetrieal kernel $k(s, t)$.

A. Palatini publishes in *Rendiconti di Palermo*, xliii, 1, p. 192, two important contributions. The first, on the fundamentals of the absolute differential calculus, in the sense associated with the recent Einstein theory, discusses invariant and covariant systems, with their addition and multiplication, derives the covariants of a mixed system, and an interesting integral formula. The second proceeds to the invariantive deduction of the gravitational equation from Hamilton's principle.

A. F. Dufton, *Proc. Roy. Soc.*, A, xc, describes a model made for the drawing of conics. The principle used is that the conic is the polar reciprocal of a circle. This has not been used before, and the author has found a simple mechanism which gives a real practical solution of a very old problem.

J. W. Nicholson, *Proc. Roy. Soc.*, A, xc, 1920, in a paper on the lateral vibrations of sharply pointed bars, continues his investigations of 1917. The vibrations of bars of circular cross-section formed by the revolution of the curve $y = Ax^n$ about the axis of x are now completely discussed for values of n between 0 and 1, and for the isolated value $n = 2$. The problem is interesting to the pure mathematician, since for its general solution, for all values of n, functions which are generalisations of the Bessel functions are needed. The properties of these functions are not yet adequately worked out.

ASTRONOMY. By H. Spencer-Jones, M.A., B.Sc., The Royal Observatory, Greenwich.

The Secular Acceleration of the Moon.—It has long been known that, in order to make the theory of the moon's motion agree both with the modern observations and with ancient and mediæval observations of eclipses and occultations, it is necessary to introduce into the theory certain empirical or quasi-empirical terms. These consist of certain periodic terms of long period with the addition of a term which indicates that

the moon has an apparent acceleration of its mean motion relative to the sun. The period of the principal periodic term is about 240 years; the length of this period renders it difficult to separate the term depending upon the acceleration from the periodic term. The problem of determining the acceleration is therefore to some extent indeterminate. The most probable value has been discussed in some important papers recently by Dr. J. K. Fotheringham, which will be reviewed in these notes in the next issue of SCIENCE PROGRESS. From these, it appears that an acceleration of 10″ per century must be very near the truth. Of this an amount of 6″·1 can be explained by purely gravitational causes.

Various explanations of the remainder of the secular acceleration have been put forward from time to time, but until recently there was no theory which could be regarded as wholly satisfactory. In default of any more plausible explanation, it has generally been accepted, though without quantitative evidence, that the balance was due to tidal friction. Some recent work by G. I. Taylor has placed this theory on a firmer theoretical basis. In a paper entitled "Tidal Friction in the Irish Sea" (*Phil. Trans.*, A. **220**, 1–33, 1919) he has calculated, by two independent methods, the mean rate of dissipation of energy by tidal currents in the Irish Sea at spring tides. One estimate is derived from an expression for the friction between a tidal current and the sea bottom; the other is based upon the rate at which energy enters the Irish Sea through the north and south channels, and the rate at which lunar attraction does work on the waters of the Irish Sea. The respective values derived by these two very different methods are 1,300 and 1,530 ergs per sq. cm. per second, an agreement which lends support to the validity of the argument. From this result Taylor calculates in a paper, "Tidal Friction and the Secular Acceleration of the Moon" (*M.N., R.A.S.*, **80**, 308, 1920), that the mean rate of dissipation of energy in the Irish Sea is about 3×10^{17} ergs per second. Dr. H. Jeffreys has discussed the theoretical bearings of this theory of tidal friction ("The Chief Cause of the Lunar Secular Acceleration," *M.N., R.A.S.*, **80**, 309, 1920), and finds that the rate of dissipation of energy necessary to account for the unexplained part of the secular acceleration of the moon is about $1{\cdot}4 \times 10^{19}$ ergs per second. Taylor therefore remarks that, on his above estimates, the Irish Sea contributes $\frac{1}{50}$th part of the total dissipation. The Irish Sea covers only $\frac{1}{10,000}$th part of the water area of the globe, but very little tidal dissipation occurs in the open ocean; the amount only becomes appreciable in long bays and channels where the water is relatively shallow. The number of these is very

limited, and it therefore appears that the dissipation of energy in the tides is of the right order of magnitude to account for the unexplained part of the moon's secular acceleration.

Dr. Jeffreys, in the paper referred to above, proceeds further and shows that, on a tidal friction theory, it is possible, when a value is assumed for the lunar secular acceleration, to calculate the value of the secular acceleration of the sun. If the value so calculated is found to agree with the value derived from observation, it would afford further support to the theory. He considers various types of tidal friction. Tidal friction might occur in the body of the earth, being due to imperfection of elasticity, which latter may be regarded as a combination of plasticity and afterworking. Plasticity in the outer crust of the earth would damp out the 14-monthly variation of latitude, whilst afterworking would render the crust impermeable to earthquake waves. It is possible, however, that there may be sufficient plasticity near the centre to account for the phenomenon, though it is concluded that the balance of evidence is against an explanation involving bodily friction. It is shown also that atmospheric phenomena and tides in mid-ocean are not capable of accounting for the secular acceleration. The theory of dissipation in regions of strong tidal currents is discussed mathematically, and it is shown that, to account for the observed lunar acceleration, a secular acceleration of the sun of $0''\!\cdot\!78$ per century is necessary. This value does not differ greatly from the most probable value of about $1''$ arrived at by Fotheringham. It is further proved that this theory does not give rise to difficulties due to the existence of the 14-monthly period of latitude-variation.

It would therefore appear that Taylor's theory gives the first satisfactory explanation of the lunar secular acceleration.

Studies of the Nebulæ.—Vol. xiii of the Publications of the Lick Observatory contains a detailed account of the extensive and important studies of the nebulæ which have been carried out by the Lick observers during the past several years. The volume is in the main a record of the observational material which has been gathered together. Although much of this material cannot yet be fitted into a theoretical framework, an attempt has been made to do so wherever possible. Most readers will probably agree, however, that the volume raises more problems than it solves. It therefore possesses great interest both for the practical and for the theoretical astronomer.

Part I contains descriptions of 762 nebulæ and clusters which have been photographed with the Crossley reflector, by H. D. Curtis. In connection with this work a count has been

made by Curtis of all the small nebulæ occurring on his plates, and from this count an estimate has been made of the total number of spiral nebulæ within the reach of modern large refractors. Estimates had previously been made by Perrine in 1904, who gave a total of 500,000, and by Fath in 1913, who reduced this figure to 162,000; both estimates were based upon counts in a number of selected regions. Curtis very considerably increases both of these estimates and places the number at 722,000, with the remark that, since the faintest and smallest members of the class are, in general, discernible only in the central regions of the plate, this figure is an underestimate, and that the total number accessible with the Crossley reflector with rapid plates and with exposures of from two to three hours may well exceed 1,000,000. A critical discussion of the wide divergence between these three estimates supports the largest of them. The distribution with reference to the Milky Way is of considerable interest, and is summarised as follows:

Galactic Latitude.	Number of Regions.	Square Degrees.	Number of Spirals.	Number per Square Degree.	Number in Area.
$+45°$ to $+90°$	117	88·50	2,997	34	205,000
$-45°$ to $-90°$	43	32·25	918	28	169,000
$\pm 30°$ to $\pm 45°$	62	46·50	1,117	24	204,000
$-30°$ to $+30°$	217	162·75	1,179	7	144,000

The density is greatest in the neighbourhood of the north galactic pole; there is a less marked concentration near the south galactic plane. A high value of the density persists to at least 60° from the galactic poles, but then shows a remarkable diminution in the neighbourhood of the Milky Way itself.

Part II contains a study of occulting matter in the spiral nebulæ, also by H. D. Curtis. It is well known that certain spiral nebulæ, when seen edgewise on, show a dark lane running down the length of the spiral, which is generally explained as being due to absorption of light by the outer layers of the nebulæ. Curtis shows that this phenomenon is more common than had been supposed, and excellent reproductions are given of 77 Crossley photographs of spiral nebulæ which show the dark band. Arguments are brought forward to support the view that the appearance is not due to a phase effect.

The planetary nebulæ, which are very few in number and have a distribution differing widely from that of the spiral nebulæ, are considered by Curtis in Part III. A valuable feature of this paper is a series of pictures of all the known planetary nebulæ, 78 in number, which are north of 34° south declination. The wide divergence of forms shown by these nebulæ, including helical and ellipsoidal formations, rings with and without nuclei, series of concentric shells, etc., makes

it difficult to form a rational theory of their structure and life-history. When to this difficulty is added the necessity of accounting at the same time for the complicated spectrographic information, the problem would seem at first sight to be impossible to solve. Curtis classifies the forms shown by these objects into seven general types, and proves that some of the simpler types can be reasonably explained on the hypothesis of oblate spheroidal shells of matter, which in some cases may be approximately homogeneous, but which in others must be thinner at the equatorial zone than at the poles. A falling in of the matter from the polar zones would then give a possible explanation of the peculiar S-shaped spectral lines given by some of the nebulæ. The planetaries are difficult objects to fit into the scheme of stellar evolution, and this matter is not solved in the present volume. The only statement which can definitely be made is that the planetary stage of existence must be relatively very brief—otherwise the number of planetaries would be much larger. In this they resemble the Wolf-Rayet stars, with which their nuclei show an essential identity. It is very improbable that the rarity of the planetary nebulæ is due to large numbers remaining to be discovered; the Harvard spectrographic survey of the sky has increased the number of classified stellar spectra from about 9,000 to over 200,000, but this increase of over 2,000 per cent. has only added one new object with a spectrum of planetary type.

Part IV, the spectrographic velocities of 125 bright-line nebulæ, by W. W. Campbell and J. H. Moore, is an important contribution to our knowledge of the velocities of nebulæ and of the complicated structure of various nebular lines. Full details are given of the observations of each nebula. Several results of interest are obtained. The average velocity of the planetary nebulæ, after separating the component of the solar motion, is found to be about 30 km. per sec. Since this is many times larger than the average velocity of the B-type stars, it does not seem probable that the latter have evolved from planetary nebulæ. Seventeen nebulæ have been observed in the Greater Magellanic Cloud, and one in the Lesser Cloud. The former give a mean velocity of 363 km. per sec. recession, and the latter gives a velocity of 307 km. per sec. recession. The velocity of the Greater Magellanic Cloud is considered in more detail by R. E. Wilson in the next part. The probable masses of three planetary nebulæ whose parallaxes have been determined are estimated on reasonable assumptions, and found to be much greater than the mass of our solar system. The behaviour of the spectral lines taken in conjunction with the photographic evidence leads Campbell and Moore to conclude that many of the planetaries may be

approximate ellipsoids of revolution, in general larger than they appear to be, especially in their equatorial regions, by virtue of invisible cooler strata lying outside the visible structure; the cooler, more slowly rotating strata cause reversals in the spectral lines which account for many of the abnormalities.

The last part contains determinations of the wave-lengths of the nebular lines, with general observations of the spectra of the gaseous nebulæ, by W. H. Wright. This paper is divided into three parts dealing with (1) the measurement of wave-lengths and the intensities of nebular lines in 48 nebulæ; (2) the study of the nebular nuclei; (3) the investigation of the distribution of nebular radiations throughout the nebulæ. Definitive wave-lengths of the various nebular lines are adopted from the means given by nine nebulæ; many of the lines were first observed in these investigations; when known, the probable origins are given. Half of the nuclei were found to be of the O-type, but the exact order of evolution remains uncertain. Many interesting subjects are touched upon, including a scheme, based on spectroscopic evidence, for the classification of the bright-line nebulæ. Lack of space prevents adequate reference to these, and the reader must refer to the book itself, which is well worth study by all who are interested in Astronomy.

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METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

The Characteristics of the Free Atmosphere (W. H. Dines, F.R.S., *Geophysical Memoirs*, No. 13).—This Report summarises much of the information concerning the physics of the atmosphere up to a height of about 20 km., that had been obtained up to 1916 by means of free balloons carrying self-recording instruments. Of the observations made in this way, about 90 per cent. have been obtained in Europe.

Temperature.—A table is given showing the mean for the whole year for every kilometre of height up to 14 km. for a number of European stations between the latitudes of Petrograd and Pavia (Italy), and also for about lat. 43° N. in Canada. A second table shows the mean vertical gradient of temperature for the same places. A very evident feature is the lower value of the temperature at the more northern stations up to 8 or 10 km., and the higher temperature above that height. The maximum gradient or lapse-rate of 7·4° C. per km. occurs between 7 and 8 km. Above that height the rate falls off to practically nothing above about 12 km., this change taking place at a lower level for northern than for southern stations. The comparatively scanty information from near the equator (*Monthly Weather Review*, Nov. 1915) shows that the mean temperature does not reach its lowest value there until a height of 17 km. is reached. The value of 193° A. for this height is far colder than the mean for the same height in Europe, which is 219° A. The table for mean monthly temperatures shows that the seasonal variation is

very much the same up to about 10 or 11 km., but becomes smaller higher up. The diurnal variation on the other hand appears to decrease rapidly with height and probably ceases above 2 km.

Moisture.—Relative humidity generally increases up to the level of the lowest clouds, *i.e.* at about 1 or 2 km., and decreases above, but for great heights the hair hygrometer will not give reliable values on account of the low temperatures encountered.

The Troposphere and Stratosphere.—Mention has already been made of the fact that the mean temperature is the same at all levels above 12 km. over Europe. In individual cases the lapse-rate generally becomes zero very abruptly, the height at which this occurs (H_c) varying considerably on different days. The region below this point is generally known as the troposphere and the region above as the stratosphere. The mean value of H_c varies from 10 km. to 11 km. at the different European stations, and shows a marked seasonal variation with the minimum in February. H_c is about 13 km. on the average in anticyclones and about 8 km. in cyclones. In the concluding pages of the Report mention is made of a close connection that is found between H_c and P_9 (the pressure at 9 km.), and between H_c and T_m (the mean temperature of the air column between 1 km. and 9 km.). By the method of Partial Correlation, it is found that if allowance is made for the effect of T_m, the variations of H_c follow those of P_9 very closely indeed. There is no obvious reason why this should be so.

Pressure.—The mean pressures for the different European stations differ by a few millibars at 1 km., and by rather more (about 12 mb.) at 7 km., but above 12 km. the differences rapidly diminish, and at 20 km. the pressure is everywhere practically the same. Near the equator the mean at 7 km. is 430 millibars, as compared with 408 mb. over Europe, but the mean at 20 km. differs little from the mean for the same height over Europe.

Wind.—Some information is given by the position of the falling place of the registering balloons sent up on the Continent, the English ascents being excluded because so many balloons fall into the Channel. The mean drift is found to be towards S.E. by E. Those balloons which reach great heights do not seem to travel farther than those attaining a moderate height, so that there is some evidence in support of the theory that the wind velocity falls off rapidly with increasing height in the stratosphere.

The Connection between Pressure and Temperature.—Over Europe, between the levels of 1 km. and 20 km., the following

broad rules hold: In high-pressure areas the lower strata are warm, the fall of temperature with height continues to about 12 km., and the strata above 12 km. are cold. In cyclonic areas the lower strata are cold, the vertical temperature gradient ceases at a lower level than usual, and above 10 km. the strata are warm. The temperature of the air depends closely upon the pressure and upon the time of year, but except quite close to the earth it does not depend upon the direction of the wind.

Since there are strong reasons against supposing that the coldness of the lower layers of a cyclone are due to loss of heat by radiation, we are driven to ascribe it to dynamical cooling. Calculation shows that air at a height of 6 km. in an anticyclone would have to rise 2·4 km. on entering a cyclone in order to cool dynamically to the temperature of the cyclone. On the other hand, air between 12 km. and 14 km. in the anticyclone would, on entering the cyclone, have to descend 1·3 km. in order to warm dynamically to the higher temperature of the latter. That a general rise of air does actually occur in the lower parts of a cyclone is proved by the indraught of the surface winds across the isobars. The difficulty consists in seeing exactly how this upward current is induced, how, in fact, cold and heavy air which one would expect to be sinking is forced to rise. The solution of this problem would constitute a big advance in meteorological theory.

Atmospheric Stirring measured by Precipitation (L. F. Richardson, *Roy. Soc. Proc.*, **96**, Aug. 1919).—Since the average water-content of the atmosphere is not increasing, the water which descends as precipitation must have been stirred up into the atmosphere. Consequently, from a knowledge of the mean precipitation at any height taken over the whole earth, the mean amount of stirring can be deduced by suitable mathematical processes. The heights dealt with are 0·5 metre, 500 metres, and 8,500 metres. Over this range the density of the air and the coefficient of stirring cannot be taken as being independent of the height, as was done by G. I. Taylor *Phil. Trans.*, A, vol. 215, 1915) in his pioneer work on this subject, consequently a more general treatment has been followed. In order to measure the stirring, a quantity ξ was used to replace the eddy-diffusivity κ, of Taylor's equations, ξ being defined by the equation

$$\frac{\delta\chi}{\delta h} = \frac{\delta}{\delta p}\left(\xi\frac{\delta\chi}{\delta p}\right)$$

where p = pressure
h = height
χ = mass of water per unit mass of atmosphere.

It is assumed that in the free air the rainfall at 500 metres does not differ materially from that at 0·5 metre, both heights being almost always below the level of the rain clouds, so that ordinary rainfall statistics can be employed for these heights. For the level of 8,500 metres consideration of the mean cloudiness at that height, together with the size of the ice particles, furnishes a rough estimate of the precipitation. From these data the following mean values were found for ξ :

Height :

8,500	metres	3 to 180 cm.$^{-2}$ grm.^{2} sec.$^{-5}$
500	,,	140,000
0·5	,,	1,000 or even less.

The stirring is therefore very much greater at 500 metres than at the other levels ; and it is interesting to observe that the value found for this height when transformed into its equivalent value of κ, agrees very closely with the mean deduced by G. I. Taylor from an analysis of the temperature records at the Eiffel Tower.

Wind Velocity in the Stratosphere (M. J. Rouch, *Comptes Rendus*, June 23, 1919).—The notion that the velocity of the wind must diminish with increasing altitude on passing from the troposphere to the stratosphere, *i.e.* at a height of about 11 km., is an attractive one from the theoretical point of view, and were it found to be true one would be able to determine the height of the stratosphere by observations of wind alone. During the war a number of pilot-balloon ascents were made at coast stations in France, and those which attained to an altitude of 11 km. or more have been tabulated to see whether they throw any light upon this question. Taking the velocity at 10 km. as a standard of reference, departures from this have been calculated to the nearest metre per second, and their frequency of occurrence is shown in the following table :

Height.	No. of obs.	Departures from velocity at 10 km.										
		−5	−4	−3	−2	−1	0	1	2	3	4	5
		Frequency.										
11 km.	36	2	3	—	1	7	3	6	7	3	1	3
12 ,,	26	—	1	2	—	5	1	4	6	—	4	3
13 ,,	17	—	—	—	1	—	1	2	2	4	—	7
14 ,,	10	1	—	—	—	—	1	—	—	2	2	3
15 ,,	7	1	—	—	—	1	—	—	—	—	1	4

These figures do not accord with the theory of a falling off of the wind, as they tend to show that the wind continues to increase with increasing altitude above the base of the stratosphere. It must be borne in mind, however, that such altitudes are only attained on days specially favourable owing to

absence of strong winds or cloud. Moreover single theodolites were used, and in calculating the height of the balloon it was assumed that the rate of ascent was the same at great and at moderate altitudes, which has not yet been certainly proved true.

Rate of Ascent of Pilot Balloons (C. J. P. Cave and J. S. Dines, *Q. J. Roy. Met. Soc.*, Oct. 1919). In a previous paper (*Q. J. R. Met. Soc.*, vol. 39, 1913) Dines found, as a result of experiments in a balloon shed at Farnborough, the following empirical formula connecting the rising velocity in still air with the dead weight and free lift of a balloon:

$$V = q\frac{\sqrt{L}}{\sqrt[3]{W+L}}$$

where V = rising velocity in metres per min.
L = free lift in grammes.
W = weight of balloon in grammes.
and q is a constant for a particular balloon.

It should be noticed that V is directly proportional to this quantity q. This formula is now in general use in this country. The question of the rate of ascent of a balloon is, however, not quite so simple as the above formula implies, and in any case the introduction of small lanterns suspended beneath the balloon for use in locating its position at night made further experiments desirable. These were carried out in the Royal Albert Hall.

A difficulty encountered throughout the work was the fluctuation in the speed of the same or similar balloons from time to time. A series of observations would yield values of q which varied but slightly among themselves, yet which differed by as much as 5 per cent. from the mean of a longer series. Since no explanation could be found to account for this, a large number of observations had to be made when investigating any particular point. The first experiments were on the effect of loading. By hanging a small weight to the neck of a balloon, the rate of ascent was in certain cases increased, *e.g.* with a 30-grm. balloon having 120-grms. lift, the variations in q for different loads were as follows:

Load	0	5	10	15	20	30	40 grms.
q	83·6	87·0	87·4	87·0	84·9	84·7	84·2

When loaded, the balloon was observed to ascend in a much less zigzag course than when in the unloaded state, and it was doubtless for this reason that the ascensional velocity was greater up to a certain point.

The next experiments were to determine whether the cotton which was normally attached to the balloon to assist in measur-

ing its rate of ascent had any effect upon the velocity. This cotton was unwound by the balloon as it rose, and the interval between the times when two small pieces of white paper, attached at a measured distance apart, left the ground was timed by means of a stop-watch. For balloons whose weight varied between 11 gm. and 21 gm., the mean value of q with cotton was 81·3, and without cotton 83·2, the difference being greatest for the smaller balloons. Lastly the effect of hanging a small lantern 2 metres below the balloon was studied. The weight of the lantern was of course added to the weight of the balloon when calculating q. It was found, from a number of experiments made on different days, that the mean for q without a lantern was 83·5 and with a lantern 83·2, so that the lantern had no appreciable effect. The explanation offered for this is that the increased air-resistance offered by the lantern is practically compensated for by the steadying effect of adding this small load, to which allusion has already been made. In conclusion, mention should be made of a result obtained from many observations, namely, that for balloons weighing between 11 gm. and 16 gm. $q = 80$, and for those between 20 and 35 gm. $q = 84$, on an average.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Resonance Potentials and Ionisation Potentials of Hydrogen.—The behaviour of hydrogen molecules and atoms when bombarded by electrons of various velocities, has been investigated recently by several independent observers. The importance of such investigations lies in the fact that they afford a means of obtaining information regarding the structure of the hydrogen molecule and atom. The conclusions arrived at may then be employed to test the various theories of molecular and atomic structure which have been proposed. It is somewhat remarkable that, in spite of the care and precautions with which these independent investigations have been made, the results are by no means wholly concordant. Nevertheless sufficient progress has been made in this field to warrant a brief summary of the conclusions. Incidentally it may be recalled that a resonance potential denotes the energy of an electron which is capable of bringing about a change in the molecule of the gas (hydrogen) in which, however, no electron is set free, *i.e.* a change such as $H_2 \rightarrow 2H$, the dissociation of the molecule into electrically neutral atoms. On the other hand, an ionisation potential represents the energy which must be imparted to an electron in order that, by its collision with an atom or molecule, it may cause an electron to be eliminated, *i.e.* the processes: $H_2 \rightarrow H_2^+ + \Theta$, or $H \rightarrow H^+ + \Theta$.

We shall first of all consider the results obtained by Franck, Knipping, and Kruger (*Ber. Deutsch. Phys. Ges.*, 1919, **21**, 728). As a result of carefully repeated experiments, these investigators recognise four distinct stages corresponding to four definite changes which the hydrogen molecule can undergo.

First, there is a weak but appreciable ionisation of the gas at $11 \cdot 5 \pm 0 \cdot 7$ volts. This is ascribed to the ionisation of the molecule thus: $H_2 \rightarrow H_2^+ + \Theta$. In support of this view Franck and his collaborators point out that the ion formed at this potential is of molecular, and not atomic, dimensions, thereby eliminating the possibility of ascribing the effect to $H \rightarrow H^+ + \Theta$, which might be expected to occur at a higher potential.

Secondly, it has been observed that a resonance potential manifests itself (photo-electrically) at $13 \cdot 6 \pm 0 \cdot 7$ volts. This is ascribed to a non-electrical process, namely the dissociation of the hydrogen molecule into two atoms, one of which is normal, the other contains two quanta. This change (reversed) corresponds to the first line of the Lyman series in the far ultraviolet. This potential is written by the authors as $(10 \cdot 1 + Q)$ volts, where $Q = 3 \cdot 53 \pm 0 \cdot 25$ volts. This term will be considered later.

In the third place, a strong ionisation has been observed at $17 \cdot 1 \pm 0 \cdot 27$ volts, which is written as $(13 \cdot 5 + Q)$ volts. This is regarded as the ionisation potential of the hydrogen molecule into an atom, a nucleus, and an electron, thus: $H_2 \rightarrow H + H^+ + \Theta$.

Finally, a new ionisation stage has been observed at the very high voltage $30 \cdot 4 \pm 0 \cdot 5$, which is written as $(2 \times 13 \cdot 5 + Q)$ volts, and is regarded as corresponding to the most violent change of which the neutral hydrogen molecule is capable, namely its ionisation into two nuclei and two electrons, thus: $H_2 \rightarrow 2H^+ + 2\Theta$.

The quantity Q ($3 \cdot 53$ volts), which seems to occur in at least three of the observed stages, is ascribed to the dissociation of the hydrogen molecule into neutral atoms. As evidence of the general correctness of this view, it is pointed out that $3 \cdot 53$ volts would correspond to 81,300 cals. per gram-molecule of hydrogen, a quantity which agrees fairly well with the heat of dissociation of hydrogen, 84,000 cals., as observed by Langmuir. This resonance potential, $3 \cdot 53$ volts, was not observed directly, though presumably it was looked for. The photoelectric effect produced by it was evidently too weak to manifest itself under the experimental conditions employed. It would obviously be of the first importance to determine its position accurately and directly.

It may be pointed out that by subtracting stage 3 from stage 4 we obtain the value $13 \cdot 3$ volts as the ionisation potential of the hydrogen *atom*, viz. $H \rightarrow H^+ + \Theta$.

Values apparently differing from those of Franck, Knipping, and Kruger have been obtained by other workers. Thus Bishop as well as Davis and Goucher found, somewhat earlier, that ionisation sets in at a potential of about 11 volts (already discovered by Franck and Hertz), and also that a second ionisation potential exists at 15·8 volts. Further, Horton and Miss Davies (*Proc. Roy. Soc.*, 1920, **97**, A, 23) have recently found the following potential stages :

First, a radiation or resonance potential at a minimum electron velocity equivalent to 10·5 volts. This is presumably the same as the 10·1 volt inferred by Franck and his collaborators as a possible resonance potential of the molecule. Horton ascribes this effect to radiation from the atom, by displacement of an electron from one orbit to another.

Secondly, a further type of radiation resonance is produced at 13·9 volts. Horton ascribes this to radiation from the molecule. This is presumably the same as the resonance potential observed by Franck at 13·6 volts.

Thirdly, Horton and Miss Davies find that ionisation of the gas occurs at 14·4 volts. This value does not appear to have any counterpart in the observations of Franck. It may be pointed out that 14·4 volts corresponds to the wave length $84\mu\mu$, which lies fairly close to the accepted value of the dispersional wave length, 86 to $87\mu\mu$, obtained from refractive index data. Horton ascribes this stage to ionisation of the *atom*, viz. $H \rightarrow H^+ + \ominus$. It will be recalled that the value calculated for this process on Franck's data is 13·3 volts. The results are discordant.

Finally, Horton and Miss Davies have observed a second type of ionisation at 16·9 volts. It is ascribed to ionisation of the molecule, namely $H_2 \rightarrow H + H^+ + \ominus$. This is presumably the same potential as that observed by Franck and his collaborators at 17·1 volts, to which the same chemical significance is attached. Horton and Miss Davies do not appear to have observed the 15·8 volt potential of Bishop, Davis and Goucher.

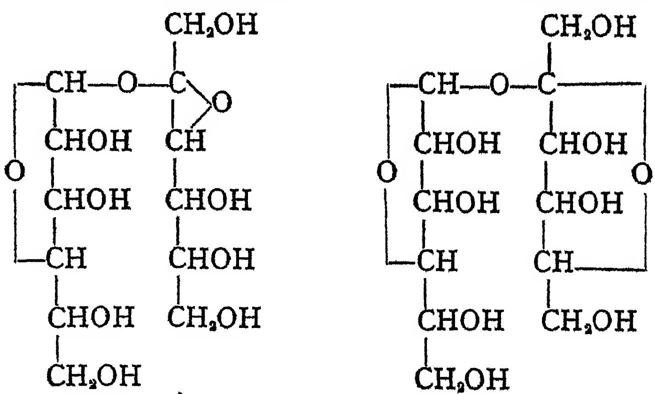
Although the various values quoted show a rough general agreement, the discrepancies seem to be beyond the experimental error involved in any actual determination. This naturally raises the question as regards the interpretation of the experimental results. It is impossible at the present time to make any definite statement in this connection, but reference may be made to a paper by van der Bijl (*Physical Rev.*, 1917, **10**, 546), who concludes that three causes operate to affect the value ascribed to a true ionisation potential. It is evident that this aspect of the problem requires further investigation by physicists. If reliable and concordant values could be obtained for hydrogen and other gases, it would represent a considerable

advance from the purely chemical point of view, for the energy quantities involved in such submolecular processes must play a fundamental part in the "ordinary" chemical changes which these substances are known to undergo.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., University College, London.

THE generally accepted idea with regard to the action of oxidising agents on acetylene is that they break this compound down completely to carbon dioxide or else oxalic acid. In two recent papers, however, it has been shown that, by carefully regulating the interaction, other products can be isolated. Thus it has been shown by Wohl and Braünig (*Chem. Zeit.*, 1920, **44**, 157) that ozone, suitably diluted with air, acts on acetylene in the presence of moisture to produce a mixture of glyoxal and formic acid. By spraying water on to the reacting gases, a 2 per cent. solution of glyoxal may be obtained, but the concentration may be increased by spraying with the glyoxal solution instead of water. The authors suggest a possible technical application of this reaction for the preparation of indigo by converting the glyoxal into its bisulphite compound and causing this to react with aniline. The second paper referred to is one by Orton and McKie (*J. Chem. Soc.*, 1920, **117**, 283) on the action of nitric acid on acetylene; by varying the conditions of concentration and temperature and using metallic salts as catalysts, these authors have been able to obtain nitroform and tetranitromethane, the latter in sufficient yield to make the method a possible source of a high explosive from inexpensive materials.

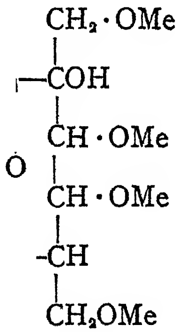
In the course of a systematic study of the constitution of the Disaccharides, Haworth (*J. Chem. Soc.*, 1920, **117**, 199) brings forward evidence which supports his constitutional formula I—



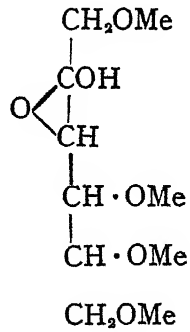
I. II.

for sucrose as against Fischer's formula II. It will be seen that the difference between the two formulæ lies in the fructose moiety, which in the case of Fischer's formula is represented as having a butylene oxide structure, whereas in formula I it contains the ethylene oxide complex. Evidence in favour of the latter constitution is furnished by the hydrolysis with 0·4 per cent. hydrochloric acid of a partially methylated sucrose, namely heptamethyl sucrose; this compound yielded a mixture of tetramethyl fructose and trimethyl glucose, which two compounds could be readily separated by distillation at the Gaede pump.

The tetramethyl fructose has not the butylene oxide structure indicated in formula III, and being very unstable towards permanganate, it conforms in general to the new type of hexoses known as ethylene oxide sugars; from the fact of its giving on oxidation a lactone, formula IV is assigned to the new tetramethyl fructose, since this formula readily accounts for the formation of an α-hydroxyacid which must have been the precursor of the lactone.



III.



IV.

The fact that sucrose itself, on hydrolysis, yields glucose and ordinary butylene oxide fructose is explained by the assumption that the ethylene oxide form of fructose is really formed in the first instance, but that it undergoes a molecular transformation into the butylene oxide form at the moment of cleavage; when, as in heptamethyl sucrose, the hydroxyl groups are protected, no such transformation can take place. The existence of a fructose of the ethylene oxide type is of considerable interest in view of the evidence for the existence of an ethylene oxide glucose (cf. Irvine, Fyfe, and Hogg, *J. Chem. Soc.*, 1915, **107**, 524).

In a paper entitled, "Lead Triaryl, a Parallel to Triphenylmethyl" Krause and Schmitz (*Berichte*, 1919, **52**, [B], 2165) describe a crystalline lead tri-p-2-xylyl, $Pb(C_6H_3Me_2)_3$, in which

the lead is attached to carbon atoms by fewer than four valencies; the new compound, which is prepared by the addition of finely powdered lead chloride to an ethereal solution of magnesium p-2-xylyl bromide, gives molecular weight determinations agreeing with the formula $[(C_8H_9)_3Pb]_2$. It is assumed that the lead atoms are united in much the same manner as the carbon atoms of the triaryl methyls. The substance is readily attacked by bromine, giving lead tri-p-2-xylyl bromide; although in solution it is rapidly and completely decomposed on exposure to light, the solid is remarkably stable towards air.

GEOLOGY. By G. W. Tyrrell, A R C Sc., F.G.S, University, Glasgow.

Economic Geology.—During 1918 the Geological Survey of the United Kingdom was still almost exclusively occupied with economic work of military or commercial importance (*Summary of Progress for* 1918, 1919, p. 70). In the summary of progress the lead- and zinc-mines of Scotland are specially treated. There is also an interesting account of the bauxitic fireclay of North Ayrshire, which has proved of great value as a refractory. This occurs in the Millstone Grit, and overlies a series of decomposed basic lavas. Its intimate association with beds rich in iron and resembling laterite suggests that its origin was similar to that of bauxite. The compact, non-porous character of the clay may be due to subsequent silicification, but it may originally have been deposited in its present form.

An important monograph, by E. C. Harder, on iron-depositing bacteria and their geological relations, has been published by the United States Geological Survey (*Prof. Paper*, **113**, 1919, p. 89). Harder shows that in addition to the iron bacteria proper, it is probable that many of the common bacteria of soil and water cause the precipitation of ferric hydroxide from solutions containing iron salts of organic acids. The second part of the work contains an excellent review of sedimentary iron ores and the part played by organic agencies, including bacteria, in their deposition.

Several reports have recently been issued on the iron-ore deposits of the United Kingdom (F. H. Hatch, "Recent Iron Ore Developments in the United Kingdom," *Geol. Mag.* 1919, pp. 387–97; B. Smith, "Iron Ores: Hæmatites of West Cumberland, Lancashire, and the Lake District," *Mem. Geol. Surv., Special Reports on the Mineral Resources of Great Britain*, 1919, **8**, p. 182; T. F. Sibly, "The Hæmatites of the Forest of Dean and South Wales," *ibid.*, **10**, p. 93; T. C. Cantrill, *et alii*, "Sundry Unbedded Ores of Durham, East Cumberland, North Wales,

Derbyshire, etc.," *ibid.*, **9**, p. 87; M. Macgregor, *et alii*, "The Iron Ores of Scotland," 1920, **11**, p. 236). For the Cumberland ores Mr. Bernard Smith arrives at the conclusion that the iron has been derived by solution from the overlying New Red Sandstones, and has been deposited by metasomatic replacement in the underlying Carboniferous Limestone, the solutions travelling along faults and other fissures. A similar view is held by Prof. Sibly in regard to the iron ores in the Carboniferous Limestone of the Forest of Dean.

The iron ores of Scotland are classified as follows: 1. Bog ores of recent origin; 2. Hæmatite ores of different ages; 3. Carboniferous clayband ores; 4. Carboniferous blackband ores; 5. Jurassic ores. Of these the most important economically are the claybands and blackbands; but the ironstones of the Millstone Grit volcanic rocks in Ayrshire, although not likely to be of economic value, are perhaps of the greatest scientific interest, as they are probably of lateritic origin. This view is supported by their association with the bauxitic clays above-mentioned.

Recent developments in the great iron-ore deposits of Kiruna (Swedish Lappland) have led to a modification of Dr. Per Geijer's well-known views as to their origin (*Sver. Geol. Undersok*, 1918, Årsbok **12**, No. 5, p. 22). He has found contact metamorphic phenomena in the hanging wall of the ore bed that correspond exactly to those hitherto exclusively found on the footwall side. The ore body is now consequently believed to be intrusive rather than extrusive. Dr. Geijer thus returns to Stutzer's original view, but regards the ore body as a sill rather than a dyke. The enclosing porphyry bodies are still believed to be of extrusive origin.

A useful summary of ore genesis in the Archæan of India is presented by Dr. L. L. Fermor in his presidential address to the Geological Section of the sixth Indian Science Congress (*Proc. Asiatic Soc. Bengal*, 1919, **15**, pp. 170–95). He deals with the syngenetic sedimentary deposits exemplified by iron and manganese ores; syngenetic igneous deposits typified by chromite ores in serpentine; and epigenetic deposits, such as lodes of copper, lead, zinc, and gold, due to granitic intrusion into the Dharwar (Archæan) sediments.

According to W. R. Jones (*Bull. Inst. Min. and Metal*, March 1920, **186**, pp. 1–27) the tin and tungsten deposits of the world are to be classified as follows: 1. Segregation deposits; 2. Contact metamorphic deposits; 3. Pegmatoid deposits (in pegmatites, aplites, quartz-porphyries, and rhyolites); 4. Quartz vein deposits; 5. Replacement deposits. From his study it appears that cassiterite is a higher temperature mineral than wolframite; and deposits in which wolframite and cassiterite

occur in intimate association are formed in a lower temperature zone than the bulk of tin deposits free from wolfram.

J. Coggin Brown and A. M. Heron describe the distribution of ores of tin and tungsten in Burma (*Rec. Geol. Surv. India*, 1919, **50**, pt. 2, pp. 101–21).

In a paper entitled "Notes on Principles of Oil Accumulation" (*Journ. Geol.*, 1919, **27**, pp. 252–62) A. W. McCoy details important experiments showing that liquid hydrocarbons form in bituminous shales at ordinary temperatures under a pressure of 5,000–6,000 lbs. per square inch, applied in such a way that differential movement can take place. Since shales of this character are generally abundant in oil-fields, McCoy believes that oil is produced therein by pressure in areas of local differential movement. Further experiments show that the accumulation of the oil into commercial pools is accomplished by the agency of capillary water.

W. F. Jones presents evidence that oil-pools in the Pennsylvania and Oklahoma fields have only slight and indirect relationships with the tectonic structures of these rocks (*Econ. Geol.*, 1920, **15**, pp. 81–7). They appear to be aligned parallel to the extension of ancient shore lines, the sources of supply being lagoonal areas along such coast lines. The present positions and shapes of the pools closely conform to these underlying or overlying sources of supply, no wide lateral movement having taken place. Acceptance of this view has the important practical corollary that oil may be found if drilled for in areas hitherto considered tectonically unsuitable.

Sedimentary Rocks.—In an interesting paper on the origin of Cretaceous flint W. A. Richardson (*Geol. Mag.*, 1919, pp. 535–47) demonstrates a striking inverse relation between the amounts of silica disseminated through the chalk and that segregated as flint. He further shows that the rhythm of flint deposition has a considerable degree of resemblance to the Liesegang banding. A study of the distribution of flint strongly supports the hypothesis that the substance is due to rhythmic precipitation from solutions diffusing through the chalk at the time of uplift of that formation.

The chert of the Wreford and Foraker limestones of Kansas and Oklahoma is believed by Twenhofel (*Amer. Journ. Sci.*, 1919, **47**, pp. 407–29) to be due mainly to the replacement of unconsolidated limestone, the silica being derived from silica in solution which was mingled with the sediments, from silica in solution in the sea water, and from solutions of organic or other silica, or of silicates, deposited with the sediments.

Another paper by Twenhofel on Pre-Cambrian and Carboniferous algal deposits in certain American localities (*Amer. Journ. Sci.*, 1919, **48**, pp. 339–52) again emphasises the significant

part taken by calcareous algæ in limestone formation. Twenhofel introduces the term *cœnoplase* for the incrusting or laminated calcareous structures precipitated by algæ.

In a paper on the gypsum-breccia of Chellaston (Derbyshire), Bernard Smith shows that the gypsum was laid down in its present position as such, and has suffered no appreciable alteration or addition since its original deposition (*Quart. Journ. Geo. Soc.*, 1919, **74**, pt. 3, pp. 173–203). There is no evidence that the rock was ever anhydrous. When anhydrite is present, the evidence favours the view that it, too, is an original mineral, deposited in the same manner as, and in sequence with, gypsum. The brecciation of the Chellaston gypsum is due to the instability of the cover overlying isolated masses of soft gypsum which fill hollows in the Keuper Marl.

H. A. Baker has studied the petrography of the pebbles of quartzite and siliceous flint-conglomerate in the Oldhaven pebble-beds of the London Basin (*Geol. Mag.*, 1920, pp. 62–70), and has arrived at the conclusion that they are rolled fragments of sarsen and pudding-stone derived from the Woolwich and Reading beds.

A study of the pebbles of the Middle Bunter Sandstones of the Liverpool district has been made by T. A. Jones (*Proc. Liverpool Geol. Soc.*, 1920, **12**, pp. 201–308). The varied assemblage has considerable resemblance to that of the Midland beds of the same age. A distinctive group of tourmaliniferous rocks points to derivation of the material from a southern or southwestern source. This view is supported by the discovery of a pebble with Ordovician fossils within the district.

Metamorphism and Metamorphic Rocks.—Dr. A. Harker's Anniversary Address as President of the Geological Society (*Quart. Journ. Geol. Soc.*, 1919, **74**, pt. 1, pp. lxiii–lxxx) is an illuminating study of the process of metamorphism under the title, "Present Position and Outlook of the Study of Metamorphism in Rock Masses." Metamorphism, defined as the response in rocks to changed conditions of temperature and stress, is treated throughout as a problem in physical chemistry, which involves the application of the phase rule and the conception of equilibrium. Shearing stress is regarded as a factor of importance co-ordinate with temperature in governing mineralogical changes in solid rocks. It favours the production of a characteristic suite of minerals, sericite, chlorite, albite, the epidote-zoisite group, amphiboles, etc., as opposed to such minerals as anorthite, potash felspar, augite, olivine, and andalusite, which are products of thermal metamorphism, wherein the dynamic element has been subordinate or absent. These groups are designated as *stress* and *anti-stress* minerals respectively.

A paper by Dr. A. Holmes on the Pre-Cambrian and associated rocks of Mozambique is an important contribution to igneous and metamorphic petrology (*Quart. Journ. Geol. Soc.*, 1919, **74**, pt. 1, pp. 31–98). Certain gneisses of the region are believed to be due to concordant injection of granitic magma into a series of ancient sediments, of which the argillaceous facies became granitised with the production of biotite-gneisses, whilst the calcareous and dolomitic facies formed hornblendic and garnetiferous gneisses by interaction with the granite. A novel feature is the correlation of some of the gneisses and granites by means of the lead-uranium ratio in their zircons. The Pre-Cambrian age of a majority of the rocks in the basal complex of Mozambique is thus demonstrated.

Dr. A. L. du Toit presents an excellent study of the contact metamorphism of dolomitic limestone in Natal (*Quart. Journ. Geol. Soc.*, 1920, **75**, pt. 2, pp. 119–37). Granitic emanations have produced more or less regular zones in adjacent marbles, of which the innermost is rich in diopside and often in scapolite, with forsterite, phlogopite, chondrodite, and spinel farther out. Dedolomitisation is usually perfect; and in some areas beyond the silicate zone the dolomite has been deprived of the bulk of its magnesia, and has been changed into coarsely crystalline calcite. This change, termed *calcitisation*, has probably been due to the action of carbonated waters during the cooling of the plutonic complex.

F. L. Hess introduces the term *tactite* as a general name for the rocks formed by the contact metamorphism of limestone, dolomite, or other soluble rocks, into which foreign matter from the intruding magma has been introduced by the agency of hot solutions or gases (*Amer. Journ. Sci.*, 1919, **48**, pp. 377–8).

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MINERALOGY. By ALEXANDER SCOTT, M.A., D.Sc.

Synthetic Mineralogy.—Since this branch of the subject was last considered in these reports (SCIENCE PROGRESS, **14**, 38, 1919), several papers of considerable importance have appeared. The ternary system $CaO-MgO-SiO_2$ has been investigated by J. B. Ferguson and H. E. Merwin (*Amer. Journ. Sci.*, **48**, 81, 1919). Since two of the three binary systems of the component oxides are somewhat complicated, the ternary system, as might be expected, is more complex than any of those hitherto investigated in the Geophysical Laboratory. In addition to the simple oxides, the forms stable in contact with the melt include three calcium silicates, one of which is pseudowollastonite, the two magnesium silicates, clinoenstatite and forsterite, and four ternary compounds. Of the last, two correspond to the minerals diopside and monticellite, while the other two are compounds which have not been prepared hitherto, their composition being $5CaO . 2MgO . 6SiO_2$ and $2CaO . MgO . 2SiO$. The latter compound is regarded as åkermanite, as not only are the optical properties in agreement with those of the Italian example of this mineral, but the formula does not differ radically from that ($8CaO . 4MgO . 9SiO_2$) ascribed to the natural material by W. T. Schaller (*Bull. U.S. Geol. Surv.*, **610**, 109, 1916). Åkermanite was regarded by J. H. L. Vogt (*Mineralbildung in Schmelzmassen*, 96, 1892 ; *Die Silikatschmelzlösungen*, **1**, 49, 1903), who considered it an essential constituent of melilite, as calcium silicate, in which part of the lime was replaced by magnesia. This explanation was negatived by the failure of A. L. Day and E. S. Shepherd (*Amer. Journ. Sci.*, **22**, 280, 1906) to find such a compound in their investigation of the system $CaO-SiO_2$, and the problem of the constitution of åkermanite has hitherto remained unsolved.

Both monticellite and the compound $5CaO . 2MgO . 6SiO_2$ form solid solutions, but these, like the pure compounds, tend

to decompose below their melting temperatures. Diopside forms solid solutions with clinoenstatite, but not with forsterite or pseudowollastonite. The relationship of wollastonite and the ternary compounds is discussed in a further paper by the same authors (*Amer. Journ. Sci.*, **48**, 165, 1919). From a partial investigation of the solidus, it is found that wollastonite can take up 17 per cent. of diopside and over 60 per cent. of åkermanite in solid solution, while the high temperature form, pseudowollastonite, is capable of taking up only a trace and 23 per cent. respectively.

A detailed examination of the wollastonite solid solutions has been made and the inversion and decomposition temperatures determined. The appearance of such phases as pseudowollastonite at temperatures far below the inversion-point is considered to be an example of the operation of Ostwald's rule regarding unstable intermediate phases, which are so prominent in the case of the silica minerals.

The systems $CaO-MgO-Al_2O_3$ and $MgO-Al_2O_3-SiO_2$ have been re-examined by A. Meissner (*Zement*, **8**, 296, 308, 1919; *Journ. Chem. Soc.*, **118**, 39, 1920), but, according to the available abstracts, the results are not, so far as the latter system is concerned, in agreement with the results of G. A. Rankin and H. E. Merwin (*Amer. Journ. Sci.*, **45**, 301, 1918). The problem of the calcium ferrites and aluminates has undergone further investigation by E. D. Campbell (*Journ. Ind. Eng. Chem.*, **11**, 116, 1919). His results, which are in agreement with those of R. B. Sosman and H. E. Merwin (*Journ. Wash. Acad. Sci.*, **6**, 532, 1916), indicate the existence of only two calcium ferrites, $2CaO . Fe_2O_3$ and $CaO . Fe_2O_3$, there being no evidence of the formation of the other compounds suggested by E. Kohlmeyer and S. Hilpert (*Ber.*, **42**, 2581, 1909). Mixed crystals of calcium ferrite and aluminate have also been prepared, and it is found that these can take up less lime in solid solution than the pure aluminates (cf. E. S. Shepherd, G. A. Rankin, and F. E. Wright, *Zeit. anorg. Chem.*, **71**, 19, 1911; *Amer. Journ. Sci.*, **39**, 1, 1915). The application of recent synthetic work on the system $CaO - Al_2O_3 - SiO_2$ to the study of slags is discussed by B. Neumann (*Stahl u. Eisen*, **38**, 953, 1918).

Although several hydrated ferric oxides are usually described in books on mineralogy, the nature of these minerals has long been in doubt. Despite the opinion of J. M. van Bemmelen (*Zeit. anorg. Chem.*, **20**, 185, 1899) that the amorphous "hydrates" were merely colloidal mixtures, these have been regarded by O. Ruff (*Ber.*, **34**, 3417, 1901) and others as definite compounds. The minerals have been subjected to an elaborate examination by E. Posnjak and H. E. Merwin (*Amer. Journ. Sci.*, **47**, 311, 1919), and they find that no series of hydrates

exists, and that the only definite compound is the monohydrate, $Fe_2O_3 . H_2O$. All the others are amorphous colloidal mixtures, but the monohydrate exists in two crystalline forms, gœthite and lepidocrocite, both of which occur in nature. No crystalline hydrate, however, has been prepared synthetically. The fibrous mineral, turgite, whose composition is approximately $2Fe_2O_3 . H_2O$, is regarded as a solid solution of gœthite and hæmatite, with possibly some absorbed water. L. J. Spencer (*Min. Mag.*, **18**, 339, 1919), on the other hand, concludes, on the basis of its optical properties and dehydration phenomena, that turgite should be regarded as a definite mineral species.

The conclusions of R. B. Sosman and J. C. Hostetter (*Journ. Amer. Chem. Soc.*, **38**, 807, 1188, 1916), regarding the existence of a series of solid solutions of hæmatite and magnetite, are called in question by T. M. Broderick (*Econ. Geol.*, **14**, 353, 1919). The latter admits the possibility of solid solutions, but holds that many of the natural intermediate forms show a lack of homogeneity when examined microscopically by metallographic methods.

An interesting note by J. B. Ferguson (*Journ. Wash. Acad. Sci.*, **9**, 539, 1919) reveals the fact that ferrous oxide in lavas is not oxidised to any great extent by steam at high temperatures, and hence a high content of ferric oxide in a magma is not incompatible with the presence of much steam. A theoretical paper on the relations between the lower oxides of iron and oxygen, by A. Smits and J. M. Bijvoet (*Proc. Akad. Amsterdam*, **21**, 386, 1919), may also be mentioned.

The ortho- and meta-silicates of the alkaline earth and related metals have been prepared by F. M. Jaeger and H. S. van Klooster (*Sprechsaal*, **52**, 256, 1919), and some of the optical properties determined. A linear relation between the melting-points of the metasilicates and the atomic weights of the metals is found to hold in certain groups. In a further paper, C. N. Fenner (*Journ. Soc. Glass Tech.*, **3**, 116, 1919) reiterates his conclusions on the stability of the silica minerals, while a revised theoretical explanation of the same system is given by A. Smits and K. Endell (*Zeit. anorg. Chem.*, **106**, 143, 1919).

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

THE group of the Actinomyces has been the subject of several important papers, notably those of Drechsler (*Bot. Gaz.* 1918), Waksman (*Journ. Bacteriology*, 1919, and *Soil Science*, 1919), and Waksman and Curtis (*Soil Science*, 1918). For a long time placed with the Bacteria, the group differs in the possession of true branching, which is profuse, in the fungal type of

the Conidia, and in the terminal growth. The septa are sparse and irregular in their distribution, whilst owing to the narrowness of the hyphæ (0·5–1·2 μ) little is known as to the cytology. Metachromatic granules are present in the vegetative parts, and in the spores there are present granules of uniform size and with the staining properties of nuclei.

Actinomycetes occur abundantly in many soils, particularly those rich in undecomposed organic matter, in consequence of which they tend to attain a maximum frequency in the autumn. Of the 25 soils from different localities studied by Waksman and Curtis the average number per gram of soil was 870,500 as compared with 4,245,000 bacteria. Like the latter, there is a decrease with increasing depth, but in relation to the bacteria a proportional increase. Waksman (1919) in an extensive paper deals with the features and behaviour of a number of species on culture media. This author finds that for most species the minimum temperature is below 18–20°, and the maximum above 40°. Whilst they will not grow under strictly anaerobic conditions, several are semi-anaerobic. Most are actively proteolytic, producing ammonia and amino-acids as a result of their metabolism. Starch affords one of the best sources of carbon, though most species can also assimilate dextrose, maltose, lactose, mannite, glycerine and inulin. All liquefy gelatin and nearly all reduce nitrates. A few species are capable of utilising cellulose. The paper is accompanied by a key for the identification of species by means of their biochemical reactions.

Genetics, etc.—According to Dorsey (*Genetics*, September 1919), the American varieties of Plum are self-sterile, but though aborted grains were found in all cases, this is seldom complete or the cause of sterility. There are three successive periods at which the pistils are shed, viz. (*a*) immediately after flowering, (*b*) two to four weeks after blooming when fertilisation has not occurred, (*c*) four to six weeks after blooming when fertilisation has been effected but the embryo development has been checked. The cause of sterility is attributed to the slow growth of the pollen tubes conditioned by genetic constitution.

A race of *Fagopyrum tataricum* has been studied by Zinn (*Genetics*, November) in which the number of carpels varied from 3 to 25 with 4 carpels as the mode. The perigones also exhibit a varying number of segments (5–18) with 5 as the mode, and there was positive correlation between the number of segments and the number of carpels. Selection, whether of normal or abnormal seeds, appeared to have no effect on the type of variability. High temperature and humidity favoured the development of abnormal flowers, which also were most

frequent on the basal regions of the plant and after the second week of the flowering period.

The variation in the different whorls of the flower has been studied in further members of the Ranunculaceae by Salisbury (*Ann. Bot.* January). The two species investigated, viz. *Anemone apennina* and *Clematis vitalba,* show the periodic type of meristic variation " curve " even more strikingly than some of the species previously studied. In the former there is an obvious correlation between the number of perianth segments and the number of stamens. " Branched " carpels were observed in several examples of both species.

M. Ikeno (*Rev. Gen. de Bot.*) has cultivated a race of *Plantago major* termed *contracta* which is recessive to the so-called type. When these recessives were self-fertilised, however, the offspring showed a certain number (0·5–10 per cent.) of reversions to the type. On self-fertilising the reversions they proved to be heterozygous and segregated in the normal manner into var. *typica* and var. *contracta.*

Yampolski (*Amer. Jour. Bot.*, December) deals with the sex intergrades of *Mercurialis annua.* At the one extreme are purely female plants, at the other purely male, whilst in between are a series of which, in the author's cultures, some were prevailingly male with from 1 to 47 female flowers, other prevailingly female with from 1 to 32 male flowers. The male flowers bear normally 8 stamens, and the female a bi- or tri-carpellary ovary, but hermaphrodite flowers also occur with only 1 to 6 stamens in which both types of sporophyll appear to be functional.

Of 50 plants raised in F_1 from a prevailingly female parent, all showed the same sex-tendency, and this was maintained in F_2 and F_3. Similarly the offspring of prevailingly male plants were prevailingly male, whilst crosses between the two strains yielded a sex ratio of approximately 1 : 1. The results indicate that neither the male nor female gamete can be regarded as heterozygous to sex.

In a further paper (*Amer. Jour. Bot.* January) dealing with the general question the same author suggests that there may be graded potencies for both types of gamete.

The rôle of the endosperm has been the subject of numerous experimental investigations, and those of Van Tieghem and Brown and Morris, in particular, have led to the view that the endosperm, though an aid, is not essential for germination, and that, in grasses at all events, it is a purely passive storehouse of food.

Andronescu (*Amer. Jour. Bot.*), experimenting upon *Zea mais,* confirms the conclusions of earlier workers, but the chief interest of his results is due to the fact that the plants were

grown on to maturity. Those developed from embryos without endosperms tend to be shorter with fewer and stouter internodes, whilst their general development is retarded.

Taxonomy.—In the *Journal of Botany* (January.—March) G. A. Boulenger describes a new species of *Rosa* from Dorset, W. J. Hodgetts a new Desmid, *Roya anglia*, and J. A. Wheldon a new Lichen, *Bilimbia cambrica*, from Snowdon.

In the same Journal Colonel Godfrey figures and describes the essential floral distinctions between *Epipactis violacea*, *E. latifolia* and *E. viridiflora* v. *leptochila*.

Additional species are described belonging to the following genera: *Aspilia*, *Crassocephalum*, *Metalasia*, *Stoebe*, and *Vernonia* (Compositae), *Hygrophila*, *Justicia*, *Rhinacanthus* (Acanthaceæ), *Vaupelia* (Borraginaceæ), *Solanum* (Solanaceae), *Phyllanthus* (Euphorbiaceæ), *Crotalaria* (Leguminoseæ), and *Dewevrella* (Apocynaceæ).

Economic.—Read and Smith (*Ins. Sci. and Industry, Australia Bull.* **14**) have investigated the properties of the fibre of *Posidonia australis*. The fibres have a low tensile strength and flexibility, but their commercial value can be greatly increased by treatment with cold, dilute, mineral acids. The ultimate fibres are about 1 mm. in length and consist of lignocellulose. They exhibit great resistance to dilute alkalis, but are readily affected by the halogens and exhibit a great affinity for dyes. Apart from the high value as an insulating medium, their use is advocated for coarse fabrics where resistance to strain is not important but where resistance to chemical and bacterial action is a necessity.

PLANT PHYSIOLOGY. By CYRIL WEST, D.Sc. (Lond.), A.R.C.Sc., Botany School, Cambridge (Plant Physiology Committee).

Dormancy, or Delayed Germination of Seeds.—Although the subject of dormancy, or delayed germination of seeds, is of great importance from the economic point of view, and consequently has attracted the attention of many workers in the past, it is only of recent years that much attention has been directed to its physiological aspect. Crocker ("Mechanics of Dormancy in Seeds," *Amer. Journ. of Bot.*, iii, 1916, p. 99) has given an excellent summary of our knowledge of the physiology of dormancy in seeds up to, and including, the year 1915, and has pointed out that, broadly speaking, dormancy, or delayed germination of seeds, can be attributed to one or more of the following causes: (1) incomplete development of the embryo; (2) impermeability of the testa to water: (3) mechanical restraint offered to the expansion of the embryo and other seed-contents by the seed-coats; (4) inhibition or retardation of the passage of gases to or from the embryo by the testa, resulting in an accumulation of carbon dioxide within the

tissues of the embryo or an insufficient supply of oxygen for germination; (5) the necessity of the embryo itself to undergo certain after-ripening processes before germination and growth under ordinary germinating conditions are possible; or, lastly (6) the induction by various means of a condition of dormancy in seeds previously capable of immediate germination. To this special form of dormancy Crocker has applied the term "secondary dormancy." This author has also emphasised the fact that problems in dormancy lend themselves well to mechanistic attack.

According to Rose ("After-ripening and Germination of Seeds of *Tilia*, *Sambucus*, and *Rubus*," *Bot. Gaz.*, lxvii, 1919, p. 281) freshly harvested seeds of *Tilia americana* with a moisture-content of 10 per cent. or less, or seeds kept in warm storage for several months fail to germinate when placed on a moist substratum at ordinary room temperatures. He has shown by numerous experiments that the germination of the seeds of this plant can be brought about by a period of after-ripening in moist storage at 0°—2° C. followed by a sojourn of 2–3 weeks at 10°–12° C. until germination is well under way. To obtain vigorous growth of the young seedlings a still higher temperature is essential. During the process of after-ripening the hydrogen-ion concentration of the seeds increases, as do also the oxidase and catalase activities.

Freshly harvested seeds of *Sambucus canadensis* similarly fail to germinate when sown at room temperature; it was found, however, that such seeds, after having been kept in moist soil out of doors during the winter, showed a high percentage of germination; but the author was unable to determine whether this result was due to the low temperatures obtaining during the winter months, as previously suggested by Kinzel ("Frost und Licht als beeinflüssende Kräfte bei der Samenkeimung," Stuttgart, 1913), to certain constituents of the soil, or to a combination of these or other factors. In the case of *Rubus Idæus* Rose found that the dormancy of the seeds was probably due to the high breaking strength of the endocarp. Germination of the seeds of this plant was unaffected by light or by darkness.

Kidd and West ("The Controlling Influence of Carbon Dioxide. IV. On the Production of Secondary Dormancy in Seeds of *Brassica alba*, following Treatment with Carbon Dioxide, and the Relation of this Phenomenon to the Question of Stimuli in Growth Processes," *Ann. of Bot.*, xxxi, 1917, p. 457) have shown that germination of White Mustard seeds, sown in the presence of carbon-dioxide under certain conditions, can be completely inhibited, and that this inhibition of germination is often maintained for long periods (twelve

months in their experiments, during which period the seeds lay on moist silica sand) after the removal of the seeds to air. This condition of secondary dormancy in White Mustard seeds can generally be terminated by redrying the seed or by removing the seed-coats without drying. No evidence was found in support of the hypothesis that changes in the seed-coats had occurred during the period of primary inhibition; on the other hand, it was shown that if the testa were removed with extreme care the naked embryos remained dormant, and it was suggested that a more stable condition of the tissues of the embryo had become established during the period of primary inhibition under the influence of carbon-dioxide. Embryos in this stable condition do not respond to the ordinary environmental factors under which germination of normal embryos takes place. A definite stimulus, chemical or mechanical, appears to be necessary to initiate growth (by cell-division) of the dormant embryo.

The recent work of Crocker and Harrison ("Catalase and Oxidase Content of Seeds in Relation to their Dormancy, Age, Vitality and Respiration," *Journ. Agric. Research*, xv, 1918, p. 137) marks an important advance in our knowledge of the physiology of dormant seeds, especially with reference to changes in respiration and in catalase and oxidase activity. Freshly harvested seeds of Johnson grass, if kept in a germinator at 20° C., will remain dormant for a year or more. That this condition of dormancy is imposed by the structures enclosing the embryo and that it is not due to a dormant condition of the embryo itself is proved by the fact that their removal leads to prompt germination. A marked reduction in catalase activity takes place while the seeds are kept in the germinator at 20° C., a reduction in catalase activity of 50 per cent. occurring during the first month under these conditions as compared with the catalase activity of similar seeds under ordinary dry-storage conditions. This fall in catalase activity is accompanied by a fall in respiratory intensity, whereas a gradual rise occurs in the respiratory intensity of the seed when dry-stored. If a gradual fall in respiratory activity is a phenomenon of general occurrence in imbibed dormant seeds, and if it is assumed that the death of such seeds depends upon the utilisation and destruction of their food-reserves by respiration, then this result will have an important bearing on the longevity of seeds under natural conditions. In this connection the conclusion of Brenchley ("Buried Weed Seeds," *Journ. Agric. Science*, ix, 1918, p. 1) that the seeds of certain plants may survive burial in the soil for at least fifty-eight years is of interest.

A number of German plant physiologists have directed their attention to the problems associated with the germina-

tion of " light-sensitive " seeds. It has long been known that the seeds of some plants do not germinate in light, whilst others do not germinate in the absence of light. For instance, Kinzel (" Teleologie der Wirkungen von Frost, Dunkelkeit und Licht auf die Keimung der Samen," *Ber. d. deutsch bot. Gesellsch.*, xxxv, 1917, p. 581) records his experience with seeds of *Aquilegia atrata* which were kept on moist blotting-paper in the dark for ten years (1907–1917). During the first twelve months only 10 per cent. of the seeds germinated ; the remaining 90 per cent. lay dormant in a perfectly healthy condition for the whole period of ten years until exposed to light, when about 60 per cent. of them germinated in fifty days. Experiments conducted by E. Lehmann (" Ueber die minimale Belichtungszeit, welche die Keimung der Samen von *Lythrum Salicaria* auslöst." *Ber. d. deutsch bot. Gesellsch.*, xxxvi, 1918, p. 157) showed that fully imbibed seeds of *Lythrum Salicaria*, only a negligible quantity of which had germinated in darkness, gave a germination of 50 per cent. in 24 hours after a very brief ($\frac{1}{10}$ second) exposure to a light intensity of 730 candle-power at a distance of one metre.

It has been shown that many organic and inorganic substances have the power of partially or completely counteracting the retarding effect of light or of darkness upon the germination of light-sensitive seeds. Kühn (" Dunkelkeimer und Substrat," *Ber. d. deutsch bot. Gesellsch.*, xxxiv, 1916, p. 369), working with seeds of a number of plants, studied the inter-relations of various mineral acids and of light and darkness as factors influencing germination. With seeds of *Phacelia tanacetifolia* on filter-paper moistened with distilled water he obtained 80 per cent. germinations in the dark, but only 18 per cent. when exposed to light. By substituting weak solutions of various mineral acids (0·1 M) he was able to raise the number of germinations in light to 60–80 per cent. In the dark, however, the acids appeared to have no effect upon the germination of these seeds. Results of a similar nature were obtained with seeds of *Solanum Lycopersicum* and *Amaranthus atropurpureus*.

Gassner (" Untersuchungen über die Wirkung des Lichtes und des Temperaturwechsels auf die Keimung von *Chloris ciliata*," *Jahrb. d. Hamburger Wiss. Anstalten*, xxix, 1911, Beih. 3 : " Ueber die keimungsauslösende Wirkung der Stickstoffsalze auf lichtempfindliche Samen," *Jahrb. f. Wiss. Bot.*, lv, 1915, p. 259 ; and " Beiträge zur Frage der Lichtkeimung," *Zeitschr. f. Bot.*, vii, 1915, p. 609), who worked with seeds of *Chloris ciliata*, found that at temperatures above 20° C. light exercised a favourable effect upon the germination of these seeds, whereas at lower temperatures, *i.e.* below 20° C., it had

the reverse effect. Weak solutions of potassium nitrate, on the other hand, increased the percentage of germination at all the temperatures tested. Thus it would appear that at the lower temperatures light and nitrogenous substances do not act in a similar way upon the germination of *Chloris* seeds. Gassner also found that nitrates did not penetrate the seeds, but were held back by a semipermeable layer of the testa. Hence we must look for the direct effect of such substances upon the germination of these seeds in changes produced in the testa, and not in the embryo.

The results of germination experiments published last year by E. Lehmann ("Ueber die keimfördernde Wirkung von Nitrat auf lichtgehemmte Samen von *Veronica Tournefortii*," *Zeitschr. f. Bot.*, xi, 1919, p. 161) show how complex the problems of light-sensitive seeds may be. Under ordinary conditions the seeds of *Veronica Tournefortii* do not germinate in light, and at higher temperatures, *e.g.* 30° C., do not germinate in the light or in the dark. Solutions of potassium nitrate act favourably upon the germination of these seeds either in light or in darkness, but their effect is much more striking in light.

Treatment with potassium nitrate in light resulted in an increase in the number of germinations of about 80–90 per cent. as compared with controls in distilled water. It was also found that the higher the intensity of light the greater its retarding influence upon the germination, and that potassium nitrate was unable to counteract the retarding effect of a light intensity of 440 candle-power at a distance of one metre. At 30° C. the nitrate had no effect upon germination either in light or in darkness.

Seeds of *Viscum album* are generally supposed to require a definite resting period of about five months before they are capable of germination. This is disproved by Heinricher ("Ueber den Mangel einer durch innere Bedingungen bewirkten Ruheperiode bei den Samen der Mistel (*Viscum album*, L.), *Sitgungsb. d. Kais. Akad. d. Wiss. in Wien, Math.-naturw.* Kl., cxxv, Abt. I, 1916, p. 26), who states that, under suitable environmental conditions, *e.g.* high relative humidity and light intensity of 1,600 candle-power, mistletoe seeds can be forced to germinate at any time during the winter.

In the course of an ecological investigation on the germination of the seeds of various forest trees, Boerker (*Univ. Studies, Nebraska*, xiv, 1916, pp. 1–90) studied incidentally the effects of light and of various other environmental factors. Although he came to the conclusion that light plays a distinct part in the problem of seed germination under natural conditions, he was of the opinion that it acted indirectly by affecting soil-moisture, evaporation, etc.

For other recent work on the effect of light upon germination the reader is referred to: Lehmann, E., "Lichtkeimungsfragen," *Zeitschr. f. Bot.*, vii, 1915, p. 560: Kling, F. ("Beitrag zur Prüfung der Gräserkeimung," *Journ. f. Landw.*, xxxiii, 1916, p. 285); Honing, J. A. ("De invloed van het licht op het keimen van de zaden van verschellende variëteiten van *Nicotiana tabacum*," *Bull. Deli Proefstat*, No. 7, 1916, pp. 1–14).

ZOOLOGY. By Prof. Chas. H. O'Donoghue, D.Sc., F.Z.S., University of Manitoba, Winnipeg, Canada.

Protozoa.—Hegner, in "The Effects of Environmental Factors upon the Heritable Characters of *Arcella dentata* and *A. polypora*" (*Journ. Exp. Zool.*, vol. xxxix, Nov. 1919), reared the species mentioned under various unusual conditions, to see what effect these would have upon certain characters in future generations. By submitting them to experimental environmental surroundings, such as water containing a solution of sodium silicate or ethyl alcohol, various temperatures and different degrees of starvation, it was found that the offspring showed a number of distinct variations in the diameter of the shell, the number, size, and shape of the spines. To eliminate one source of error, the initial specimens were chosen from clones of known size and spine character. It was found that, when forms modified by this treatment were chosen as parents and were restored to a normal environment, their offspring resumed normal characters.

Other papers include:

Calkins, "*Uroleptis mobilis* Engelm: III, Renewal of Vitality through Conjugation" (*ibid.*, Oct. 1919); Dawson, "An Experimental Study of an Amicronucleate *Oxytrichia*: I, Study of the Normal Animal, with an Account of Cannibalism" (*ibid.*, Nov. 1919); and Collett, "The Toxity of Acids to Ciliate Infusoria" (*ibid.*, Nov. 1919).

Invertebrata.—Hargitt, in "Germ Cells of Coelenterates: VI, General Considerations, Discussion, Conclusions" (*Journ. Morph.*, vol. xxxiii, Dec. 1919), has brought to an end a series of papers dealing with the subject indicated at the beginning of the title. The author claims that in Coelenterates the germ cells are first differentiated with the approach of sexual maturity, and are not segregated at an early stage of ontogeny. In the Hydrozoa the germ cells may arise from either or both layers of the body even in the same individual, and in certain cases they have been observed to come directly from differentiated body cells. In the Scyphozoa the germ cells are probably entodermal in origin. It is further claimed that, in view of the evidence obtained from a study of the germ cells, regeneration, budding, etc., the theory of the continuity of the germ cells cannot be regarded as applying to the Coelenterata.

Other papers include:

Hausman, "The Orange-striped Anemone (*Sagartia luciæ*), an Ecological Study" (*Biol. Bull.*, vol. xxxvii, Dec. 1919); and Bourne, "Observations on *Arachnactis albida*, M. Sars" (*Quart. Journ. Micro. Sci.*, vol. lxiv, Oct. 1919).

A paper "On a New Type of Nephridia found in Indian Earthworms of the genus *Pheretina*" is published by Bahl (*ibid.*, vol. lxiv, Oct. 1919). The Oligochætes and Hirudinea in general possess what is designated a meganephric type of nephridium, *i.e.* a few large segmental nephridia opening separately to the exterior. Certain members of both groups also have plectonephric varieties, *i.e.* numerous small nephridia opening to ducts which generally lead to the exterior. The new type, named "enteronephric," found in Pheretinids, consists of numerous nephridia opening into systems of ducts, which in turn lead into the gut. Small integumentary nephridia also occur, each opening on to the surface of the body. It is pointed out in a footnote that such a type of nephridial system raises an interesting question as to the origin of these structures, since there is a possibility of their not being derivatives of the ectoderm.

Other papers include:

Hyman, "Physiological Studies on Planaria: III, Oxygen Consumption in Relation to Age (Size) Differences" (*Biol. Bull.*, vol. xxxvii, Dec. 1919); Goodrich, "The Pseudopodia of the Leucocytes of Invertebrates" (*Quart. Journ. Micro. Sci.*, vol. lxiv, Oct. 1919); Garrey, "The Nature of the Fertilisation Membrane of Asterias and Arbacia Eggs" (*Biol. Bull.*, vol. xxxvii, Nov. 1919); Richards and Donnel, "Notes on the Effect of X-radiation on the Development of Cumingia Eggs" (*ibid.*, Oct. 1919); Berry, "Light Production in Cephalopods: I, An Introductory Survey" (*ibid.*, Dec. 1919); and Barrows, "The Occurrence of a Rock-boring Isopod along the Shore of San Francisco Bay, California" (*Univ. Cal. Pub. Zool.*, vol. xix, Dec. 1919).

Arey and Crozier, in "The Sensory Responses of *Chiton*" (*Journ. Exp. Zool.*, vol. xxix, Oct. 1919), and Crozier and Arey in "Sensory Reactions of *Chromodoris zebra*" (*ibid.*), record the results of a series of investigations on the reactions of these two molluscs. In both cases quite well differentiated receptive mechanisms were discovered that are able to respond to tactile, chemical, and photic stimuli. In the former case it is shown that certain reactions vary with age. Both species show a complex relationship with the varying conditions of the environment. In *Chromodoris* the animal through its eyes is positively phototrophic, and in strong currents the rhinophores act as directive organs for negative rheotropism.

Other papers include:

Minnich, "The Photic Reactions of the Honey-Bee" (*Journ. Exper. Zool.*, vol. xxix, Nov. 1919); Seyster, "Eye-facet Number as Influenced by Temperature in the Bar-eyed Mutant of *Drosophila melanogaster* (Ampelophora)"

(*Biol. Bull.*, vol. xxxvii, Oct. 1919); Shinji, "Embryology of Coccids, with Especial Reference to the Formation of the Ovary, Origin and Differentiation of the Germ Cells, Germ Layers, Rudiments of the Midgut, and the Intracellular Symbiotic Organisms" (*Journ. Morph.*, vol. xxxiii, Dec. 1919); Strong, "Roughoid, a Mutant Located to the Left of Sepia, in the Third Chromosome of *Drosophila melanogaster*" (*Biol. Bull.*, vol. xxxvii, Dec. 1919); and Riley, "Some Habitat Responses of the Large Water Strider, *Gerris remigis*, Say" (*Amer. Nat.*, vol. liii, Dec. 1919).

Vertebrata.—Tales of the wonderful power of *Echeneis*, or Remora, the sucking-fish, have crept into zoological literature from the earliest times, and in "On the Use of the Sucking-fish for Catching Fish and Turtles: Studies in *Echeneis*, or Remora, III" (*Amer. Nat.*, vol. liii, Dec. 1919), Gudger brings to a conclusion a series of inquiries on this subject. The previous parts of this work dealt critically with all the known references to the subject in reputable writings, and the sub-title, "Are these Accounts Credible?" gives a fair indication of the scope of the present instalment. The matter is gone into from the point of view of the actual performances of the fish, and the author's final summary is, "To the present writer, all the evidence at hand sustains and confirms the stories of the living fish-hook from the time of Columbus to the present day"; and it is good to find one fable of youthful days has some scientific backing.

Other papers include:

Hubbs, "A Comparative Study of the Bones forming the Opercular Series of Fishes" (*Journ. Morph.*, vol. xxxiii, Dec. 1919); Bellamy, "Differential Susceptibility as a Basis for Modification and Control of Development in the Frog" (*Biol. Bull.*, vol. xxxvii, Nov. 1919); Lillie, "The Early Histogenesis of the Blood in *Bufo halophilus*, Baird and Girard" (*Am. Journ. Anat.*, vol. xxvi, Nov. 1919); Smith, "The Individuality of the Germ-nuclei during the Cleavage of the Egg of *Cryptobranchus alleghenienesis*" (*Biol. Bull.*, vol. xxxvii, Oct. 1919); Parmenter, "Chromosome Number and Pairs in the Somatic Mitoses of *Amblystoma tigrinum*" (*Journ. Morph.*, vol. xxxiii, Dec. 1919); Rogers, "Experimental Studies on the Brain Stem: III, The Effects on Reflex Activities of Wide Variations in Body Temperature caused by Lesions of the Thalamus" (*Journ. Comp. Neur.*, vol. xxxi, Oct. 1919); Pohlman, "Concerning the Causal Factor in the Hatching of the Chick, with Particular Reference to the Musculus Complexus" (*Anat. Rec.*, vol. xvii, Oct. 1919); Hoshino, "A Study of Brains and Spinal Cords in a Family of Ataxic Pigeons" (*Journ. Comp. Neur.*, vol. xxxi, Dec. 1919); and Goodrich, "Note on the Reptilian Heart" (*Journ. Anat.*, vol. liii, July 1919).

In "The Morphology of the Pulmonary Artery of the Mammalia" (*Anat. Rec.*, vol. xvii, Dec. 1919) Huntington has thrown new light on this subject. In studying the development of the pulmonary artery in the cat, he has shown that it does not arise as an outgrowth from the ventral region of the sixth or pulmonary arch, as is usually stated in textbooks. On the other hand, it takes origin from a plexus of vessels derived from post-branchial derivatives of the dorsal aorta in the lung

region. These anastomose in the lung rudiment, and give rise to a longitudinal vessel, which is the origin of the pulmonary artery. This finding is considered in relation to its phyletic significance, and also the general question of the interpretation of this portion of the vascular system in a generalised primitive Vertebrate circulation. Lastly, its bearing is shown in the case of various anomalies and mutations of the pulmonary artery in man and lower Vertebrates. To his numerous studies of the albino rat Donaldson, with the assistance of Conrow, has now added another, entitled "Quantitative Studies in the Growth of the Skeleton of the Albino Rat" (*Amer. Journ. Anat.*, vol. xxvi, Nov. 1919). In all a series of 106 skeletons of rats from birth to 500 days old were investigated. The bones were weighed wet and after drying, thus allowing a determination of their water content. It was found that "the proportions of the mature skeleton tend to become formed at about weaning-time, and the weights and lengths of some long bones early attain a fixed relation to the weight of the entire body or the skeleton, and that these values can be recovered by the use of a single bone." The change in the shape of the long bones during growth is also noted, and certain comparisons made with the data in man. Rasmussen has recorded the results of a study on "The Mitochondria in Nerve Cells during Hibernation and Inanition in the Woodchuck (*Marmota monax*)" (*Journ. Comp. Neur.*, vol. xxxi, Oct. 1919). It might be expected that such a marked physiological change, both in activity and in condition, brought about by hibernation, would probably effect profoundly the cells of the entire animal. Counts and estimations gave the number of mitochondria per cubic millimetre of cytoplasm as varying between 186 and 354, with a remarkable constancy in the cells of a given nucleus. There was no noticeable difference in the number, size, shape, or staining reactions of animals examined before, during, and after hibernation. Furthermore, several weeks of inanition following hibernation also proved to have no effect.

Other papers include:

Cooper, "The Hypophysis Cerebri of the California Ground Squirrel, *Citellus beechii* (Richardson)" (*Amer. Journ. Anat*, vol. xxvi, Nov. 1919); Dixon, "Notes on the Natural History of the Bushy-tailed Wood Rats of California" (*Univ. Cal. Pub. Zool*, vol. xxi, Dec. 1919); Huffmire, "A Case of Persistence of the Left Superior Vena Cava in an Aged Adult" (*Anat. Rec.*, vol. xvii, Nov. 1919); Isaacs, "The Structure and Mechanics of Developing Connective Tissue" (*ibid.*, Dec 1919); Ivy, "Experimental Studies on the Brain Stem: II, A Comparative Study of the Relation of the Cerebral Cortex to Vestibular Nystagmus" (*Journ. Comp. Neur.*, vol. xxxi, Oct. 1919); Koch and Riddle, "Further Studies on the Chemical Composition of Normal and Ataxic Pigeon Brains" (*ibid.*, Dec. 1919); Komine, "Metabolic Activity of the Nervous System: IV, The Content of Non-protein Nitrogen in the Brain of Rats kept in a State of Emotional and Physical Excitement for

Several Hours " (*ibid.*) ; " The Innervation of the Gonads in the Dog " and " Experimental Degeneration in the Testis of the Dog," both by Kuntz in *Anat. Rec*, vol xvii, Dec. 1919 ; Reagan, " On the Later Development of the Azygos Veins of Swine " (*ibid.*, Nov. 1919) ; Smith, " Description of a Case of Persistent Left Duct of Cuvier in Man " (*ibid*) ; Stockard and Papanicolau, " The Vaginal Closure Membrane, Copulation, and the Vaginal Plug in the Guinea-pig, with Further Considerations of the Œstrus Rhythm " (*Biol. Bull.*, vol. xxxvii, Oct. 1919) ; Takenouchi, " Studies on the Reputed Endocrine Function of the Thymus Gland (Albino Rat) " (*Journ. Exp. Zool.*, vol. xxix, Oct. 1919) ; Terry. " The Relation of the Facial Nerve and Otic Capsule" (*Anat. Rec.*, vol. xvii, Dec. 1919) ; and Whiting, " Inheritance of White Spotting and other Colour Characters in Cats."

General.—It is far from generally recognised that, in spite of having performed no direct experiments upon the subject, Charles Darwin came very near to the Mendelian explanation of certain phenomena in heredity. Roberts, in " Darwin's Contribution to the Knowledge of Hybridisation " (*Amer. Nat.*, vol. liii, Dec. 1919), has gone into this widely overlooked point, and by means of a carefully selected series of extracts shown Darwin's attitude on the subject in a way that will save the reader the necessity of going through his various works. In one case, for example, it is pointed out, in reference to one quotation, that " the above paragraph comes more nearly being a statement of the true nature of the hybrid or hetereozygote condition, as Mendel's analysis has revealed it, than any other account hitherto published."

Other papers include :

Baldwin, " The Artificial Production of Monsters Conforming to a Definite Type by Means of X-rays " (*Anat. Rec.*, vol. xvii, Nov. 1919) ; Hooker, " Behaviour and Assimilation " (*Amer. Nat.*, vol. liii, Dec. 1919) ; and Kappers, " The Logetic Character of Growth."

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S., Zoological Laboratory, Cambridge,

The *Journal of the Royal Anthropological Institute* for the first six months of 1919 (vol. xlix) is an exceptionally interesting number. The first article is the Presidential Address delivered by Sir Hercules Read, and is entitled " Anthropology and War." This subject is one which has been discussed at some length from time to time in Science Progress, and the present reviewer contributed one article on the subject (July 1915). It will be remembered that during the war various writers declared that, from the eugenic point of view, war was certainly a disaster to the race—wars, that is, of the modern type, in which the ranks of the fit are decimated and the unfit remain safely at home. This somewhat facile conclusion was disputed by the Editor of this journal and others ; and one of the points which I endeavoured to make in the

above-mentioned article was that on the very principle brought forward by the pessimistic writers, it was impossible to be sure of the racial effect of a war until we knew the quality of those who return from the war as compared with those who had gone to the conflict. It was necessary to reckon with the possibility that the quality of those who returned from a war was so much higher than that of the whole army which had originally become engaged, that the quality of the survivors, as a whole (namely, the returned soldiers and those who had never joined the army) was actually superior to the quality of the male population as a whole before the war. This possibility appeared to me to be of some interest, and in his general discussion of the matter, Sir Hercules Read mentions some statistics which bear upon this point and of the existence of which I was unaware in 1915. The figures in question relate to the effects of the Franco-German War. The existence of conscription in France made it possible to judge of the character of the boy-babies born during and after the Franco-German War—to judge them, that is, when they had grown to military age. And it was found that whilst the generation born during the war—the youths who appeared as conscripts in 1890—were, indeed, below the average of stature and physique, those born immediately after the war were much above the average, which seemed to prove that they were the sons of exceptionally fine sires. Many factors would, of course, have to be considered; for instance, the inferior quality of the men born during the war might, and in some cases certainly would, be caused by the privations existing in parts of France during the conflict; but in view of the discussion which has taken place the statistics are very interesting and deserve to be widely known.

In connection with this subject, another paper may be mentioned, which appears in that excellent journal the *American Naturalist* (vol. liv. No. 630, Jan.–Feb. 1920). This paper is by Prof. Raymond Pearl and is entitled "Certain Evolutionary Aspects of Human Mortality Rates"; and it will be found as suggestive as papers in the *American Naturalist* usually are.

Mr. J. Reid Moir's recent work has been largely directed towards a demonstration of the possible evolution of the rostro-carinate implements into the ordinary paleolithic implements of the so-called river-drift type. Another of his papers bearing upon this and cognate questions will be found in the above-mentioned number of the *Journal of the Royal Anthropological Institute.* The paper in question is under the title: "On the Occurrence of Humanly-fashioned Flints, etc., in the 'Middle Glacial' Gravel at Ipswich, Suffolk."

The Italian anthropologists have in the past made more elaborate attempts than others to divide mankind into a large number of races or subspecies, according to definite anatomical criteria. The attempts have often over-reached themselves, more particularly as the results obtained have often been in no way correlated with the advance of Mendelian research. But in connection with this line of investigation an important paper by Prof. V. Giuffrida-Ruggeri should be mentioned. The paper occurs in *Archivio per l'Antropologia e la Etnologia* (vol. xlvii), and is entitled, " Prime Linee di un' Antropologia Sistematica dell' Asia."

One of the most remarkable finds of Anglo-Saxon remains ever made has recently come to light through the investigations of Captain Vaughan Williams, of Manor Lodge, Old Windsor. The find was made by Captain Williams in Windsor Park, where the park borders on Egham parish. Captain Williams had suspected that Edward the Confessor had a palace near this spot, and in the result, numerous Anglo-Saxon remains were unearthed. It was not only Saxon relics which were found, however. The site appears to have been occupied by the Romans; and some of the Saxon relics belong to the Pagan period. A perfect example of a Saxon kitchen was found. A full and interesting account of these researches will be found in the *Morning Post* for January 13, and the investigations are being continued.

The following papers on social anthropology may be recorded:

In the *Journal of the Royal Anthropological Institute*, vol. xlix, Jan.–June 1919): " The Application of Anthropological Methods to Tribal Development in New Guinea," by E. W. P. Chinnery; " Rain-making among the Lango," by J. H. Driberg; and " Social Organisation in San Cristoval, Solomon Islands," by C. E. Fox. In *Man*: " Mothers and Children at Zuni, New Mexico," by Elsie Clews Parsons (November); " Relationships in Central America," by A. C. Breton (December); and " The Kopiravi Cult of the Namau, Papua," by A. C. Haddon (December). And in *Biometrika* (vol. xii, pts. 3 and 4, Nov. 1919): " The Inheritance of Psychical Characters," by Karl Pearson.

And the following papers on physical anthropology may be recorded:

In the *Journal of the Royal Anthropological Institute* (as above): " Anthropological Observations on German Prisoners of War," by F. G. Parsons. In the *Proceedings of the Royal Society* (Series B, vol. xci, No. B635: " A Preliminary Study of the Energy Expenditure and Food Requirements of Women Workers," by O. Rosenheim; and " Report on the Metabolism of Female Munition Workers," by M. Greenwood, C. Hodson, and A. E. Tebb. In *Rivista di Antropologia* (vol. xxii): " L'Indice Trocanterioco e L'Indice Pubico," by Prof. V. Giuffrida-Ruggeri.

And the following papers on prehistoric anthropology may also be recorded :

In the *Journal of the Royal Anthropological Institute* (as above) : "Early Fijians," by A. M. Hocurt. In *Man*: "An Irish Decorated Socketed Axe," by Sir W. Ridgeway (November) ; and "A Piece of Carved Chalk from Suffolk," by J. Reid Moir (December). And in *Spolia Zeylanica* (vol. xi, pt. 41, October 1919), "Outlines of the Stone Ages of Ceylon," by E. J. Wayland.

EDUCATION. By A. E. Heath, M.A., University, Manchester.

The problem of the relation between educational theory and cognate sciences is raised in several recent publications. The issue as a whole is complex ; but the matter discussed seems to fall into separate sections.

(1) First of all, it is becoming increasingly clear that there are two distinct purposes for which the special sciences can be pressed into the service of education. They may be used to improve the means for attaining previously decided ends ; or they may be used to provide a solid basis for criticism of the ends proposed. In his Inaugural Address to the newly constituted Educational Section of the British Psychological Society, Prof. T. P. Nunn expresses this as follows : "Education is a biological function more ancient even than man, for it is found in a rudimentary form among animals. . . . In relation to that great vital function, the psychologist must always be contented with the position of a critic, whose primary business is not to determine the aims of education, but to ensure efficiency and economy in the means by which those aims are pursued. He is concerned with the aims of education only in a secondary way, in so far as his criticism of traditional procedure may lead to what is, in effect, a revaluation of accepted ideals" (*Brit. Journ. of Psy.*, 1920, **10**, 2 and 3, pp. 169–76). These remarks clearly apply also to any of the sciences cognate to education. They may, however, be supplemented by Dr. R. R. Rusk's statement that it is for experimental education to decide whether the aims proposed are compatible with the child's nature (*Introduction to Experimental Education*, Longmans, 1912 ed., p. 8).

(2) Let us, then, accept the view that the end of education is, as Prof. James Ward put it long ago, a wider "social and ethical problem" (*Journ. of Ed.*, Nov. 1890) ; and let us therefore confine ourselves to the application of the special sciences to educational method. In this restricted field a steady drift of opinion is apparent towards the view that education is an autonomous study, with its own special difficulties and subject-matter, and not a mere dumping-ground for other sciences. The concepts necessary for its ordering should therefore be developed in the field of education itself : they cannot,

without danger, be lifted bodily from cognate sciences—any more than the concepts of pure mathematics can be transferred complete to mechanics, but must rather be used to further the development of the special concepts required in the latter domain. Prof. H. Bompas Smith puts this point clearly in his plea for an educational psychology with interests and special methods of its own: "The illegitimate procedure is to take over the selection of material and the detailed methods of one branch of science, and force them into the service of some distinct but kindred interest, without effecting the modifications which are required" (*Journ. of Exp. Ped.*, 1919, **5**, 2, pp. 57–67). This view clearly involves us in educational experiment in the school itself. In the interval between the first and second editions of Dr. Rusk's *Experimental Education*—that is, from 1912 to 1919—such experimentation has grown from small and tentative beginnings to firmly established foundations for future work. Dr. Rusk goes so far as to assert in his later edition that "we have now reached a point in educational enlightenment where opposition to the scientific method must be frankly pronounced a prejudice." One has only to glance through the papers published, during the few years of its existence, in the *Journal of Experimental Pedagogy*, to see that there is much to justify that assertion. In his lecture before the Modern Language Association, to inaugurate the Department of Educational Experiment, Mr. E. Allison Peers contends that the day of the mere opinionative pedant has passed. "Not only can experiment alone inquire into questions of method, but the results of experiment alone can command respect." The old heresy that the personality of the teacher is a factor which will upset all experiments is, he continues, mere nonsense. All experimentalists know what careful allowance has to be made for the personal element in this science as in others. To the opinion that personality is the only thing that matters in teaching, Mr. Peers simply replies: "Personality is, of course, a most important factor in education. . . . But why not have everywhere the best teaching obtainable, and the personality where we can, in addition, to inspire it?" (*Journ. of Exp. Ped.*, 1920, **5**, 4, pp. 179–87). To this we may add that the one in some degree involves the other. For we do not mean by personality, in this context, mere individual idiosyncrasy, but something which involves the same elements of self-control that are reflected in the control of subject-matter we call method.

(3) The ground of criticism of this view, that so far as method is concerned education is a science of co-ordinate rank with the other sciences, has significantly shifted. What intelligent criticism there is does not now consist in the contention

that this way is sterile, but rather that it is too fruitful. An able and witty critique on these lines appeared recently in *The Times Educational Supplement* (April 15, 1920, p. 193). The carefully designed devices for stimulating every side of the child's activity are so efficient, it is urged, that they lead to overstrain. The only possible reply is that if they do so they are not carefully designed. For the first essential to a reasonable procedure will be a complete realisation of the mental and physical needs of the child ; and to be fair we must acknowledge that in the best modern work the factor of fatigue is kept constantly in view. If any type of schooling interferes with growth, that type must go. But it will fall under the blows of rational criticism, and not if we weakly rely on drift and incompetence as the sole way out of intolerable situations.

The following is a selection of references to recent work :

Grace A. de Laguna, *Psy. Rev.*, 1919, **26**, 6, pp. 409–27, " Emotion and Perception from the Behaviorist Standpoint," gives a good impression of the general tendency towards a more distinctly biological view of behaviour ; R. L. Archer, *Journ. of Exp. Ped.*, 1919, **5**, 1, pp. 7–17, " What is Suggestion ? " ; M. F. Basset and C. J. Warne, *Amer. Journ. of Psy.*, 1919, **30**, 4, pp. 415–18, " On the Lapse of Verbal Meaning with Repetition," a set of experiments with an obvious moral ; T. L. Patterson, *Journ. of Educ. Psy.*, 1918, **9**, pp. 497–510, " Pedagogical Suggestions from Memory Tests."

C. J. Parsons, *Brit. Journ. of Psy.*, 1917, **9**, pp. 74–92, " Children's Interpretation of Ink-blots : a Study in Some Characteristics of Children's Imagination " ; William Boyd, *Journ. of Exp. Ped.*, 1919, **5**, 3, pp. 128–39, " A Child's Fears " ; Canon Stuart Blofeld, *loc. cit.*, 1920, **5**, 4, pp. 168–74, " The Problem of Leisure " : the work and leisure of a man should interact the one with the other for the good of each ; so, if our aim is the development of the whole human being, we should find a place for the cultivation of the resources, both internal and external, that make for a graceful leisure.

S. S. Brierley, *Journ. of Exp. Ped.*, 1918, **4**, 5, pp. 239–54, " Analysis of the Spelling Process " ; R. C. Moore, *loc. cit.*, 1918, **4**, 5, pp. 221–36, " The Psychology of Number " ; G. C. Brandenburg, *Journ. of Educ. Psy.*, 1918, **9**, pp. 632–6, " Psychological Aspects of Language " ; Katherine Steinthal, *Journ. of Exp. Ped.*, 1919, **5**, 3, pp. 146–9, " An Experiment in Teaching Elementary Geometry," an interesting method of engaging the free activities of young children in class-work by the introduction of a mediæval " scholastic tournament." Mr. E. A. Craddock's experiment referred to in a previous article (SCIENCE PROGRESS, April 1920, p. 585) is now set out in full in his newly published book, *The Classroom Republic* (Black, 1920).

These notes would be incomplete without some reference to the appearance of Prof. Nunn's volume, *Education : Its Data and First Principles* (Arnold, 1920). It is given to few books to bear from the outset the obvious mark of lasting importance. That this claim can be made for the volume before us arises in the main, I think, from two circumstances. In the first place, Prof. Nunn has deliberately levered himself out of the current confusion of means and ends, and boldly set himself the task of " reasserting the claim of Individuality to be re-

garded as the supreme educational end, and to protect that ideal against both the misprision of its critics and the incautious advocacy of some of its friends." The clarity of outlook and intellectual honesty, which render it easy for friend and foe alike to appreciate or criticise the defence of this thesis, are sufficient to raise it above the ordinary level. But there is a second reason for seeing in the book something of permanent value. The great development of psychological science in recent times has almost wholly consisted in advance in our knowledge of the non-cognitive aspects of human behaviour. We need only mention Prof. S. Alexander's fruitful "Sketch-Plan" of a conational psychology; the work of Dr. McDougall on instinct and emotion; and the increased insight into normal behaviour which has followed the regarding of mental abnormalities, not as wholly unique and incalculable, but as expressions of the over-development of factors already present in the normal. This replacement of the older, too exclusive concern with the cognitive side of human behaviour by a psychology based more closely on the biological aspects of the living creature has often, however, been seized upon as an excuse for an entirely gratuitous irrationalism. It is an outstanding merit of Prof. Nunn's book that it sifts the grain from the chaff, and embodies the results of modern advances cleared of such irrelevant accretions. Moreover, by the use of certain new terms Prof. Nunn is able to effect a most convenient grouping of those results; and so to provide the means both of bringing out the common characteristics of behaviour at all biological levels, and of building his treatment of educational questions on firm and wide foundations.

ARTICLES

LANGMUIR'S THEORY OF THE ARRANGEMENT OF ELECTRONS IN ATOMS AND MOLECULES.[1]

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The theory propounded by Langmuir, published in the June number of the *Journal of the American Chemical Society*, explains so wonderfully the properties of elements and compounds and brings to light such remarkable relationships, formerly unsuspected, that it necessarily contains a large element of truth. Although, doubtless, it will be subject to considerable modification, the power of the theory is so great that it must have the widest application.

Hitherto the problem of atomic structure has usually been attacked from the physical point of view, with little consideration of the vast store of chemical relationships which, in Langmuir's opinion, provide a better foundation for a theory than the relatively meagre physical data available. Disregarding for the present the physical evidence in favour of their rotation, the electrons in atoms are assumed to be stationary, or to oscillate about a mean position, in accordance with the stereochemical testimony that the primary valence forces between atoms act relatively in nearly fixed directions. For example, if wood is carbonised under certain conditions, or if tungsten trioxide is reduced in very dry hydrogen, the resulting elementary substances occupy about the same volume as the original compound. The volumes are about twenty times as great as those of the corresponding elements in their crystalline form. The whole behaviour of such bodies tends to show that their atoms are arranged in fixed branching chains, indicating, as Langmuir considers, that the valency electrons are immobile.

In conformity with this supposition, each atom is conceived as comprising a nucleus of positive charges of electricity around which the electrons are disposed in a definite configuration. The number of unit positive charges in the nucleus is the same as the atomic number of the element, which corresponds to its position when the elements are arranged in order of increasing atomic weight. The equivalent number of electrons or unit

[1] *Jour. Amer. Chem. Soc.*, 1919, **41**, 868; Hendrick, The Langmuir Postulates, *Chem. and Metall. Eng.*, 1919, **21**, 73.

negative charges are allocated in concentric shells of equal thickness around the nucleus, each shell being divided into a number of equal cells proportional to the area of the shell. The first shell has two cells. The second shell, having four times the surface, contains eight cells, the third eighteen, and the fourth thirty-two cells. Moreover, each cell, except the two innermost, may contain either one or two electrons, which are usually arranged radially. But before a given cell can contain more than one electron, all the cells in the inner shells must be completely filled. Consequently the electrons in each shell, except the first, are arranged in two layers which may be numbered I, II*a*, II*b*, III*a*, III*b*, IV*a* and IV*b*.

The following table shows the arrangement of the electrons in the atoms of each element :

Layer.	Number of electrons in kernel.	Number of electrons in outside shell.										
I.	—	0	1 H	2 He	3	4	5	6	7	8	9	10
II*a*	2	He	Li	Be	B	C	N	O	F	Ne		
II*b*	10	Ne	Na	Mg	Al	Si	P	S	Cl	A		
III*a*	18	A	K	Ca	Sc	Ti	V	Cr	Mn	Fe	Co	Ni
		10	11	12	13	14	15	16	17	18		
III*a*		Niβ	Cu	Zn	Ga	Ge	As	Se	Br	Kr		
		0	1	2	3	4	5		7	8	9	10
III*b*	36	Kr	Rb	Sr	Y	Zr	Cb	Mo	43	Ru	Rh	Pd
		10	11	12	13	14	15	16	17	18		
III*b*		Pdβ	Ag	Cd	In	Sn	Sb	Te	I	Xe		
		0	1	2	3	4	5	6	7	8	9	10
IV*a*	54	Xe	Cs	Ba	La	Ce	Pr	Nd	61	Sa	Eu	Gd
			11	12	13	14	15	16	17	18		
IV*a*			Tb	Ho	Dy	Er	Tm	Tm_2	Yb	Lu		
		14	15	16	17	18	19	20	21	22	23	24
IV*a*		Erβ	Tm	$Tm_2\beta$	Ybβ	Luβ	Ta	W	75	Os	Ir	Pt
			25	26	27	28	29	30	31	32		
IV*a*		Ptβ	Au	Hg	Tl	Pb	Bi	Ra*F*	85	Nt		
		0	1	2	3	4	5	6				
IV*b*	86	Nt	87	Ra	Ac	Th	Ux_2	U				

Under the influence of the attraction of the nucleus, and of their mutual repulsions, the electrons tend to arrange themselves in definite groups. In the first shell the stable formation is the pair, in subsequent layers the octet appears to be the most favourable group. In this case the actual arrangement of the electrons in space is at the eight corners of a cube. If the electrons in a single atom are unable to dispose themselves in such a permanent configuration, they tend to associate themselves with the electrons in other atoms in such a way as to do so. This is the explanation of chemical combination.

The realisation of a stable formation in a single atom results in the production of the inert gases of the first group in the periodic table. With Helium ($N = 2$)[1] the positive charge on its nucleus is 2, and it has 2 electrons. These dispose themselves symmetrically on either side of the nucleus, each in the centre of its shell. The symmetrical distribution results in almost complete external compensation of the electric forces with little stray field. Helium is therefore the most stable element known. Because the electrons are as suitably grouped as possible, with little external field of force, there is no tendency to associate with atoms either to form molecules or chemical compounds. Consequently helium is an inert gas with a boiling-point only 4½ degrees above absolute zero.

The next element in this group is Neon ($N = 10$), with a positive charge of 10, and 10 electrons. The first 2 electrons complete the first shell, which corresponds to that of the helium atom, and the remaining 8 form an octet in the layer II*a*. Thus a second stable configuration has been attained, and Neon is the second most stable element. The following element in this group, Argon ($N = 18$), has a nucleus with 18 positive charges and 18 electrons disposed around. Here we have 2 electrons in the inner shell, 8 in layer II*a*, corresponding to Neon, and another 8 in layer II*b*. Argon is, therefore, Neon with an extra octet. Since the electrons are again completely satisfied, we have another inert gas with low boiling-point and no tendency to form molecules.

In the third shell there can be 18 electrons, 9 in each hemisphere. One of these electrons will go to the pole, forming a pair with the corresponding electron in the other hemisphere, and the remaining 16 are distributed symmetrically about them, 8 in each hemisphere, forming 2 octets. This arrangement corresponds to Krypton. Xenon has a further 18 electrons in the III*b* layer, and Niton follows at the end with 32 electrons in the fourth shell, 16 in each hemisphere. These inert elements are therefore built up of consecutive layers of electrons

[1] N = Atomic number.

arranged in the ideal positions about the nucleus, which is the reason for their stability and inactivity.

With Hydrogen ($N = 1$) the first element in the next group, the conditions are very different. It has a single positive charge and a single electron. Its positive and negative poles form an electric doublet of high moment which tends to attract all other bodies something like a small magnet. Thus atomic hydrogen is very strongly adsorbed on surfaces. By sharing their electrons, two hydrogen atoms can hold a stable pair, and therefore the atoms form diatomic molecules. These have an unusually weak external field, so that hydrogen is a gas with a very low boiling-point. For Lithium ($N = 3$) the nucleus has 3 positive charges surrounded by 3 electrons. Two of these electrons complete the first shell, forming a kernel similar to the Helium atom, but with an extra positive charge, which can hold an additional electron in the second shell. This extra electron tends to make the atom very active chemically. Like those of hydrogen, the lithium atoms are electric doublets and attract one another. However, owing to the greater size of the kernel compared with the hydrogen nucleus, there is still a strong electric field around a pair of atoms, so that they can attract a third, and so on. Thus, lithium is a solid ; its atoms do not form molecules, but the positive kernels and single outside electrons arrange themselves in a space lattice in a manner quite similar to the sodium and chlorine in a crystal of sodium chloride. The single electrons, being surrounded on all sides by positive charges, are free to move under an electric force, and metallic lithium is a conductor of electricity.

The chemical properties of lithium, beryllium, and boron, are determined largely by their tendency to revert to the stable form corresponding to helium. They are said to give up their extra electrons to form stable arrangements with the electrons of other atoms ; thus they have respectively unit, double, and treble valencies.

With Carbon ($N = 6$), Nitrogen ($N = 7$), and Oxygen ($N = 8$), which have respectively 4, 5, and 6 electrons in the outside shell, a tendency becomes manifest to take up electrons to form stable octets. This opens up new possibilities for the formation of compounds, and explains the remarkable differences in the properties of these elements from those of lithium and beryllium.

With Fluorine ($N = 9$) there are 7 so-called " free " electrons in the outer shell, and the octet is nearly complete. The properties of fluorine are determined by its intense desire to attain to these stable groupings. When brought into contact with an atom having a single free electron, *e.g.* lithium, the

extra electron is taken up. The outer shell now resembles that of the inert atom of neon, with the difference that within there are only 7 positive charges instead of 8. Consequently the fluorine atom becomes negatively charged, while the lithium atom is positively charged, having given up its electron. In other words, the fluorine and lithium " ions " are held together by electrostatic forces. There will be a free external field, and lithium fluoride is a solid body, building up a space lattice like metallic lithium. It is a non-conductor because, every octet being completed, there are no free electrons. On melting, or solution, it becomes a conductor, since the ions can move under the action of an electromotive force.

Alternatively, an atom of fluorine can complete its octet by sharing a pair of electrons with another atom of the same or another kind. If we think of the electrons in the outer shell as occupying the corners of a cube, the molecule of fluorine may be pictured as two cubes with an edge in common, two electrons doing duty in both cubes. The molecule is again very stable, as exemplified by the very low boiling-point of the gas.

This view of chemical combination as the sharing of electrons leads to Langmuir's octet theory of covalence. The covalency of an atom is the number of pairs of electrons it shares with other atoms. If we represent by e the total number of available electrons in the outside shells of the atoms forming a given compound, and let n be the number of octets formed, holding p pairs of electrons in common, we see that, for every pair of electrons shared, there is a saving of $2p$ in the number of electrons needed to form octets. We have, therefore

$$e = 8n - 2p, \text{ or} \qquad \text{(i)}$$
$$p = \tfrac{1}{2}(8n - e) \qquad \text{(ii)}$$

Of course, electrons held by a hydrogen nucleus in common with an octet must not be counted in reckoning the value of p, since they do not result in any saving in the numbers of electrons required to form octets.

The use of formula (ii) will be clear if we employ it to determine the structures of a few compounds. The internal arrangement of water molecules is of great interest from its wide application as a solvent. Since the two hydrogen nuclei always tend to hold pairs of electrons and never octets, we may take $n = 1$ for the oxygen atom. There are 6 available electrons in the oxygen atom and 2 in the two hydrogen atoms, making $e = 8$, whence equation (ii) gives $p = 0$. This means that no electrons are held in common between octets, which is obvious in this case, since there is only one octet. The two hydrogen

nuclei are held by electrostatic attraction to two pairs of the electrons forming the octet. The water molecule may, therefore, be pictured as a cube with two hydrogen nuclei hanging on to opposite edges. This structure indicates that water forms molecules in which the electrostatic forces are almost completely compensated internally. All the electrons form an octet, and hence the molecule should have a rather weak external field of force. Water should, therefore, be easily volatile and should not be a good conductor of electricity. But the less symmetry of the molecule, as compared with the neon atom, shows that the boiling-point should be much higher.

For carbon dioxide, CO_2, we expect each atom to form an octet. Supposing $n = 3$, we have $e = 4 + 2 \times 6 = 16$, which gives $p = 4$. This means that four pairs of electrons are shared between three octets, leading to a structure resembling three cubes arranged side by side in a row with the carbon cube between two oxygen cubes. Consequently carbon dioxide is a thoroughly saturated non-polar substance, a non-conductor of electricity, easily volatile and chemically rather inert.

This type of union, in which two atoms are joined by sharing pairs of electrons, is regarded as the normal mode of chemical combination, as distinguished from that which results in the formation of salts, such as lithium fluoride, when one or more free electrons are "given up." It will be noted that the forces holding the atoms together are electrostatic in both cases; but, while the metallic ion has no definite point of attachment, in normal compounds the relative positions of the atoms are fixed.

The characteristic distinction between a neutral compound such as CO_2 and a salt like LiF lies in the fact that, while the molecule of the former is electrically non-polar, with little external field of force, the molecule of the latter consists of two distinct parts or ions, which are oppositely charged, and, though bound together by a strong internal field, are capable of separation in solution, or in the fused state, by the action of an electromotive force. There is also considerable external field.

It will be found that equation (ii) leads to results identical with the ordinary theory of valency when applied to most inorganic and almost all organic compounds. In addition, however, it explains the structures of many substances which have hitherto been difficult to account for.

There is hardly a case where the ordinary theory of valency fails so completely as for the compounds of nitrogen. Let us see how the octet theory applies to these bodies. For the compounds with hydrogen the theory postulates the occurrence of NH_3 and H_2N-NH_2 with the properties they possess, while

the compounds NH, NH_3, NH_4, and NH_5 cannot exist, since in these cases we find $p = 1, \frac{1}{2}, -\frac{1}{2}$ and -1 respectively, instead of zero, as it must be for a single octet. For the oxides we see from (ii) that if p is to be an integer, e must always be even. Since nitrogen has 5 available electrons and oxygen 6, there must always be an odd number of electrons unless the number of nitrogen atoms is even. This gives N_2O_x as the simplest general formula for the nitrogen oxides.

When $x = 1$ the theory indicates the structure N = O = N or N = N = O, where each valence link represents the sharing of a pair of electrons. The extraordinary fact immediately appears that the internal arrangement of such a substance should be almost exactly like that of carbon dioxide. Both molecules are represented by three cubes side by side in a row. CO_2 has a carbon atom with a charge on the nucleus of 4 positive units, while the two oxygen nuclei have 6 positive units, making a total positive charge of 16. N_2O has two nitrogen atoms each with 5 positive charges, and one oxygen atom with 6 positive units, the total being 16 as with CO_2. In both cases the nuclei are surrounded by completed octets, giving a small external field which must be very similar for both molecules. If the theory is correct the physical properties of these two gases should be almost identical. The following table shows how remarkable is this agreement:

	CO_2	N_2O
Critical temperature	31·9° C.	35·4° C.
Critical pressure	77 atmos.	75 atmos.
Solubility in H_2O at 0° C. . .	1·780	1·305
Solubility in EtOH at 15° C. . .	3·13	3·25
Density of liquid at −20° C. . .	1·031	0·996
Density of liquid at −10° C. . .	0·858	0·856
Viscosity at 20° C.	148×10^{-6}	148×10^{-6}
Heat conductivity at 100° C . .	0·0506	0·0506
Refractive index μ_D at 16° C. . .	1·190	1·193
Dielectric constant of liquid at 0° C.	1·582	1·598
Magnetic susceptibility of gas at 40 atmos. 16° C.	$0{\cdot}12 \times 10^{-6}$	$0{\cdot}12 \times 10^{-6}$

Both gases form hydrates with six molecules of water. The vapour pressure of $N_2O{\cdot}6H_2O$ is 5 atmospheres at 6° C., while the other hydrate has the same vapour pressure at 15° lower. The surface tension of liquid N_2O is 2·9 $\frac{\text{dynes}}{\text{cm.}^2}$ at 12·2° C., while CO_2 has the same surface tension at 9·0° C. The power of the new theory is strikingly illustrated by its indication of such extraordinary relationships between compounds the properties of which have been known, though uncorrelated, for so many years.

Returning to the consideration of the oxides of nitrogen, we find that the octet theory accounts for N_2O_2 as $O = N - N = O$, N_2O_3 as $O = N - O - N = O$ or

$$\begin{array}{c}O = N - N = O, \\ \begin{array}{l}\ \ \ \ \ \ \| \\ \ \ \ \ \ \ O\end{array}\end{array}$$

N_2O_4 as

$$\begin{array}{ccccc}O = & N & - & N & = O, \\ & \| & & \| & \\ & O & & O & \end{array}$$

N_2O_5 as

$$\begin{array}{ccccccc}O = & N & - & O & - & N & = O. \\ & \| & & & & \| & \\ & O & & & & O & \end{array}$$

For N_2O_6 and higher values of x, the number of electrons required becomes greater than can be formed from the available electrons in the atoms, except on the unlikely supposition of the formation of rings or strings of oxygen atoms. The formation of NO and NO_2 is more difficult to account for. Let us first consider the structure of the nitrogen molecule. The properties of elementary nitrogen are in many ways extraordinary, and the contrast between its properties and those of carbon, which is adjacent to it in the periodic table, could hardly be more striking. Carbon does not melt at 3,700° C., and combines readily with oxygen at moderate temperatures. On the other hand, nitrogen has a lower boiling-point than any element except hydrogen, helium, and neon, and combines with oxygen only at exceedingly high temperatures. Moreover, the nitrogen molecule is not appreciably dissociated even at 3,900° C. This stability of the molecule indicates some unusual kind of structure. Supposing the nitrogen molecule contains two octets, we have $n = 2$, $e = 10$, $p = 3$, giving a structure $N \equiv N$, resembling that of acetylene, a substance which is endothermic, relatively unstable, and easily forms addition products. Such a structure could not account for the nitrogen molecule.

Many considerations show that the absolute boiling-points of substances give a measure of the external field of force of their molecules, while the freezing-points appear to be largely dependent on the symmetry of the molecules. In many of its properties nitrogen resembles argon. Let us compare these properties of O_2, N_2, and Ar:

	O_2	N_2	Ar
Freezing-point	38° K	63°	85°
Boiling-point	90°	77°	87°
Difference	52	14	2

Its boiling-point indicates that nitrogen has a weaker field than either oxygen or argon, while the difference between the freezing-point and boiling-point shows that nitrogen is much more symmetrical than oxygen and more like argon. The evidence suggests, therefore, that the nitrogen molecule is surrounded by a single octet like that in the outer shell of argon. Actually two nitrogen atoms have 14 electrons, of

which 2 each go to complete the first shells, leaving 10 over. In view of the stability of the octet, it seems reasonable to suppose that 8 of these electrons arrange themselves in one octet around two nuclei, leaving 2 extra electrons imprisoned somewhere within, held by the attraction of the 14 positive charges. Indeed, there seem to be a number of exceptional factors which tend to produce this result. The 5 free electrons in each atom cannot form two octets without sharing 3 electrons. This does not seem to be a stable grouping in the case of nitrogen. Nor can three atoms form a molecule, since this would give an odd number of electrons. Actually, there are only 2 electrons more than are required to form a single octet round two kernels of small volume and large positive charges. This structure explains satisfactorily the remarkable qualities of nitrogen, the great number of endothermic compounds it forms, the inertness of the nitrogen molecule, and the activity of the element in combination.

The structure of carbon monoxide has long been a puzzling problem. According to the usual view carbon should be divalent in this compound and very much unsaturated. On the contrary, it is a relatively inactive substance. Its very low boiling-point indicates little external field, which is confirmed by its small solubility in water and the few substances with which it combines at ordinary temperatures. If we apply the octet theory, and suppose $n = 2$ for the two atoms, we have $e = 10$, which gives $p = 3$. Again, this result would indicate a very unsaturated compound. Here also we have a total of 14 electrons in the outer shell, around two nuclei, one with 6 positive charges and the other with 8, or 14 positive charges in all. This at once suggests that the arrangement may be similar to that of the nitrogen molecule. The properties of carbon monoxide should therefore be co-ordinate with those of nitrogen. These are tabulated for comparison below:

	CO	N_2
Freezing-point	66° K	63° K
Boiling-point	83°	78°
Critical temperature	122°	127°
Critical pressure	35 atmos.	33 atmos.
Critical volume	5·05	5·17
Solubility in H_2O at 0° C	3·5	2·46
Density at boiling-point	0·793	0·797
Viscosity at 0° C	163×10^{-6}	166×10^{-6}

Similar considerations indicate an analogous arrangement for the nitric oxide molecule. It has 15 electrons, only one more than nitrogen and carbon monoxide, and the remarkable paramagnetic properties of nitric oxide may be due to this extra electron.

Substances like CO_2 and N_2O, or N_2 and CO, which have the same number and arrangement of electrons, Langmuir calls "isosteres." If the substances have also equal positive charges, their external fields will be very similar, leading to almost identical physical properties. If the nuclei differ in total charge, one of the isosteres must be electrically charged when the other is not, so that the external fields will be very different and most of their physical properties will also differ. A number of such cases are described in the original paper.

In conclusion we will consider the structure of hydrofluoric acid as an example of an acid. The octet theory indicates that the hydrogen atom shares its electron with the fluorine atom, thus completing the stable pair in the first shell for the hydrogen atom and forming an octet round the fluorine atom. The hydrogen nucleus is then held by a pair of electrons forming the octet. From its structure the molecule of hydrofluoric acid should have a small stray field. Most of its surface resembles that of neon, except where the hydrogen nucleus is. But lack of symmetry should make the stray field greater than that of hydrogen or neon. We thus expect hydrofluoric acid to be a gas or liquid not greatly different from water in its boiling-point. As a liquid it should be a non-conductor, but, because of its polar character, it should become an electrolyte when dissolved in water.

The properties of a few typical elements and compounds have now been considered. To continue the investigation would exceed the limits of a short paper and, incidentally, both the reader's and the editor's patience. For a fuller account reference must be made to the original paper, the study of which may be facilitated in some degree by this preliminary survey. It will there be seen that the theory explains the magnetic properties of the ferromagnetic elements as well as the peculiar characteristics of the metals of the rare earths. In addition, the covalence equation accounts for the existence of the class of substances which Werner regards as second-order compounds, and shows that they may preferably be looked upon as typical primary valence compounds. Unfortunately, the hypothesis is not easily reconciled with Bohr's theory of the astronomical atom which has had such marked success in explaining spectra, especially those of hydrogen, but Langmuir hopes that later developments may show that the two theories are not inconsistent. Indeed, it seems as if stationary electrons are unnecessary for Langmuir's theory. The resultant force holding two atoms is electrostatic, and may be fairly constant in direction, while the shared electrons continue to revolve in conformity with the electric field.

CORNISH PHENOLOGY, 1912—1919

FRANK H. PERRYCOSTE, B.Sc., AND HONOR M. M. PERRYCOSTE

THERE is no need to emphasise the importance of systematised phenological records as indices to what one may perhaps call resultant climate; or to point out that the nett effect of temperature, rainfall, latitude, elevation, exposure, etc., is summed up in, *e.g.*, the dates of flowering of plants without any ambiguity or possibility of error.

We understand that recently the Meteorological Society has made a special effort to increase the number of its phenological stations and to secure additional observers of the dates of flowering of a dozen or two plants under fairly rigid conditions. The objects of this article are (1) to illustrate the phenology of this part of Cornwall[1] during the last eight years by observations made under somewhat different conditions; and (2) to indicate the existence of a considerable mass of observations covering some twenty years in all parts of the country, and to show that these comprise very valuable phenological data which should be secured and worked up before they be destroyed—if indeed many have not already been destroyed.

There has existed for many years a certain "Wild Flower Society," the members of which, divided into some twenty branches, keep diaries of the dates of flowering of all the wild flowers they can find, and compete for first place in the branch. Since one mark is given for each flower found, but two marks for the earliest record (in each branch) of every flower; and since the coveted first place is won, not necessarily by the member who finds the largest number of flowers in the year, but sometimes by one who has found fewer flowers but has earned more "early marks"; the competition among the real enthusiasts is very keen: and the sequel will prove that this "early marks" system has resulted in the accumulation of records which have indubitable phenological value and should certainly be utilised.

The differences in character between the records at present demanded by the Meteorological Society and those available in the diaries of the Wild Flower Society are these: the

[1] *I.e.* Polperro, 20 miles west of Plymouth.

former asks for records of about a dozen species (plus certain extras if the observer feel inclined to record them), stipulates that in successive years the same or a closely adjacent plant shall be selected in each species for observation, and bars any record (except under a warning proviso) of a plant that may be supposed to have been in flower more than four days before it have been noted; whereas the latter affords data concerning a far larger number of species, but with no guarantee—or even probability in very many cases—that the locale and situation are identical in successive years, and no guarantee that the plant may not have been in flower for more than four days before discovery. It will be shown, however, that, if groups of plants be taken according to season, and their dates averaged, an obviously reliable result is obtained.

On the other hand, certain precautions must be observed. If the diarist be usually or frequently away from home in any given season, the flowers of that season will not be available among the records of his or her "station." In a general way, such flowers should be chosen as grow near the observer's home and are either so common or conspicuous—or so specially sought every year on account of their rarity—that there is small chance that their flowering will escape speedy observation; whereas plants that can be obtained only at the expense of a long walk or a cycle-ride may necessarily have to await the observer's leisure or a holiday, and thus may be obtained sometimes only appreciably after their first flowering.[1] Any very rare plant found only at a distance is specially unsuitable for these records: since, if experience have shown that no other member of the branch ever finds this species, even a keen competitor may defer a special journey for it until other and later-flowering plants can be obtained in the same locality.

One other proviso remains. No entry can be made in a Wild Flower Society diary before March 1—a date very suitable perhaps for the Northern members, but far too late for Southern stations. Here on the coast of South Cornwall, in a forward season, from four to five dozen plants are found in flower on that date, and some of them—*e.g.* Snowdrop, Primrose, Hellebore, Hazel, Small Celandine, etc.—have been in flower for weeks. Hence the absence from the following tables of very many common and familiar plants that would figure prominently in a Northern record.

There is, however, some compensation for this unfortunate blemish in the records, in that there is annually a keen competition among the members of the Society—irrespective of branches—to find the greatest number of flowers on March 1:

[1] These remarks do not apply if the observer be, *e.g.*, a country-doctor, or otherwise have daily occasion to wander far.

Group.	No. of Records.	Name of Species.	Time Range in Days.	Average Date of Flowering.
A	8	Luzula campestris	12	March 17 = 17th day
,,	8	Oxalis acetosella	49	,, 22 = 22nd ,,
,,	8	Anemone nemorosa	57	,, 27 = 27th ,,
,,	8	Salix viminalis	40	,, 27 = 27th ,,
,,	8	Geranium molle	49	,, 27 = 27th ,,
,,	6	Salix purpurea	33	,, 27 = 27th ,,
		Average	40	

(*Oxalis*, *S. viminalis*, and *Geranium* were in flower on March 1 in 1918, 1918, 1913, respectively.)

Group.	No. of Records.	Name of Species.	Time Range in Days.	Average Date of Flowering.
B	8	Myosotis collina	40	April 1 = 32nd day
,,	8	Allium ursinum	37	,, 3 = 34th ,,
,,	7	Potentilla tonmentilla	35	,, 5 = 36th ,,
,,	8	Anchusa sempervirens	42	,, 8 = 39th ,,
,,	8	Scilla nutans	39	,, 9 = 40th ,,
,,	8	Carex præcox	23	,, 10 = 41st ,,
,,	8	Moenchia erecta	21	,, 12 = 43rd ,,
,,	8	Prunus cerasus	25	,, 12 = 43rd ,,
,,	7	Trifolium subterraneum	44	,, 13 = 44th ,,
		Average	34	
C	6	Parietaria officinalis	31	April 16 = 47th day
,,	7	Orchis mascula	23	,, 17 = 48th ,,
,,	8	Alliaria officinalis	26	,, 19 = 50th ,,
,,	8	Acer pseudoplatanus	24	,, 22 = 53rd ,,
,,	8	Narcissus biflora	33	,, 22 = 53rd ,,
,,	7	Vicia hirsuta	31	,, 28 = 59th ,,
,,	7	Rumex acetosa	12	,, 29 = 60th ,,
		Average	25·7	
D	8	Carex flava	17	May 4 = 65th day
,,	5	Carex pendula	13	,, 5 = 66th ,,
,,	7	Cratægus oxycanthus	16	,, 7 = 68th ,,
,,	6	Nasturtium officinale	30	,, 9 = 70th ,,
,,	8	Veronica beccabunga	30	,, 15 = 76th ,,
,,	8	Spergularia rubra	30	,, 15 = 76th ,,
,,	7	Urtica dioica	11	,, 15 = 76th ,,
,,	8	Iris pseudacorus	24	,, 15 = 76th ,,
		Average	21·7	
E	8	Digitalis purpurea	12	May 17 = 78th day
,,	7	Bromus sterilis	17	,, 17 = 78th ,,
,,	6	Lychnis floscuculi	19	,, 17 = 78th ,,
,,	7	Rubus cæsius	27	,, 17 = 78th ,,
,,	7	Sambucus nigra	23	,, 17 = 78th ,,
,,	7	Hieracium pilosella	31	,, 17 = 78th ,,
,,	7	Aquilegia vulgaris	19	,, 17 = 78th ,,
,,	7	Sedum anglicum	32	,, 20 = 81st ,,
,,	8	Euphrasia officinalis	31	,, 20 = 81st ,,
,,	7	Solanum dulcamara	28	,, 22 = 83rd ,,
,,	7	Ranunculus flammula	21	,, 23 = 84th ,,
,,	7	Carduus palustris	12	,, 24 = 85th ,,
,,	8	Holcus lanatus	19	,, 25 = 86th ,,
,,	8	Trifolium procumbens	19	,, 27 = 88th ,,
,,	8	Lonicera periclymenum	22	,, 31 = 92nd ,,
,,	8	Scabiosa arvensis	22	,, 31 = 92nd ,,
		Average	22·1	

Group.	No. of Records.	Name of Species.	Time Range in Days.	Average Date of Flowering.
F	8	Malva sylvestris	17	June 1 = 93rd day
,,	8	Rosa canina	17	,, 1 = 93rd ,,
,,	6	Polygonum aviculare	24	,, 2 = 94th ,,
,,	8	Stellaria graminea	30	,, 3 = 95th ,,
,,	8	Poa pratensis	22	,, 3 = 95th ,,
,,	6	Valerianella auricula	26	,, 4 = 96th ,,
,,	8	Festuca ovina	16	,, 5 = 97th ,,
,,	7	Poa fluitans	26	,, 7 = 99th ,,
,,	8	Vicia cracca	15	,, 11 = 103rd ,,
,,	7	Mimulus luteus	26	,, 11 = 103rd ,,
,,	8	Galium mollugo	16	,, 12–13 = 104–5 ,,
,,	8	Achillea millefolia	19	,, 14 = 106th ,,
,,	7	Apium graveolens	27	,, 16 = 108th ,,
,,	7	Rosa rubiginosa	19	,, 17 = 109th ,,
,,	7	Ononis arvensis	20	,, 18 = 110th ,,
,,	8	Spiræa ulmaria	24	,, 20 = 112th ,,
		Average	21·5	
G	6	Trifolium arvense	20	June 25 = 117th day
,,	5	Teucrium scorodonium	17	,, 28 = 120th ,,
,,	6	Linaria vulgaris	24	,, 30 = 122th ,,
,,	5	Digraphis arundinacea	26	July 6 = 128th ,,
,,	7	Sambucus ebulus	19	,, 6 = 128th ,,
,,	6	Eupatorium cannabiense	23	,, 13 = 135th ,,
,,	7	Artemisia vulgaris	26	,, 19 = 141st ,,
,,	5	Arctium lappa	15	,, 20 = 142nd ,,
		Average	21·25	
H	6	Crithmum maritimum	14	July 28 = 150th day
,,	7	Mentha aquatica	24	August 1 = 154th ,,
,,	6	Mentha arvensis	30	,, 8 = 161st ,,
,,	7	Humulus lupulus	28	,, 8 = 161st ,,
,,	7	Spiranthes autumnalis	21	,, 25 = 178th ,,
,,	8	Hedera helix	22	Sept. 6 = 190th ,,
		Average	23·16	

and the March 1 records in successive years thus afford a valuable phenological index to February or even to January and perhaps December as well.

Now the junior of the present authors has been for over nine years a very keen and zealous member of the Wild Flower Society; and the data here utilised have been carefully selected from her diaries[1] from records of over 400 plants occurring in this neighbourhood. For reasons just given, all species normally or frequently found in flower on March 1

[1] As a precaution, the records of her first year—1911—have been ignored; as it seemed probable that she was not then so fully acquainted, as afterwards, with the habitats of some species, and may therefore have failed to find the flowers at their earliest dates.

are excluded—although the totals of these are dealt with separately. Others were rejected because they can be found only at a distance which involves a special expedition, and notoriously are not usually noted until they have been in flower for some time; whilst others again were rejected because one or two records were so aberrant in date as to suggest that in one case there had been an abnormally early "sport," and in another some accidental delay in looking for the flower. After all such rejections some ninety species were chosen and arranged in seasonal groups; but, since the list still included some species with an abnormally large time-range, these were critically discussed by us, and several rejected as unreliable; and finally, after the average time-range of each group had been determined, any species whose time-range exceeded this average by 50 per cent. was struck out. The curves ultimately obtained indicate very satisfactorily the reliability of our thus sifted data.

In the preceding tables we exhibit the seasonal groups, the time-range in days of each species and its average date of flowering during the eight years 1912-19, the average time-range of the group, and the average date of flowering –reckoned in days after February 28 or 29—of the group in each year. *These average group-dates are the data from which the curves are constructed.* Prefixed to the name of each species also is the number of records thereof during the eight years; for owing to occasional absences from home some species were sometimes collected elsewhere; and in such cases there are necessarily blanks in the phenological record for this place.[1]

[1] As regards species found in flower on March 1, it must be understood that these fall into several distinct series, viz.:

(*a*) Species always or normally in flower at this date here.

(*b*) Two or three species occasionally found in flower in exceptionally forward seasons.

(*c*) Stragglers, or survivors through a mild winter from the previous year. *Heracleum spondylium* and *Lychnis dioica* are characteristic normal survivors; but, since some species are found as survivors in one year and others in another, the total number thus unavailable for phenological purposes—for no species has been selected that is sometimes found as a survivor, since no record is made in the Wild Flower Society diary of the first flower (in the new season) of such species—is appreciable.

(*d*) Occasional obvious sports; *e.g.* the *Ox-eye Daisy* has been found on March 1.

Now, (*a*) and (*c*) are necessarily excluded from the groups. As regards (*d*), in perhaps one or two cases the species has been included for the years of normal flowering, and the "sporting" year treated as a blank. The only real perplexity has been caused by (*b*); and after consideration these species have been included in the groups as well as in the March 1 tables—but in each case a note has been appended to the table. The fact is that, after the exclusion of species normally or frequently found on March 1, so few March flowers were left that we could not afford to cut out these; but in at least one instance—as we shall have occasion to point out—such inclusion of one species appreciably affects a curve.

Let us turn now to the charts of curves. On Chart I are depicted graphically the numbers of plants found in flower on March 1—and on this chart we have included the record for 1911 also and the recent record for 1920.

The curve speaks for itself: its two outstanding features being the abnormal minimum of 1917—the result of the bitterly cold winter of 1916-17—and the abnormal maximum of 1920.

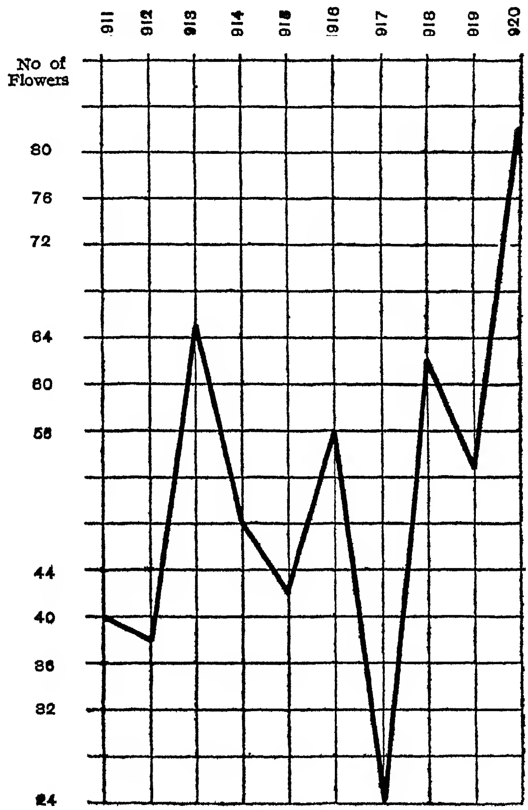


CHART I.

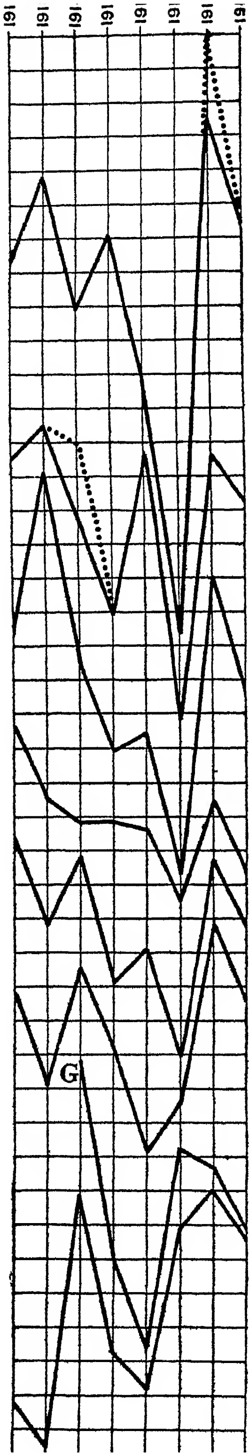
On Chart II we have the curves[1] from year to year of the groups A–H, and it seems desirable briefly to call attention to the leading characteristics of these. Curve A is generally parallel to the March 1 curve, but inverse in 1915 and 1916—thus indicating that, although 1915 was behind 1914 on March 1 (*i.e.* in February and January), it was more forward by the end of March; and that 1916 was more forward than 1915 and 1914 up to March 1, but behind both by the end of March.

[1] These were drawn on the vertical scale of 1 centimetre : 2 days.

To economise space the curves have been telescoped into one another as far as possible, so that, *e.g.*, day 44 on A and B and day 40 on C are at the same level; and in all 165 days have been telescoped into the space that would otherwise have accommodated only 83; but in each curve the scale of one centimetre : 2 days has been preserved. To give one illustration—curve A ranges from days 10-11 (March 10 and 11) in 1918 to between days 45 and 46 (April 14 and 15) in 1917.

The process of averaging necessarily yielded in various cases such results as 10·3 for a date.

The alternative dates on A for 1918 and B for 1914—giving rise to the alternative dotted curves—indicate the inclusion or exclusion of the species specially marked [1] and [2] in the tables of average dates.



So, too, curves A and B are inverse for the same years—B here agreeing with the March 1 curve; and the consequent indication is that by about mid-April the three years 1914-16 were relatively much as they had been before March.

We do not propose to describe in detail all the many parallelisms and fewer inversions which any reader may trace for himself on this chart; but we must call special attention to the striking phenomena of 1917 and 1916—pre-eminently of 1917. The effect of the abnormally cold winter of 1916-17 is strikingly indicated by the curve on Chart I, and by curves A,B,C,D,E; but F shows that before mid-June 1917 had recovered, and was rather more forward than 1916 in June, while G and H show that the year which had begun so much in arrear finished as only slightly less forward a year than the premier summer-years 1914 and 1918.

On the other hand, 1916, which had been ahead of 1915 before March and in the first half of April, and had just managed to keep ahead of it until the end of May, fell off in June and July and—as we have seen—was utterly beaten afterwards by 1917, which had started under so severe a handicap. We wish to draw special attention to the fact that by taking averages of seasonal groups, as we have done, it is thus possible to trace graphically the loss or gain of each year, month by month, relatively to other years.

A very little inspection of the eight curves on Chart II should suggest their combination into a smaller number; and on Chart III are exhibited the results of such combinations—viz. of A,B,C (March

AVERAGE DATES (RECKONED IN DAYS SINCE FEBRUARY) OF FLOWERING OF EACH GROUP IN SUCCESSIVE YEARS.

Group.	1912.	1913.	1914.	1915.	1916.	1917.	1918.	1919.
A . . .	23·4	18·3	26·2	21·8	30·3	45·2	10·3 or 15·0[1]	21·4
B . . .	35·0	32·9	34·0 or 38·8[2]	44·1	34·7	50·3	34·6	37·7
C . . .	49·2	39·6	51·0	56·1	55·1	63·3	45·9	52·9
D . . .	66·7	71·0	72·2	72·1	72·75	76·8	71·0	75·25
E . . .	77·2	82·3	78·2	85·9	83·8	91·6	78·75	82·5
F . . .	98·4	103·8	96·8	101·7	107·8	104·7	94·25	98·9
G . . .	136·25	[3]	120·3	131·9	137·25	125·5	126·6	130·3
H . . .	172·2	175	160·3	169·5	171·6	162·2	160	162·8

[1] According as March 1 records of *Oxalis* and *S. viminalis* are included or excluded.
[2] According as March 1 record of *Potentilla* included or not.
[3] Only one species recorded at home.

AVERAGE DATES ON COMBINATIONS OF GROUPS.

Group.	1912.	1913.	1914.	1915.	1916.	1917.	1918.	1919.
A+B+C . .	36·35	31·0	34·2	41·9	40·0	53·0	31·5	37·4
D+E . .	73·5	78·5	76·25	81·3	80·0	87·0	76·4	80·0
A+B+C+D+E	56·2	55·75	54·6	62·5	61·0	67·3	54·4	60·8
F+G+H . .	122·0	120·8	115·6	123·8	128·4	122·0	111·0	118·4

and April), D,E (May), A,B,C,D,E (March, April, May) and F,G,H (June, July, August). Except for the inversion of F,G,H to the other curves in 1916 and 1917—as remarked above—for the fall instead of rise of A,B,C from 1913-14, and the fall instead of rise of D,E from 1912-13, all the curves are approximately parallel; and we claim this final demonstration of parallelism as a conclusive justification for the utilisation of Wild Flower Society data according to our method as above described for the construction of phenological curves. From Chart III we learn that, as regards forwardness or backwardness, the eight years studied can be arranged as follows in descending order:

For the three spring months—

$$\left.\begin{matrix}1918\\1914\\1913\\1912\end{matrix}\right\} \quad \left.\begin{matrix}1916\\1915\end{matrix}\right\} \quad 1919 \quad 1917$$

and for the summer months—

$$1918\text{–}14\text{–}19\text{–}13\text{–}\left.\begin{matrix}12\\17\end{matrix}\right\}\text{–}15\text{–}16$$

In conclusion, we wish to appeal to phenologists to take the necessary steps to utilise—while it is still obtainable—the abundant mass of data existing in the diaries of members of the Wild Flower Society.

For any given phenological district the material may be treated as we have treated our material; and obviously the flowers chosen might and would include many excluded by us and *vice versa*; for instance, in the North of England and Scotland, where hardly any plants can be found in flower on March 1, nearly all the early flowers that we have been unable to use (except for the purposes of Chart I) would fall into the A or B seasonal groups. Moreover, notoriously the majority of species are absent from some or most localities; and there is therefore double reason for basing the phenological curves of different districts on their own specially characteristic common species.[1]

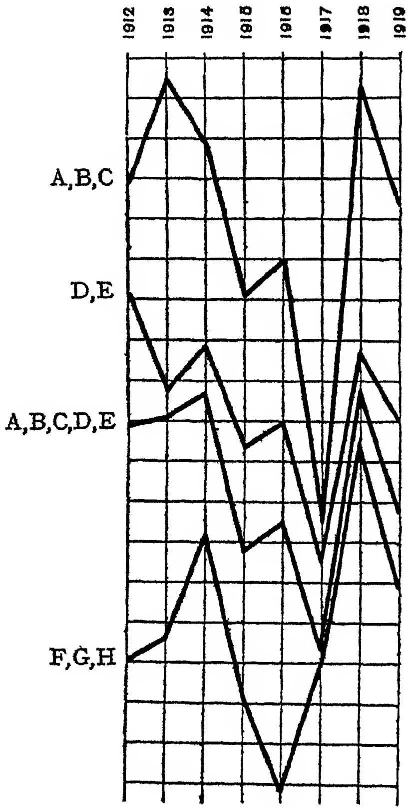


CHART 3.

Secondly, however, if a sufficiently large proportion of species common to each of a number of districts could be agreed upon, it would be possible to construct curves showing the seasonal differences relatively to variation in latitude and isotherms, etc. This, of course, is what the Meteorological Society aims at by the observation of a dozen very common species; but we suggest that the additional Wild Flower Society material is well worth the attention of the Society, and that the long series of records of flowers found on March 1 in all parts of England should be of especial value in this connection.

We quite realise that the great majority of the members of the Wild Flower Society would be utterly disinclined to utilise their data as we have utilised ours; and that, indeed,

[1] Incidentally in many districts the diaries of several members could be "pooled" as an extra safeguard.

even if not disinclined, they would probably modestly disclaim any faculty for constructing curves out of dates: and we in no wise suggest that they be asked to do anything of that sort. Our suggestion is that a few keen phenologists, working in unison, should extract and utilise the data. It is highly desirable that there should be unity of method and that all the data be critically examined; and these desiderata necessitate that the work be done by a few phenologists working in common. It should be quite practicable to obtain from the President of the Wild Flower Society (who, as we happen to know, is anxious to assist phenological inquiry) a list of the names and addresses of members who have kept diaries for eight or ten years and upwards; and circular letters might be sent to these, or to a selected number of them, requesting the loan of their diaries for the extraction of the necessary data. We do not underrate the expenditure of time and labour involved—for we realise how much have been required for the construction of the few curves on our charts; but we suggest that the work may be worth doing.

THE SIGNIFICANCE OF FACIAL BEAUTY

By C. F. BADCOCK

NATURE is prodigal of objects gratifying to the æsthetic sense, but it is universally recognised that such gratification culminates in the contemplation of the "human form divine," of which the beautiful face is at once the concentration and the symbol. In what essentially, we may ask, consists this powerful appeal made by personal beauty?

Sir Joshua Reynolds defined beauty as "the medium of form." E. H. Aitken considered that beauty might be defined in terms of motion—that a truly functioning motion, whether in mechanical action or natural growth, will result in beautiful form. "The designer of a yacht," he says, "studied the inexorable laws of dynamics, and produced a form which charms the cultured eye." "Geometry," said Rodin, "is at the bottom of sentiment."

Aitken remarks, "We discern beauty—as we discern harmony in music—by sense, the judgments of which are independent of our ability to give a reason for them." Reasons, however, there are; and, just as harmony in music rests upon a mathematical basis, so personal beauty should have its biological explanation.

A good torso and good limbs are beautiful as a concrete expression of perfect and well co-ordinated movement and efficient performance of function. So also with regard to the face, the beauty of which depends largely on good features, but still more on harmony or balance of features. All the features of the face should respectively constitute the outward and visible signs of an inward and organic efficiency; and the facial movements, whether expressing the emotions or sentiments, or masticating and insalivating food, must be effectively performed and well co-ordinated if the resulting contours of the face are to be considered beautiful.

A good deal of the æsthetic value of the face is based upon its faculties of emotional expression and indication of character. In his classical work, *The Expression of the Emotions*, Darwin emphasises the rôle of the respiratory system in emotional expression. Its effects are chiefly exhibited by the upper and

middle portions of the face—forehead, eyes, and nose. The lower and lateral, or "alimentary," portion is also of service in expressing emotional states; and on this portion—jaws, mouth, cheeks, and chin—we rely perhaps even more for indication of character.

In the expression of the higher emotions, the most important factors are undoubtedly the eyes and the mouth, or, to be more exact, the muscular integuments surrounding these features, which, in accordance with the three principles enunciated by Darwin, control the size and mould the shape of the ocular and oral orifices.

An important factor in the æsthetic import of the lower face is the impressive part played by the mouth in the final delivery of the voice. The shape of the jaws, the fitness of the muscles, the fulness of the lips and cheeks, and the contour of the oral orifice, are all valuable features, and all depend on good development of the dental arches. It is when the mouth opens, however, that the æsthetic value of the lower face becomes most striking. The importance of the teeth in this connection is remarkable, and it is during their display that the face is most animated and attracts most attention.

Concerning the part played by the upper and middle face in character-indication much has been written and sung. Let us consider for a moment how the lower face may be assumed to have acquired its significance as an indicator of character.

Powerful, well-formed organs of nutrition would suggest a well-developed, powerful, healthy body, and the mental contemplation of such a body involves that of the *mens sana in corpore sano*. Firmness and strength of character are generally supposed to be indicated by well-developed jaws and powerful mandibular and cheek muscles. Weakness of character and mental feebleness are usually associated in people's minds with poorly developed jaws, flaccid cheeks and lips, drooping and open mouth, and receding chin. That is to say, efficiency of the masticatory apparatus indicates efficiency of the nutritive system generally, and consequently good development of body and mind; and so leads on to the impression of "character."

Another direction in which we may seek for the significance of the lower face as a character-indicator is in the phylogeny of the masticatory apparatus. In the earlier history of the race this apparatus was largely used in combat, and the man who made most impression on his fellows in those days must have been to a considerable extent he who possessed the best developed dental armature and the greatest skill in its use. The idea engraved on our racial memory by powerful and well-developed jaws and teeth, therefore, would be one of virility and strength of purpose. The women of those times,

one supposes, would not require to use their teeth as weapons to anything like the same extent, and from this distinction has possibly grown the admiration of less powerfully developed masticatory organs in the " weaker sex."

We have so far considered facial beauty as the outward and visible sign of respiratory, alimentary, and other organic efficiency, as an indicator of character, and as a medium of worthy expression.

Beauty of this class, or anatomical beauty, as it may be termed, may be as faultless as is possible, and yet fall short of many particular instances which will readily occur to any-one—is, in fact, on a different plane from the beauty that makes a special appeal, the beauty that thrills.

In considering this higher quality of beauty, or " loveliness," we feel that these explanations are inadequate, that even when the conditions demanded are fulfilled, the fundamental biological significance of such beauty is yet to be sought. As ordinary beauty has its significance, so surely there is an even deeper meaning in transcendent beauty.

We may suppose the idea of the handsome or lovely face to be arrived at by enhancement of the lines and contours suggestive of anatomical perfection or organic efficiency—a progressive accentuation of the differences between the face with ill-formed and badly balanced features and the well-constructed and properly proportioned face: and we may imagine this evolution of the ordinarily beautiful face on enhanced lines being brought about by sexual selection, and helping to mould, as well as being moulded by, the human ideal.

Darwin has pointed out that the appreciation of *ordinary* female beauty by the men of various races tends to accentuate the racial type, but he cites Sir Richard Burton as believing that a woman whom Europeans consider *exceptionally* beautiful is admired throughout the world.

The contemplation of beauty of this kind, involving certain intensifications of well-formed and well-balanced features, certain delicate modelling and especially graceful contours, strongly appealing to both æsthetic and sexual emotions, naturally tends to impress one of the opposite sex with the eminent desirableness of the being exhibiting it. Entrancing beauty, or " loveliness," however, does not often characterise the hypersensual or wanton physiognomy. There is always an element of purity, even something sacred, in extreme beauty.

In modern communities the workings of sexual selection must necessarily be intricate, and the development of any particular type continually swamped. This fact may be held to account for the unfortunate rarity of beautiful persons. But

sexual selection is undoubtedly at work, and here and there its operation will be manifest.

The possession, therefore, of such beauty as we have been considering would appear to be evidence of an heredity comprising the qualities that characterise the most desirable partners in life, as well as ability to exercise in each generation the selection of the fittest mates. It would consequently imply the promise of reproductive success in large measure. Conversely, ugliness or unattractiveness would take what might seem its natural office as an indication of but moderate fitness for the most eugenic union.

Evidence as to the facts might be obtainable by Galtonian methods.

May we not, then, conclude, with regard to the significance of this order of beauty, that it should indicate capacity for the highest fulfilment of the sexual life; that, as ordinary beauty is an index of a well-formed and well-functioning body, so an amplified and exceptional beauty is of the most profound biological and psychical significance—Nature's symbol of perfect parenthood?

Rossetti, in one of his wonderful sonnets, gives expression to his sense of the profound import of transcendent beauty:

> "Beauty like hers is genius. Not the call
> Of Homer's or of Dante's heart sublime,—
> Not Michael's hand furrowing the zones of time,—
> Is more with compassed mysteries musical:
>
> . . . This sovereign face, whose love-spell breathes
> Even from its shadowed contour on the wall."

POPULAR SCIENCE

THE EVOLUTION OF MAN AND HIS MIND

By MAJOR THOMAS CHERRY, A.A.M.C., M.D., M.S.
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Speculations in regard to the Origin of Man are always interesting on account of the personal equation. We all like to know as much as we can about the line of one's own ancestry. These theories are of increasing importance because with the spread of education man's conduct is becoming more and more influenced by his thought. Democratic public opinion decides the line of action of the nation, and things learnt in youth constitute one factor at least in the moulding of public opinion. There can be little doubt that the general popular acceptance of the crude struggle for existence was one thought which went far towards unifying German policy before the war. And this simple faith we are now told is not warranted by the facts of science. The current doctrine, " extinction of the less fit, and survival of the fittest, no longer commands the universal assent of zoologists. Indeed it has been seriously undermined by the discoveries of recent years." [1] In other words, while the continuity of the protoplasm and the advance of the type has been preserved, the means by which this has been brought about are probably more complex than the simple factors put forward by Darwin and Wallace.

In regard to the special case of the evolution of man, my object is to show that recent advances in knowledge have introduced new difficulties both on the side of structure and of function, and have made untenable the current theory of the comparatively recent separation of the human and the ape stocks. I shall then try to picture the forces and environment which seem to be the most probable causes of the evolution of progressive man.

Difficulties of the Current Theory

1. Taking two examples of structure, the premaxilla and the foot, there is no doubt that the problem of the premaxilla

[1] Bourne, in *Animal Life and Human Progress*, 1919, p. 56.

is one of the most difficult points in any theory of the descent of man, and its significance is probably commensurate with its difficulty. The following are the salient facts. Man differs from all the other mammalia in the fact that the upper incisor teeth are carried by the maxilla instead of by a separate bone named the inter- or premaxilla, lying below or in front of the nose. This bone is well marked as a separate entity in all the lemurs, monkeys, and apes. In the chimpanzee and orang it unites with the maxilla sooner or later in the adult life of these apes, and the suture between the bones is obliterated. On the bony surface of the human palate a line is usually to be found which was formerly taken as indicating a margin of the premaxilla, but some authorities now consider it doubtful if this line has any such significance. At all events one of the earliest centres of ossification in the fœtus appears at the sixth week in the maxilla near the site of the future canine teeth, and from this the portion of the bone which will carry the incisor teeth is laid down during the following month apparently by direct extension from this original centre. The details of the formation of this part of the skull have perhaps not yet been completely worked out, but it seems certain that all traces of the premaxilla—if such ever existed apart from the maxilla—are very speedily lost. So absolutely unique is this disappearance of the bone, that it makes the human embryo distinguishable from that of all other animals at a time when the foot has the shape of the generalised reptilian type; the five digits are arranged like a fan and equally spaced apart, and the cartilage of the little toe is quite as large as that of the great toe.[1] But according to the recapitulation theory, that the embryological life of the individual gives a summary of his ancestry, the disappearance of the premaxilla so very early in fœtal life would indicate (*a*) that the pre-human stock had lost the bone, and therefore had already separated from the apes at an immensely distant epoch; or (*b*) that man has been able in recent times to get rid of inherited structures which he did not require; or (*c*) that the common ancestor had a small premaxilla which has become proportionately more prominent in the apes and less so in man since the separation of the two stocks.

Light may be thrown on this problem by the parallel case of the mammalian lower jaw. This bone is ossified in membrane from one centre for each half, any little areas of cartilage being probably the remains of Meckel's cartilage. In the reptilian ancestors of the mammals each half "ought" to have been composed of at least three bones. These, however, have become completely fused and interlocked, so that no trace

[1] "Digit" and "toe" are used to include the metatarsals.

can be found of the separate constituents, which should have presumably remained as vestiges of the reptilian type. "Meckel's cartilage atrophies in its greater part, but its front part, taken into the bone, is ossified, and probably therefore represented in the bone between the mental foramen and the symphysis, and is possibly responsible for the prominence of the chin."[1] The lower jaw is also instructive, as from it a far-reaching principle may be deduced in regard to the late evolution of the bones. The Heidelberg and other very early human jaws are large, thick, and clumsy. In particular the sigmoid notch is nearly absent. But it is not probable that the presence of this notch and of a light "elegant" bone would of itself be a character of survival-value. We here probably meet some force which is present in most surviving mammals, but which was not so marked in extinct ones. This force makes for economy in the substance of the bone, with the distribution of the material in such a way that the maximum of strength is attained in any required direction. We shall see that in the case of man economy in bony structures has been correlated with the special growth of the brain. Absence of present-day markings on the jaw for the attachment of muscles may be a character of doubtful value, as an indication of the absence of the power of speech.

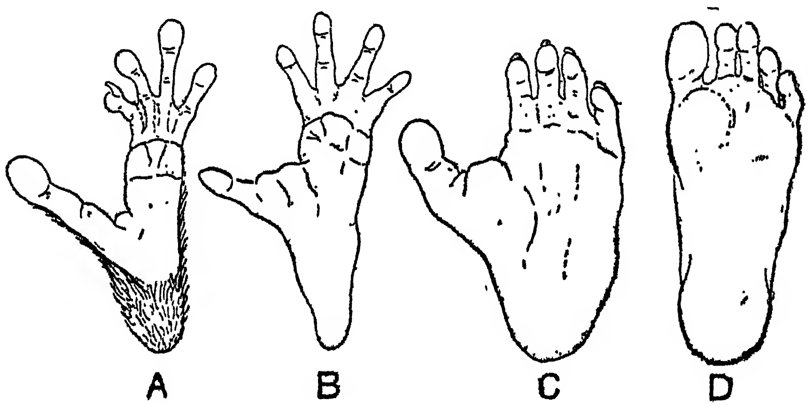
A similar story is unfolded in the embryological history of the tail in man and the apes. The scaffolding for at least six separate bones is faithfully laid down in cartilage, such being apparently the length of the tail in the primitive ancestor. The outline of the first four is seen in the coccyx, but the last two are so small that they disappear by being absorbed into the fourth segment. On the current theory of vestiges it would appear that, as the premaxilla has disappeared more completely than the tail, it was the earlier of these two structures to be lost by the human stock.

2. The foot has always been recognised as a special feature which played an essential part in establishing the mental superiority of man. The older anatomists looked upon the upright gait as the indication of a gap sufficient to justify the definition of a special Order, Bimana, for man alone. But since the death of Owen man has been grudged even a Family for himself. Current opinion is thus stated by Pocock: "The moment we get to the apes we see a progressive series of modifications, beginning with the gibbon, ending with the chimpanzee or gorilla, attesting most clearly the stages of the evolution of the human foot from the ape type."[2] Here the genetic relationship is inferred, and the progressive series is

[1] Frazer, *Anatomy of the Human Skeleton*, 1914, p. 250.
[2] Pocock, in *Conquest*, February 1920.

assumed to have begun with an opposable great toe and to have ended in the distinctive human great toe, which is the corner-stone of the foot. It is possible, however, that the opposability of the simian great toe may be a secondary character, and that the common ancestor may have had a clasping foot.

Pocock proceeds to discuss the monkey hand, foot, and method of locomotion in the trees and on the ground, and points out the fundamental differences between these and the corresponding organs and functions of the gibbon. He concludes that the arboreal and terrestrial habits have been learnt



A. Foot of the bipedal arboreal lemur.
B. Foot of the quadrupedal arboreal monkey
C. Foot of the gorilla, a bipedal, partly arboreal ape
D. Foot of man.

FIG. 1.

independently by the monkeys and the apes; that "since ancestral traits lost in the adult often persist for a longer or shorter time during youth, we may conclude that the orang's progenitor was biped before the adjustment of the organisation for climbing had reached the pitch exhibited by the existing ape"; and that the short broad foot of the chimpanzee and gorilla, "their ability to stand and walk erect, and their peculiar way of climbing, all point to the conclusion that they are descended not from a truly arboreal ape, but from an ape which had already taken to terrestrial life." If the specialised monkey foot may thus be ruled out as a stage in the ancestry of man, the question arises, What evidence is there as to the condition of the great toe in the primitive primate?

At the sixth week the human embryo has the fan-shaped

reptilian foot already mentioned; a week later the great toe has begun to approximate to the line of axis of the middle toe; and in the ninth week this digit has assumed its commanding position and lies nearly parallel with the cartilage of the middle toe. The characteristic human tarsal arch is apparent a week or two later. There is thus no indication of the assumed course of evolution from a foot with the great toe at right angles to the line of the other toes. The transition from the reptilian to the human type proceeds direct without retracing any step.

A feature which has been prominent in the foot since very early times is the styloid process of the fifth metatarsal bone. This bone is specially associated with the lesser peronei muscles,

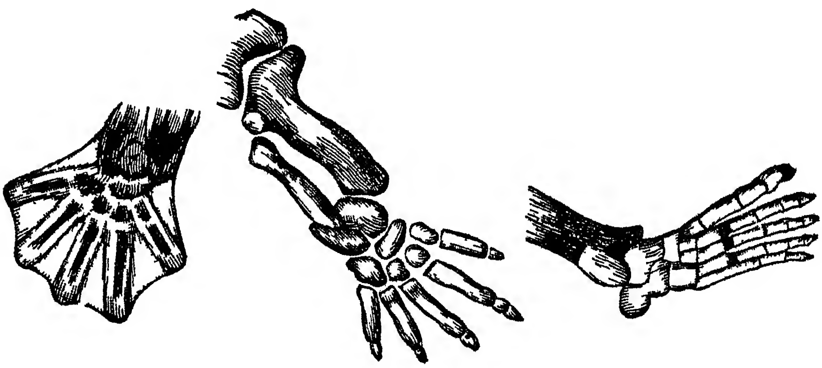


Fig. 2.

Development of Right Foot at 5, 6, and 9 weeks Length of Embryo—$\frac{1}{2}$ in., $\frac{3}{4}$ in., and 2 in. From Keibel and Mall.

the third of which is an exclusive human possession and one that appears early in fœtal life. The styloid process is important in many marsupials, and was of very large size in the extinct diprotodon. It is mentioned here as an example of the tenacity with which cell-memory holds on to useful structures and often exploits them for new uses. In man the styloid process sometimes asserts its lineage by having a separate centre of ossification for itself.

We do not know what was the shape of the foot of Anaptomorphus or other very early primate. Phenacodus primævus of the Lower Eocene was near the ancestral line of all the ungulates, and therefore the three middle toes had already become far enough specialised to commit its progeny to the choice of hoofs. From Cope's figure it will be seen that the tarsal bones and the great and little toes are suggestive of the origin of the feet of all the lemurs, monkeys, apes, and man

from such a type. In the one direction the great toe might be made movable and opposable as it is in most cases; or it might be lost completely or in part; or it might grow and keep close to the toes, and so assume the human type. But in accordance with Dollo's generalisation on the irreversibility of the course of evolution it is unlikely that the type should change from rigidity in the ancestral ground ape to mobility in the gorilla, and back again to rigidity in man. Life on the ground

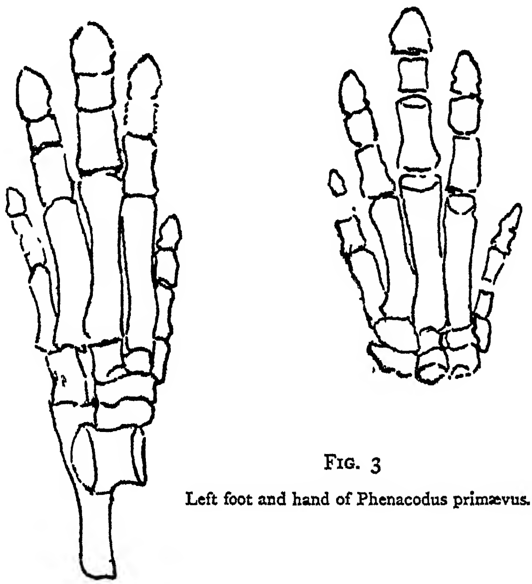


FIG. 3

Left foot and hand of Phenacodus primævus.

would be as fatal to a projecting great toe as it is assumed to have been to the ancestral tail.

3. Passing now from structure to function we select instinct as a well-marked line of demarcation between man and the apes. Its absence in man is correlated with the great brain and with the absence of any structures elaborated into weapons of attack or defence, such as the teeth of the gorilla or the ears of the chimpanzee. By some means the brain of man has, during its period of super-growth, been able to *forget* the impressions which it inherited from the pre-human stage of existence. The cells of the embryonic tail and vermiform appendix still faithfully appear as they have done for millions of years. But the cells of the brain have changed and are different from their ancestors' perhaps only a few hundred thousand years ago. This is another special human phenomenon.

While it is probable that instinct in birds and mammals was not as well marked in the Eocene as it is at the present

day, it is certain that instinct was developed earlier than intelligence. In the case of the human brain the growth of intelligence was incompatible with the persistence of instinct, and therefore the cells of the brain which inherited instinct have disappeared as completely as those of the premaxilla. To take an example. The survival of the mammalia is dependent upon the mother knowing, as soon as the first emergency arises, the proper way to dispose of the umbilical cord and placenta. A primitive mammalian instinct has been universally inherited in regard to this matter. But in the case of modern woman this instinctive knowledge has completely disappeared, and one is safe in saying that the baby knows more about the mother than the mother about the child. In this case a few thousand years of tradition and teaching have been sufficient to eliminate one of the most fundamental instincts. The most probable explanation seems to be that the type of brain which retained the primitive instinct has died out.

Instinct appears to be less marked in the great apes than it is in the baboon, and none of the primates have the marvellous instincts of the beaver or the penguin. The chimpanzee is terrified by lions, tigers, and bears, but learns to throw biscuits into the mouth of the hippo from a safe distance. It has also a well-marked building instinct. All the primates except man seem to dread the snake. An Indian gibbon (*hylobates agilis*) "sings in perfectly pure and melodious notes up and down the scale of an octave, the distance between the notes being exactly half a note."[1] This, however, is not a musical faculty but camouflage, as all animals in the jungle are instinctively afraid of thunder or any loud noise, as they infer that the sound is proportional to the size of its author.

All these examples of instinct represent the inheritance of things of survival-value. Man has none of them, and it is probable that the gorilla and orang are in the danger-zone between instinct and intelligence. The bones of the arms and legs of old males often exhibit the signs of former fractures in positions which make it probable that they have been caused by falling out of trees. These apes are slow and cautious climbers, and the orang is said to test a branch before venturing upon it. But they appear to make mistakes occasionally. Now instinct never makes a mistake. Hence these apes may have been too intelligent to acquire new tree-climbing instincts at the time when they took to the trees as the descendants of the ground ape. On the other hand, the great air-pouches of the orang and his deafening roar, like that of the panther, are indications of the inheritance of structure and function similar to that of the gibbons.

[1] Haeckel, *History of Creation*, ii. 408.

4. Our next example of a special human function is that of the growth and metabolism of the body. In 1908 Rubner showed that man differs in a remarkable way from all the other animals with which he experimented—horses, cattle, sheep, pigs, dogs, cats, rabbits, and guinea-pigs. He arrived at the following three conclusions : (*a*) During the early period of growth of an animal the total amount of energy supplied by the food is the same per unit of body-weight for all animals except man. Each kilogram of weight requires for its production food-energy equal to 4,800 calories. Man requires six times as much, or 29,000 calories per kilogram. These results are independent of the time taken to double the body-weight. (*b*) In all these mammals the same fractional part of the total food-energy, namely 34 per cent., is utilised for purposes of growth. But in man this fraction is found to be on the average only 5 per cent. (*c*) The energy-value of the food consumed per kilogram of body-weight during the period of maturity and old age averages 191,000 calories for all the other animals, and 725,000 calories in the case of man. His age-limit was calculated as eighty years. That is, man requires more than four times the average of these animals per unit of body-weight.

So far as I know, experiments have not been carried out with any of the monkeys to see if they form a series leading up to the singular position assigned to man. But in any case it would appear that the unique brain and other distinctive characters of man have not been achieved by exactly the same forces as those of all the other mammalia. In all other animals evolution appears to have run a uniform course and to have brought forth uniform results, but man's metabolism appears to be a special case.

When did the Man-ape Stock Separate ?

The current theory as to the later separation of the human stock from the large ground apes is untenable for the following reasons. A large animal—large for its own order, as the chimpanzee among the primates—has always become specialised ; special organs imply corresponding instincts, for instinct and organ go together. It is always assumed that disuse can lead to reduction in size, but as we shall see, it is very questionable if a useless organ ever atrophies and disappears unless it is a handicap to the survival of the animal. And if the animal has acquired the organ and the mechanism and instinct to use it, how can he reverse the experience of millions of years and take to a new experiment ? For example, the baboons have efficient canine teeth and know how to use them against the dogs of the hunters. They also occasionally make use of sticks

and stones. Is it conceivable that they will ever lose faith in their teeth and take to either sticks or stones? The criterion of success in the struggle for existence is survival, and therefore unsuccessful experimenters always die out. Hence the advance of mammalian life is clearly the triumph of specialisation - the generalised forms in the Eocene were all small animals. When once a species is committed to any line of evolution—special teeth or special legs—it must make a success along that line or else it will become extinct. It cannot retrace its steps and begin again. Hence Dollo, of Brussels, has propounded his law of the irreversibility of the course of evolution.

The one outstanding feature in the case of man is his brain. On the theory that man separated from the chimpanzee and gorilla when they were about the size of the smaller of these apes, we are faced with this problem. Man has made little addition to the body-weight of the common ancestor, while his brain has increased threefold and the canines and bones of the face have been reduced in size. No cause can be assigned for the overgrowth of the brain. In the parallel history of the horse the disappearance of the lateral digits has been correlated with life on the hard stony plains which made a split hoof a great handicap. There has been at the same time the elaboration of a digestive system suited to hard dry food, and the canines have atrophied because they were a handicap in preventing the lateral movement of the lower jaw in grinding the hard food. During the millions of years—Eocene to Pliocene—occupied in bringing about these changes, the direction of the progress of the evolution of the horse has never changed, and the weight of the animal has increased fifty-fold since he had five toes.

The chimpanzee falls in line with the horse in the growth of the body, of the canine teeth, and probably in the size of the ears, as compared with the apes of the Oligo-miocene. These characters are all of manifest survival-value, and they have all proceeded in the one direction. Both the horse and ape illustrate Dollo's law.

On the theory of the recent separation of man and ape the anomaly of the ribs and backbone is another mystery. Man has seventeen dorso-lumbar vertebræ and twelve ribs. The gibbon, gorilla and chimpanzee have seventeen vertebræ and thirteen ribs, the orang sixteen vertebræ and twelve ribs. It is probable that the thirteenth rib is a character of direct survival-value. Primitive races of mankind are like the apes in possessing no waist, but they have only twelve ribs. The thirteenth is occasionally found in man, but only as a rare curiosity. Presumably then the recent common ancestor had thirteen ribs and seventeen dorso-lumbar vertebræ. The rib

may be a trifling loss, but it is a variation, nevertheless, in the wrong direction as regards survival. It is difficult to believe that the offspring of Palæopithecus or of any large ape of the Pliocene could have varied in three directions, and yet have ever afterwards bred true in regard to such basal parts of the skeleton as the ribs and spine; and to add to the enigma, Flower and Lydekker regard the brain of the orang as being nearest to that of man.

Reconstruction—The Lower Eocene

Starting with a generalised primate of the Anaptomorphus type, we have an animal about as large as a rat. It had a large brain and very large eyes. It is inferred that this animal lived in the trees and had clasping feet, perhaps with partially opposable thumbs. What part of the tree did it frequent and what was its object in going to the trees? An opposable thumb as part of a very small hand is seen in some marsupial forms not much larger than a rat. These live during the night on the small twigs and branches amongst the leaves, where the diameter of the part of the plant supporting them is small enough to be at least partially encircled by the hand. As the large eyes indicate nocturnal habits, this proto-primate may have caught and eaten birds roosting in the small branches. As it depended on sight and not on scent, the moonlight made the difference between full and scanty supplies of food. Hence the monthly cycle was established in the female, and this has persisted in all the descendants of this proto-primate, including the lemurs. The effect of special food has been stamped on all these animals.

Some other inferences become probable. Anaptomorphus has small and even teeth and a short face. This type of face has persisted in some South American monkeys, and it is easier to picture a short nose and muzzle growing long than the opposite trend of development. For instance, in the case of dogs the breeds with a very short muzzle appear to be rather at a disadvantage. The general type of the alimentary canal in the primates is another important point. This more resembles that of the carnivora than that of the herbivora, and the size and structure of each type is related to the percentage of nitrogen in the food-supply. The proto-primate therefore secured a diet rich in nitrogen, almost like a carnivore, and the normal pattern of the hand and foot suggest that the diet was not insects, spiders, and grubs, for if these were the original food of the arboreal primates one should expect to see many modifications of the teeth and fingers similar to those found

in the aye-aye. Still it was probably insects that first attracted them into the trees in search of nitrogen.

As for obtaining nitrogen direct from leaves and nuts it is probable that these parts of the plant were not so rich in nitrogen in the Eocene as they are at present. Arboreal forms, such as squirrels, lemurs, and monkeys, eat birds, mice, and eggs, a habit which shows that the protein problem is not easily solved on a purely vegetable diet; and the primate type of the alimentary canal combined with the large eye points to birds and not to leaves and seeds. It may be that the attacks of such small animals brought about the roosting instinct of many birds, to which reference will be made at a later stage. It is also of interest to note that at the present day some of the lemurs are tamed and employed to catch birds in the trees at night.

The Upper Eocene Change

It was probably during the Eocene that the lemurs, New World monkeys, and Old World monkeys separated from one another. The profound differences between the three indicate that the proto-primates were still very generalised at the time when this separation took place. For the most part the descendants of all three branches have remained small and arboreal. They have each developed their own type of teeth, of hands, of the feet and of the tail. Concurrent with the evolution of these organs each of these branches developed instincts along its own special lines. Proto-primate had probably eleven ribs and some seven or eight lumbar vertebræ, and hence his descendants have been able to add to the ribs and to adjust the lumbar and sacral vertebræ according to the needs of bony protection, and of strength or flexibility of the backbone.

It was probably in the Upper Eocene that the branch of the Old World monkeys which gave rise to the apes and man came to the ground. Perhaps it had grown too heavy for the small branches of the trees, and birds had become too wary to be caught as easily as of old. This branch had peculiarities which already marked it off from most of the monkeys, and had retained many primitive features. It had small conical canine teeth, small bones formed the capsule of the olfactory nerve, a rounded head perhaps resembling that of the young chimpanzee, a vermiform appendix, and probably a very short tail. The relative length of the arms and legs was perhaps the same as has been retained by the chimpanzee; let us suppose that it had an opposable thumb and a clasping foot, the condition of the two extremities being the opposite of that found in the American monkeys.

The five existing genera descended from this common ancestor—man and the four apes—have retained many very primitive anatomical features. It is impossible to strike a balance and say that on the whole the gibbon, because more monkey-like, is more primitive than man. The significance of the primitive features cannot be weighed and measured; we cannot say that an ounce of brain is equal to a pound of bone. But it appears to be the soundest principle to regard all apparent reversions as sign of survival unless there is clear evidence to the contrary.

The common ancestor is usually supposed to have taken to a life on the ground and there acquired a fair approximation to a bipedal gait. In this case his diet would probably have consisted of lizards, grubs, and scorpions, and the miscellaneous articles collected by baboons. It was evidently of animal origin to a great extent, because the digestive organs continued to be small, and none of his descendants except man have developed the side-to-side movement of the jaw in the act of chewing. This is a fairly constant indication of a vegetable diet in the case of all mammals.

The plan of the digestive system seems to be an indicator of great importance in tracing out the early history of any species of animal. In man the alimentary canal is nine times the length of the trunk, in which relation he stands intermediate between the carnivora and herbivora. The corresponding ratios are: cat four, dog five, horse twelve, ox twenty, and sheep twenty-six. Many monkeys and the apes (except the orang) have provided for the increased size of their digestive organs by securing thirteen ribs. Even with this support they have protuberant abdomens, no waist, and in the gorilla the cæcum and vermiform appendix are pushed deep into the cavity of the true pelvis. There is a species of lagothrix monkey on the Amazon called "barrel-belly" by the Portuguese on account of the immense amount of fruit that it eats. It is not probable that nuts and fruit or even seeds and grasses were so rich in protein in the Pliocene as they are now, because the evolution of nutritious plants has been related to the increasing richness of the surface soil, and this has been the joint work of the animal and plant. In some parts of Australia the vegetation is comparatively ancient in type and absence of water has restricted the marsupial animals. We therefore find very little difference in regard to the percentage of plant food in the surface soil as compared with the subsoil; whereas the rule is, in temperate regions, for the surface foot of soil to contain from two to three times as high a percentage of nitrogen and phosphoric acid as is found in the deeper parts. On some of the Australian gold-fields the surface soil and the rock four thousand feet below the

surface contain the same amount of phosphoric acid. Rich soil has produced highly nutritious food, and conversely rich food produces fertile land. Man's brain was the product of concentrated food—without which children are soon in difficulties.

We have to account for the action of forces which made steadily in the direction of the human hand and foot, the growth of the brain, the non-development of the special senses and the obliteration of instinct. Two manifest conditions of the environment were abundant nitrogenous food and safety. We may further assume that in order to find the food it was an advantage to assume the upright position, and that intelligence was of more value in selecting the food than keen powers of smell or great teeth. Pocock has demonstrated the extreme probability that the apes had learned to walk fairly well before they took to the trees a second time. Their object in attempting to perfect the bipedal gait was presumably to set free their hands while searching for food. This food was not berries on small bushes, or otherwise this diet would have left its mark on the teeth and the digestive system. When the common ancestor first came down from the trees he would scamper off on all-fours the same as the monkeys do, and the search for food amongst rocks and under stones seems to have led to the baboon type rather than to the anthropoid. That is, this animal must have always kept one side of its brain alert for possible carnivorous neighbours while it was looking for food. As the apes stand between the baboons and man it is probable that the forces which produced the ape type with its human characteristics continued to act for a longer period on one branch of the common stock and so brought about the evolution of man. In this sense let us assume that the apes left the common ancestral home in succession and began the secondary changes which have made them what they are.

Life on the Seashore

Now, the conditions of safety from the carnivora and a perpetual supply of highly nitrogenous food can only be found combined on the seashore. Carnivorous animals track their prey chiefly by scent, and herein lies the safety of the shore, for the scent vanishes as soon as the sand has been washed by the next wave. Generally speaking, there is a stretch of dry sand between the water and the grass or trees, so that the beach is not frequented by the herbivora. Leopards sometimes search the sand between the tide-marks for dead fish, but they do not find much because the birds and crabs are always waiting for anything that may come ashore. Marsupial

carnivora also visit the sands, and the kangaroo has been known to swim across an arm of the sea two miles in width. Monkeys in Sierra Leone sometimes visit the shore and eat the oysters,[1] and baboons make use of sticks as levers to raise stones in their search for food, and of stones wherewith to crack nuts. The ease with which the apes are taught to use knives and simple tools, and the readiness with which they learn to master the secret of the lock in order to let themselves out of their cages, bespeak their knowledge of external things. It is therefore in no way improbable that one family of the Late Eocene primates may have chanced on some part of the beach where shellfish are plentiful and there begun a course of evolution different from all their former associates in the trees. The epoch-making difference was the fact that they could live in safety and plenty on the sand and not trouble about going back to the trees at night. The necessary quota of carbohydrates in the diet might have been obtained from seakale and other plants which grow close to the beach.

On such a beach as that at Port Said or Alexandria there would be no difficulty in a man finding sufficient to eat. The sand-crabs are very numerous, but they are not easy to catch because they are so quick-sighted. Still, it may have been their movements which first attracted the attention of our hypothetical monkey. These crabs are on good terms with the small sea-birds with whom they dispute for the possession of anything that is eatable. A monkey would perhaps frighten both away and pick up the morsel. At all events he might have found sustenance on the beach without even using a stone to crack a shellfish. On this hypothesis an improvement in the powers of observation and imitation would be of greater survival-value than a lengthening of the nose or of the canine teeth. In other words, as soon as a monkey began to live on the sandy shore, survival would depend on the growth and elaboration of the brain. The cranial nerves were all already sufficient for their new uses, but new centres were required in the brain for functions which lead direct to the brain of man.

Next we have to account for the improvement of the hand, the broadening of the sole of the monkey foot and the gradual approximation to the upright posture. These are all provided for by the extension of the food-producing area to a rocky beach. The power to identify stranded mussels on the sand would lead to the recognition of living specimens attached to the rocks. Oysters and other shellfish would soon be added to the list. Stones would be used for cracking the shells and sticks for levering them off the rocks. In this way the use of tools began. We need not therefore be surprised if the

[1] Scott Elliot, *Prehistoric Man*, 1915, p. 28.

Oligocene eoliths found in Belgium prove to be manufactured articles. A small animal seeking its food amongst the rocks between the tide-lines would be compelled to try to stand on its hind legs in order to use a pebble at first with both its hands. As it grew larger and firmer on its feet, one hand became strong enough to do the work. As the upright position gradually asserted itself, it was an advantage in searching the sides of the rocks to have the axis of the eyes at right angles to the perpendicular trunk. Hence the occipital foramen gradually reached the centre of the base of the skull, and its plane became nearly horizontal. The most successful of these dwellers on the beach were those that learnt to walk upright. Provided the little animals were not hunted by enemies, they had no need to scamper away on all-fours, and the type of body gradually changed until they became bipeds. Balancing the body and the new uses of the hand and foot were muscular functions that advanced the type of brain. The smaller the animal that began this series of changes the easier it is to account for the results, and it is clear that such a life was more varied and complicated than the old one in the trees.

At whatever point therefore in such a history the ancestors of the apes left the seaside and went back to the forest, they were at once exposed more fully to the stress of the ordinary struggle for existence. The earlier they went back the less they resembled the human type in the matter of the foot and head As Propliopithecus and other apes of the Egyptian Late Oligocene appear to be early gibbons, and as tree-living gibbons are found in the Miocene, this genus seems to have been the first to return to the trees. The large canines, long fingers and arms, long toes, and great toe at right angles to the foot are modifications that have been elaborated since the Oligocene, for the Egyptian fossils have comparatively small teeth. The next emigrant was the orang. Some of the families that left at that time may have been slim creatures that have died out, but one of them was prepotent in regard to immense strength combined with a short spinal column, and these two features have survived. Last of the surviving apes was the common ancestor of the gorilla and chimpanzee. As we have already pointed out, he carried away the human spine, and the need of protection has modified his ribs in two ways: they are broader and stronger than those of man, and the thirteenth has become a permanent addition to the chest.

Concerning the Teeth

The orang and gorilla make great use of their canine teeth in obtaining their food. These teeth are not so specialised as

those of the baboon, macaque, or gibbon, but they are very formidable weapons. They seem to have been developed as implements for obtaining food. The gorilla eats the kernel of nuts with very hard shells, and gnaws great pieces out of the trunks of trees in order to get access to the pith. "Here is probably one purpose of that enormous strength of jaw which long seemed to me to be thrown away on a non-carnivorous animal. These habits account for the great canines becoming worn, as they are in almost all adult gorillas." Hence the great bony ridges on the skull, the early closing of the sutures, and the large spinous processes of the vertebræ of the neck. In order to wrench off the pieces of wood the immense muscles have been developed and lead to "the almost total absence of the neck which gives the head the appearance of being set into the shoulders."[1] The older the fossil of the ape the smaller are the teeth and bony ridges in proportion to the size of the animal, Propliopithecus having small teeth of uniform height approaching in appearance the human series. Moreover, the permanent canines of the great apes are guided into their position by the milk canines, whereas the human canines do not appear until the eleventh or twelfth year. In the meantime our permanent incisors and bicuspids have long been in position, and the canine is often crowded out of its proper place and forced to fall out of line. "Back teeth" are therefore not evidence of our simian ancestry, but on the contrary quite the opposite.

The junction of the premaxillary bone and the maxilla has been the favourite site for the appearance of great teeth ever since the theriodont reptiles lived in the Permian period. The growing and ossifying tissues along the edges of the two bones are the parts most richly supplied with blood. This fact, in conjunction with the intermittent pressure caused by biting, appears to be the explanation of the commanding size of the teeth which grow next the suture. These are the canines in most cases, but the lateral incisor forms the tusks of the elephant, mammoth, and rhinoceros. With the exception of man, all the primates have specialised in the canines, and the greatest of all is the gorilla.

These considerations enable us to offer a probable solution of the history of the premaxilla. In the early primate, when he first came from the trees, it was a small bone, perhaps less than half an inch across. The structure which was of service to the gibbon, orang, and gorilla was not this bone, *but the suture with its special blood-supply*, which separated it from the maxilla. Those apes survived which had the largest canine teeth, and the line of the suture was essential to the increase

[1] Du Chaillu, *Equatorial Africa*, 1890, pp. 272 *et seq*.

in size of these teeth. Hence the premaxilla enlarged with the growth of these teeth because it held in trust the necessary blood-supply to ensure their growth. On the other hand, in the case of man the bone remained at its original size because man's canines have never been very large. In the chimpanzee the canines are much smaller than those of the gorilla, and the suture, having served its purpose, disappears as this animal becomes adult.

The apes have not made the same success of their canines as the baboon has done, perhaps because they began to specialise too late in the history of the species. A broken orang tooth has been found in the Pliocene, and many large orangs and gorillas break their teeth. This is not done by fighting, but by honest hard work to obtain their means of subsistence from the nuts and trunks of trees. Man has become lord of all because he never had great canines. The apes are rapidly becoming extinct because they developed theirs too late.

After the migration of the gorilla it would appear that no other emigrant was able to make good his footing in the forest or on the plains until the evolution of *Homo sapius*. Some of them tried to do so, and it is probable that many remains corresponding to Pithecanthropus will gradually be found. Such failures are not steps in the evolution of modern man, but races which left the seashore too late to develop specialised organs of attack and defence, and too early to survive through their superior brain-power alone. Eoanthropus and Neandertal man also wandered away from the safety of the shore too early. Such races may have each been struggling against the wild beasts and climatic conditions for an average of a hundred thousand years before they became extinct. It is clear that many modifications of structure may have resulted, and on the whole these modifications would be in the direction of the chimpanzee—the youngest of their cousins which successfully trod the same path. It follows that no stage in the ancestry of man may have been very like either one or other of these extinct races.

As the essential part of this theory is the evolution of man in safety at the seaside, we shall now give the evidence—negative and positive—in favour of this view.

Evidence for a Seaside Life

The negative evidence is the improbability of man's success anywhere else. When the common ancestor left the trees and became a ground ape he had only three types of locality open: the plains amongst the rocks, for there was little grass until the Oligocene; the scrub or jungle at the foot of the forest

trees ; and the seashore. The rocks appear to have impressed their conditions on the baboon and the Gibraltar monkey, and these are not steps in the evolution of the apes. Wallace thought the steppes were the scene, believing that " the seeds of indigenous cereals were present, and the numerous herbivora and game-birds would develop his skill as a hunter, trapper, and fisherman."[1] But as I have tried to show in a recent Paper contributed to the *Manchester Philosophical Society*, the first that we know about the cereals is the discovery of wild barley and millet by the proto-Egyptians in the Nile Valley less than six thousand years ago. Further, because the little primate had been able to catch birds asleep in the trees is no evidence that he could do so on the ground. To do so he would by analogy have been compelled to develop his sense of smell as well as canine teeth after the manner of the fox. As for fishing, nets and hooks or even traps are indications of the work of a brain higher than that found in any mammal except man, and the instinct of insects does not help us to solve the puzzle, for instinct is not the beginning of wisdom.

Next, the jungle where the gorilla lives to-day can hardly have been the environment in which the apes and man were evolved, and this for two reasons. First, the absence of nitrogenous food was more marked in the Oligocene than now, and even in the Australian scrub there is remarkably little to eat. The Tasmanian and Australian blacks seldom went into the heavy rainfall forests ; the Brazilian forest supports a human population only in selected localities. Presumably the end products, the orang, gorilla, and man, are better able to find food than their ancestor was millions of years ago. Secondly, the scrub country is unsafe. The three end products are compelled to defend themselves, and in particular all primates except man have developed an instinct against snakes, which first appear in the Miocene. As the common ancestor did not climb trees he would probably sleep in hollow logs at the foot of the trees. The recession of the sense of smell is against this hypothesis. Finally, if man was evolved in jungle country he would have had an age-long struggle against mosquitoes and similar forms, and therefore he would have become immune to malaria, trypanosomes, dysentery, and many allied diseases.

Returning now to the seashore, we have seen that it is marked by the two essentials, safety and abundant food. The sands have been exploited by no mammal, an anomalous fact, of which the probable explanation is the absence of movement and scent in shellfish. Their recognition as food therefore demanded some intelligence, and the way a primate, well

[1] *Darwinism*, 1889, p. 459.

developed by arboreal life, might have extended his knowledge from dead molluscs to the living ones has already been pointed out. This function, together with the new uses for the hand and foot, would further develop the brain, and thus the brain became the organ of survival-value. As the shellfish could not fight or run away, there was no call to develop great teeth, swift legs, or any other organs of attack and defence. The sands are clean, soft, and free from insects, so that the new primate did not evolve callosities, nor did he become immune to disease in the way that the lemurs and monkeys have done. Protoman may have cracked shells with a stone instead of using his teeth. Seashore food is nitrogenous, soft, nutritious, and requires little mastication. Man's third molar is decadent; human babies can digest oysters, but not bananas, coconuts or the cereals. All these are facts, and there may be a causal relationship between them.

Life on the seashore will also explain the loss of the hair. Man's skin is not a monkey's skin minus the hair. It is far better supplied with sweat glands, and man can thus survive a degree of exposure to the sun which is speedily fatal to a monkey. Man's naked skin is a conspicuous contrast to the condition of all the other primates. Darwin and Haeckel more or less cautiously attributed the change to sexual selection; other speculators, who wish to emphasise the ferocious beast theory, have assigned the change to natural selection in order to get rid of vermin. This is an unnecessary calumny, as no healthy ape or monkey harbours lice or fleas—their regular toilet is performed in order to pick out little flakes of dead epithelium. Among the most primitive races, the Tasmanian women used to wade and dive for shellfish, wrenching the oysters off the rocks under water by means of a short wooden chisel, and the shells were broken with a stone. The Fuegians use much the same methods. If we suppose that as protoman increased in stature, and so required more food, he did the same, the loss of the hair is easily explained. When a hairy animal comes out of the water the evaporation from the surface chills the body, especially in the wind; a naked animal dries much more quickly and is less affected by the cold. The cold body requires more food and is more liable to lung diseases. Hence in times of scarcity this selective action would act rapidly. The apes have a good coat of hair on their backs and heads, but much less on the front and inner aspects. This is no evidence against the theory of the seaside selective action, because two other allied animals diverged in a similar way—the elephant went naked and the mammoth developed a shaggy coat.

The primates are very subject to lung diseases. Most

monkeys in captivity, and some in the wild state,[1] die of broncho-pneumonia, which is often tuberculous. They seem to have failed to adjust the heart to the increased strain involved by the erect attitude, and the right side of that organ gives way. The same diseases are depopulating some of the South Sea Islands, where the natives, formerly naked, now wear linen, which chills them after it has been drenched by the tropical rains. The primates usually live in hollow trees, and some of them build shelters from the rain. Man lost his hair at a very early period, for the hair varies profoundly in the three great varieties of man. The hairy parts of the body are well supplied with sebaceous glands, and many races believe that the liberal use of grease prevents any further loss of hair.

A very long period at the seaside, during which our ancestors were slowly changing from protoman to man, is the best explanation of the accentuation of the monthly cycle in woman. This cycle probably began with the ancestral primate, but in some monkeys it is obscured by the fact that they have a definite breeding-season, usually twice a year. This longer cycle is a secondary phenomenon related to the annual food variations at the time of the rains. On a tidal shore similar variations occur every fortnight. As the rise and fall of "spring" tides is about three times as great as the difference between high and low water at "neap" tide, a far greater extent of rock and sand can then be searched for food. Moreover, the harvest from below the mean level of low water is much richer than that from the shallow depths exposed at neap tide. For instance, oysters are usually found below the two-fathom line, and are therefore more readily obtained at spring tide. The knowledge that they were there may have induced our ancestors to dive like the Tasmanian women. One week there may have been little to eat except plants; the following week there was a superabundance of food rich in protein. Turtles also lay their eggs by moonlight, and these may have increased the lunar feastings. Hence the new feasting reinforced a tradition which had been growing all through the Eocene. Dr. Marie Stopes believes that most women have a fortnightly rhythm, marked among other things by a special feeling of *bien être*. If this is so, the cause can hardly be referred to anything except the ebb and flow of the tides.

Another possible result of such a life at the seaside is the human shoulder-joint, which, from the point of view of the "fittest," is evolved beyond the proper limits. Movements are extremely free, the capsule is very loose, and consequently dislocation of the shoulder is several times more common than that of all the other joints taken together. After such an

[1] Darwin, *Descent of Man*, p. 7.

injury there may be a slight thickening of the lower part of the capsule. This limits the movements, especially in the effort to raise the elbow to a right angle with the body. The shoulder adds very greatly to the usefulness of the hand, but this joint was not produced by swinging clubs or throwing spears. The gibbon, which perhaps comes next to man in regard to the range of movement, has produced its joint by swinging by the hands from branch to branch. This is the regular mode of locomotion of this ape. Man may have produced his joint by swimming and diving, and if the seaside theory is accepted, these new movements were carried out in the search for food. Food-supply from the sea thus makes the brain of survival-value, establishes the upright posture, and causes the evolution of the foot and leg, the hand and arm. A correlated series of structures are thus evolved which became capable of carrying out the behests of the brain when that organ had grown great enough to think out other problems beyond those of mere survival.

The objection may be taken that this seaside theory should have made the human child able to swim by instinct. The reply is that human instincts have not developed since the period when the brain became the organ of survival-value. Nevertheless, in races which have kept up the habit of being much in the water, the children appear to swim without any special training. It is also true that the orang cannot swim, a fact that may be accounted for by supposing that he left the sands for the forest before swimming had become general.

The number of years covered by each of the tertiary periods is so great in comparison with the number of generations of each species of animal that the time-limit is long enough to account for the necessary structural changes. For instance, in a million years there would be far more than a hundred thousand generations. Suppose the height of the animal had increased in that time by fifty inches, the advance would be quite inappreciable, being only one two-thousandth of an inch in each generation. The problem is to find a set of factors which would account for a steady advance in the one direction. This problem seems to be easier of solution if the advance was begun when the animal was still comparatively small. The improbable thing to happen is the change of plan. If the needs of survival have produced great canines it is not likely that these can be surrendered and the instinct to use them forgotten. The animal then carries his own implements with him as part of his body. When such organs have become fully specialised the progress of the brain is stopped, because the animal has become a self-contained and self-sufficient organism.

So strongly did this idea appeal to the philosophic biologist

that Darwin remarks upon the advantages of man beginning as a weak animal, because it would compel him to remain social. Schoetensack claims that the necessary conditions of safety point to Australia as being the place of man's evolution ; and Klaatsch goes so far as to hold that the apes are degenerate branches of the pre-human stock, the way upward being cut off in particular by the reduction of the thumb. We have remarked that the gibbons have become best adapted for their new life because they left the sands while they were still small animals in the Oligocene. The other species have a precarious hold on continued survival because they faced the new conditions after their feet were too specialised to turn into first-class tree-climbing organs, and their teeth were too large to acquire the sharp posterior edge of the canines of the baboon.

Man Evolved in Safety

The evidence that man was evolved in conditions of comparative safety, so that the struggle was less severe than in the case of other animals, may be deduced from many points of structure and function. First of all, the removal of the need for continuous watchfulness may have set free the growing brain, allowing of the development of intelligence instead of instinct. The more rigorous the conditions of life the more rigid the instincts. Amongst vertebrates we may instance the penguin and the beaver. The wariness of birds is well known. Thus in Australia the white cockatoo lives in large flocks and always stations a picquet in the trees while the flock is feeding on the ground. Similar care is taken in selecting a tree upon which to roost at night. This instinct is carried a stage farther by the antarctic penguins, which are safe on the ice, but are eaten by a large " shark " in the water. The flock marches down from the rookery to the ice edge, where they line up and try to push one another into the water. The photographs of the birds watching over the edge to see the fate of the unwilling scout are well known. If he begins fishing, they know that all is safe, and drop into the water also. If he disappears, they go back to the rookery and wait for a more auspicious omen. Their absence of fear on the ice is seen in the nesting instincts. The male " proposes " to the female by placing a stone in front of her as the first step towards building a nest. If she accepts it, they regard each other as man and wife. But a bachelor bird will propose in the same way to a sailor if he stand quiet for a few minutes. The dam of the beaver, the modification of the tail wherewith to smooth the outside of the mud house before it is frozen, and the canals which are made to keep the pond full, are some of the instincts

that look most like intelligence. But the beaver begins to build a dam in his house from any kind of unsuitable objects, such as boots or brushes, and this although there is no water in the place.

Secondly, the metabolic scheme investigated by Rubner, and already referred to, is an indication of abundant food and the leisurely evolution of man. He had both food and easy conditions of life, and perhaps in consequence his reason is the precise antithesis of the instincts of the beaver and the penguin, for reason must be correlated with freedom.

The absence of organs of attack and defence, the non-development of the special senses, the non-growth (that is the apparent retrogression) of the bones of the face, and the great toe of the female foot, all indicate safety. This toe is relatively as well as actually smaller in woman. As this toe may be called the chief organ of running, one would have expected this method of escape to have been very highly developed in those who were less able to fight.

In addition, as old age approaches, all the bones become thinner, lighter, and more fragile. The skull alone loses about two-fifths of its weight. Female skulls are thinner and lighter than male, but heavier in proportion to the weight of the rest of the skeleton (1 : 6 as compared with 1 : 8). The brain grows very rapidly in the early years of life, and attains its maximum weight before the age of twenty, that is before the bones are complete. All these facts may be taken to indicate safe surroundings. For as the brain became more important as regards survival, it has been thrown more and more upon its own resources for safety, instead of being protected by a case like that of the gorilla.

How the Brain took Command

For the past six thousand years there is evidence that the brain of the Mediterranean race has altered remarkably little, just as the bones of the little toe were as atrophic then as now. But there has been a remarkable change in the mind of man. Certain facts are known which show that the normal brain is acting perhaps more definitely than ever as the chief selective organ. Mentally defective children have small heads, usually much below the average size for the age. In one form of idiotcy the child of European parents has the Mongolian head and features. The normal child exhibits the desire to talk very early in life ; that is, speech seems to have become almost instinctive.

To review the successive stages by which the human brain has acquired the power to command: When the little insecti-

vores first left the ground and took to the trees, the new co-ordinated muscular actions and the use of sight as well as of smell to recognise the food marked the first stage of progress. This may have been continued by the catching of birds at night with little assistance from the sense of smell, and by the use of the four extremities independently of each other. The sands present conditions for further progress. By watching the crabs and sea-birds—trying to catch them perhaps—new food might have been recognised by the sight alone, even if it had no feathers and did not move. Touch may have been used to examine the mollusc, and a small stone used to crack the shell. The animal learnt that some shells were empty and others full. Much later curiosity was rewarded by the epoch-making discovery that shells similar to those on the beach were attached to the rocks, and that these were always worth cracking with a stone. The hand and foot were then steadily advanced towards perfection by the needs of the new food-supply, and the skull was gradually poised on the condyles. The new food-supply was handed on from father to son by tradition, not by instinct, and the growth of the brain was assured as soon as it had reached the point of thinking of things that were out of sight. The instinct of the squirrel has taught it to bore a small hole in a nut to see if it is worth while going farther in the matter, and I am told that the baboons steal tins of bully beef as readily as fruit or loaves of bread.

If we suppose that protoman was social and had as good a language as the baboons, the tradition would soon lead to the elements of speech. It has recently been suggested that sign language arose before speech, chiefly from the fact that the same signs were common to all the North American Indians, although their speech was quite different.[1] Of course very early human skulls have the bony markings for the muscles of the tongue of a primitive type, and the view is widely held that speechless man was quite a large animal. But when speech began, the human race had not yet separated into the three great divisions, and yet man was already right-handed, for the speech centre is always on the left side of the brain. Thereafter brain growth was accelerated and knowledge in a new sense began. The consciousness that thoughts can be expressed by words must have been a continuous stimulus to evolution along the new path. The dawn of this consciousness forms the turning-point in the education of the blind deaf-mutes. It was very marked in the classical case of Laura Bridgeman. The changes which ensued all appear to be very ancient, and they are characteristic of all existing races as compared with the apes. The head of the fœtus was enlarged, the period of

[1] Evans, *Anthropology and the Classics.*

gestation prolonged, and correlated changes took place in the skeleton of the mother. The internal carotid overtook the external in regard to size, and the face did not grow at the same rate as the brain. It is easier to explain all these sequences if we hold that incipient speech marked the little protoman, and that it was a triumph of peace. A parrot has not the human organs, but it can "articulate" very distinctly, and it seems more probable that the American picture-language drifted round the world from the Nile, rather than a gorilla-man began to think and talk on his own initiative.

Objections to this Theory

In conclusion, the way must be briefly indicated by which a number of obvious objections may be met.

In favour of the recent separation of man and ape the baby is cited. He sometimes is clever at hanging by his hands soon after birth, and he walks at first on the outside of the foot, the head of the astragalus being directed more inwards, and its neck is longer than in the adult. The lower races of mankind are more ape-like in having shorter thighs, no waist, ridges on the bones of the skull, and other points less marked in Europeans. Similarity in mentality is also seen between the orang and the Mongolian, and so forth.

The reply seems to be that descent from a common ancestor is undoubted, but the genetic relationship need not be recent. Mere increase of size from a small animal to man may have occupied only a few hundred thousand years. Hence if there were large apes in the Miocene these were not necessarily in the direct line of the human race. For the structural pattern and similarity of function are essentially conservative and slow of change. No biologist would claim that the ox is the lineal descendant of a codfish, even though the internal secretions of the pituitary bodies appear to be very much alike. Continuity looms more largely in nature's plan than variation appears to do.

Blood relationship and liability to the same diseases is a new "proof" which has been exploited a good deal of recent years. Nuttall's results, on which our knowledge is based, showed that the lemurs and American monkeys are far away from the apes and man; the Old World monkeys come next, and the *simiidæ* are nearest related to man, but the orang, chimpanzee, and gorilla appear to be equally human in this aspect.[1] This work was carried out at a time when modern laboratory methods were in their infancy. If the application of recent refinements in technique should confirm the position

[1] *Blood Immunity and Relationship*, 1904, pp. 2, 222.

that the three apes are equidistant from man, this result would make it certain that the common ancestor lived before the appearance of the large apes in the Miocene, for no one would look upon the orang as being the parent of the gorilla, or *vice versa*. As to the common susceptibility to malaria, syphilis, bilharziosis, and similar diseases, it is too soon to make any dogmatic statement as to the degree of kinship implied. The experts who know most about the Wassermann test are cautious in accepting any theory of the action of complement and amboceptors as being final. At present it may be asked if any known facts about men and monkeys are more suggestive than the relationship which is demonstrated by vaccination between the child and the calf. We do not even know why red-haired people should be specially subject to rheumatism.

Lastly, it is always assumed that disease alone can cause atrophy—the use of weapons for fighting and killing by early man led to the disuse of the canine teeth, and the consequent reduction in the size of the teeth led to a decrease in the size of the jaws, "as we may feel almost sure from innumerable analogous cases. In a future chapter we shall meet with a closely parallel case in the reduction or complete disappearance of the canine teeth in male ruminants apparently in relation with the development of their horns; and in horses in relation to their fighting with the incisor teeth and hoofs."[1] In all herbivora the large canines are a handicap because they prevent any side-to-side play between the upper and lower molar teeth. The original canines of the *Condylarthra* have disappeared in order to allow the herbivora full use of the molars; that is, horns were acquired because the canines were lost, not *vice versa*. In rumination the lower jaw is swung as much as two inches from side to side. The survival-value of good molars in such animals is seen in the case of some breeds of dairy cows in which the lower jaw has become so narrow that many of them have become "bad doers," that is, they cannot masticate the food sufficiently to maintain the highest condition of health. The large canines of the musk deer, pig, and hippo form a separate problem, as they grow from persistent pulps.

Other examples of handicap are seen in the lost tail and projecting great toe of some monkeys, the legs of snakes, wings of apteryx and ostrich, and the legs of the whale. The small eyes of the mole and cave-dwellers are to be explained by the fact that a large eye offers a very easy way of severe injury, and that it is therefore only justified by its utility. The loss of the lateral toes in hoofed animals is obviously useful. The disappearance of the body of the scale insect, of the organs of many parasites, the atrophy of unused muscles, and the loss

[1] Darwin, *Descent of Man*, p. 53.

of weight of the bones in old age, are all examples of the organism using these structures for purposes of food, just as the fat and the reserve material in the liver are continually being used for food. In the same way most of the tissues are used to conserve the life of an animal suffering from starvation. On the other hand, examples of apparently useless parts which do not disappear are seen in the human vermiform appendix and in the mammary gland in the male.

The true position seems to be that the needs of survival will account for large specialised organs, such as the teeth of the baboon. A part not required for survival purposes is dealt with in different ways, and is often tolerated for countless generations. But there has been some force tending towards economy, especially in the hard parts. Since the Eocene this has reduced the size of all bones to the minimum weight consistent with due strength. It has modernised the teeth, given the female small bones, and reduced them all in old age. It has converted the inside of the bones into secret laboratories. The brain is conserved at all costs both in development and in times of starvation. The fat is cheap and stored up for hard times. We know that part of this regulation is done by the pituitary and thyroid. It seems that full mental and sexual development cannot be correlated with massive bones, and a very large man has often too weak a heart to carry him through an attack of pneumonia. Hence giants and the Neanderthalers have all died out. This regulating mechanism has only quite recently (that is six thousand years ago) brought forth progressive man in the proto-Egyptians. The apparent atrophy of the Heidelberg jaw and the Talgai palate is not due to disuse, but to the action of these correlating forces, which have in the meantime found the Mediterranean race far more plastic than the Australian aborigine or the Eskimo.

CORRESPONDENCE

To the Editor of "Science Progress"

MATHEMATICAL PHILOSOPHY

From HON. BERTRAND RUSSELL, F.R.S.

Sir,—The review of my *Introduction to Mathematical Philosophy* by the late P. E. B. Jourdain, in the April number of Science Progress, contains some statements which I cannot pass by in silence.

In the first paragraph, on p. 673, Mr. Jourdain repeats his complaint that I ignored his attempted proof of the multiplicative axiom. In actual fact, I took endless pains over it. At first, I told him where I thought it faulty; he then sent me a new version, altering other parts, but leaving the vital point unchanged. This process occurred repeatedly. I consulted all competent people whom I could get hold of, and they all agreed with me in thinking the proof invalid. I have read his proof with minute care in its various successive forms; I have written to him over and over again explaining where I thought it faulty; but on this point it has not changed. Mr. Jourdain was a friend of many years' standing, for whom I had both affection and respect, and I was reluctant to enter into public controversy with him. Therefore when I did not think well of his work I did not mention it. Neither in connection with the multiplicative axiom, nor in any other connection, had I any other motive for not referring to him. This applies in particular to the matter mentioned in the second paragraph of p. 673.

In conclusion, I must entirely repudiate the statement at the top of p. 672: "Mr. Russell acknowledged the justice of the present writer's strictures in the *Cambridge Review* on a crop of similar errors, but said they were all due to Dr. Whitehead." This odious accusation must be based upon some conversation of ten years ago, and I can only base my denial upon the certainty that that is not the sort of thing I should do.

Yours, etc.,
Bertrand Russell.

April 20, 1920.

To the Editor of "Science Progress"

THE ICE-AGE QUESTION

I

From A. H. BARLEY

Dear Sir,—I am looking forward to the answer of astronomers to the question propounded in Major Marriott's very temperate reply to Mr. H. Spencer Jones's letter in the July number. It seems hardly likely I am alone in this, for the present period is one in which "authority" is viewed with disfavour except in so far as it justifies its position through the appeal to

reason: this is markedly so in matters social and political, and still more should it be so in matters scientific. Therefore it is to be hoped that either Mr. Jones or some other representative of Greenwich has been prevailed upon to explain why Drayson's discovery—no inverted commas, please—has been left uninvestigated by official astronomy for close on half a century.

The geological facts and arguments adduced by Major Marriott are not, it would seem, contested by geologists and therefore stand Yet official astronomy, while offering no explanation itself, rejects that which he gives; assigns no grounds for so doing; and seems content to reflect that the two sciences are at variance on another point. This is unsatisfactory, and indeed unreasonable. However, by their rejection of Drayson's problem astronomers have involved themselves in a very singular dilemma, as I will briefly explain.

There is a point in the heavens from which all the stars appear to be separating, those on the one side increasing their right ascensions and those on the other decreasing them. This spot is known as the Apex of Solar Motion, since it is thought to be the direction taken by the solar system in its motion through space Its exact location is not definitely known, and many different determinations have been made during the last hundred years or so. Just about fifty years ago Drayson demonstrated that this spot was in

R. A. 265.1 N. Dec. 31.1

The latest determination by Weersma in 1908 assigns it to

R. A. 268.0 N. Dec. 31.4

It will be seen that these positions are almost identical. They are at any rate vastly more accordant than any two that can be picked out of the list of thirty-six different determinations given by Campbell in his *Stellar Motions*.

Weersma's result is derived from all the "proper motions" that were available to him in 1908 and is presumably to be preferred above any of the earlier ones cited by Campbell. Yet fifty years earlier Drayson secured an almost identical result—not by guessing, but by calculation—and without having recourse to the "proper motion" of one single star!

Essentially, both these determinations rest upon certain definitely ascertained facts, namely the recorded observations of stars and the discrepancy existing between the observed positions and the predicted positions as derived by calculation from the orthodox precession theory. But whereas in finding an explanation to fit the facts orthodox astronomy throws the effect upon the stars generally, and on the solar system as a whole, predicating heterogeneous motions in many vast and distant bodies, Drayson on the other hand explains the same phenomena—and, be it noted, reaches the same numerical result—by throwing the effect upon one homogeneous movement of a single small and near body, the earth.

The first of Newton's three *regulæ philosophandi* requires that no more causes are to be admitted than are sufficient to explain the phenomena. Official astronomy is consequently confronted with this dilemma: that she must either admit Drayson's explanation, or palpably fly in the face of the First Rule in Philosophy.

I trust you may be able to publish this letter, for the matter is too important to be let drop, and should be threshed out. Other points might have been enumerated, but the one given is that which I personally consider the most striking as *prima facie* evidence on the astronomical side of the question.

Yours faithfully,

ALFRED H. BARLEY.

II

FROM G. W. TYRRELL, A.R.C.Sc., F.G.S.

DEAR SIR,—The intervention of the physicists and the astronomers in geological problems has frequently been unfortunate for geological science, and Major Marriott's irruption into the glacial problem does not seem likely to effect a change in this respect. In spite of the confident title of his recent paper, "The Ice-Age Question Solved," I fear that this problem will occupy the patient attention of glacialists for many weary years yet. A study of the most recent textbooks indicates that astronomical theories of glaciation are at present greatly discredited by geologists. Pirsson and Schuchert (*Textbook of Geology*, 1915) devote one short paragraph in small type to only one of the astronomical theories (polar wandering) in their discussion of the causes of glacial climates, and remark that (p. 953) "As yet there is no accepted explanation of why the earth from time to time undergoes glacial climates, but it is becoming clearer that they are due rather to a combination of causes than to a single cause. Perhaps the greatest single factor is high altitude of the continents, with great chains of new mountains (the hypsometric causes) which disturb the general direction and constitution of the air currents (the atmospheric causes) and the ocean currents as well." Chamberlin and Salisbury (*Geology*, vol. iii, 1906, pp. 433 *et seq.*) devote considerably more space to hypsometric than to astronomical hypotheses.

Glacial periods appear to be associated in geological time with the relatively short periods of crustal unrest, characterised by broad and high continents, shrunken oceans, and climatic extremes; which alternated with vastly longer periods of crustal quiescence, characterised by relatively small, low continents, wide-spreading shallow seas, and climatic equability. This is a well-established geological fact: hence it is no wonder that for the explanation of past climatic variations, of which glaciation is only one aspect, geologists are turning to purely terrestrial and geological causes, especially diastrophism or crustal movements.

Major Marriott is apparently chiefly concerned to establish the hypothesis of glaciation as due to change in the obliquity of the ecliptic, based on Drayson's cycle of 31,756 years instead of the orthodox astronomical cycle of 25,868 years. This leads to the view that the last minor glaciation (not the "boulder-clay" glaciation; see R. A. Marriott, *Changes of Climate*, pp. 17 *et seq.*) ended about 7,000 years ago and culminated about 15,000 or 16,000 years ago. Major Marriott cites figures by Holst and others, based on De Geer's method of counting seasonal bands in glacial and post-glacial deposits, which lend some support to this view. But it would be easy to cite figures, equally reliable (or unreliable), which support the orthodox cycle, or which are at variance with both.

The differences between the figures for the two cycles are insignificant from the geological point of view, for geologists cannot as yet provide such exactitudes from their side of the question as can the astronomers. They have Huxley's mathematical mill too much in mind to place much reliance upon actual time estimates. Hence, even if astronomical hypotheses of glaciation were entertained, geologists would be not at all concerned to choose between the orthodox astronomical Tweedledum and the Draysonian Tweedledee.

One difficulty with these astronomical cycles of glaciation is their regularity. From anything to the contrary in Major Marriott's papers they may be presumed to extend back, cycle on cycle, into the remote past. Where do they begin, and why? The American evidence, according to Chamberlin and Salisbury, is strongly in favour of the view that not only were the separate glaciations of the Pleistocene epoch of different lengths and intensities, but that the intervals between them were also variable. Is the succession of the

minor cycles of glaciation (orthodox or Draysonian) begun and ended with the superimposition of a greater rhythm? Major Marriott himself (*Changes of Climate*, p 18) has to admit that the recurrent glacial conditions worked up to a maximum of intensity (in the "boulder-clay" glaciation) and then again decreased, and has to introduce an irregular rhythm of alteration in the shape of the earth's orbit with varying periods of hundreds of thousands of years to explain this. Furthermore, how do these superimposed cycles link on to the great Permian equatorial and other glaciations of the geological past?

A further objection to the astronomical cycles is their short period, which, under the most favourable conditions, would not allow the known spread of the Pleistocene ice sheets, their oscillation about their extreme limits, and their final retreat and extinction, within the time limits proposed. For example, Chamberlin and Salisbury state that the Labrador and Keewatin ice sheets appear to have pushed out from their centres about 1,600 and 1,500 miles respectively; but allowing only 1,000 miles of advance at the rate of one foot per day (an estimate much beyond the probabilities) and not including halting and retreating stages, it would take more than 14,000 years for the ice margin to reach its observed limit of extension. If the safety factor of 500 miles be included, a corresponding increase in time must be allowed. Similar figures must obtain for the European Pleistocene glaciation. Even within the limits of the augmented Draysonian cycle it is difficult to find time for the spread of the ice sheets, for the known oscillations of their margins about extreme positions, and for their final retreat and disappearance.

Finally, is it at all certain that an increase in the obliquity of the ecliptic is competent to bring about glacial conditions? Major Marriott dismisses this question very inconclusively in a paragraph (*Changes of Climate*, p xi), and states that the question has never been subjected to discussion among physicists. This, however, is incorrect, as reference to chapters xiii and xiv in W. B Wright's *Quaternary Ice Age* (1914) will show. In reference to the hypothesis of glaciation by change in the obliquity of the ecliptic, Wright tabulates (p. 300) the calculations of Meech regarding the amount of heat received on each 10 degrees of latitude when the obliquity of the ecliptic was near its maximum value 10,000 years before 1800, and remarks, "It is considered that these figures demonstrate that no marked climatic changes can be produced in this way."

Geologists are coming to believe that the great climatic variations of the past are based upon an intricate compound rhythm into which several components enter (see Barrell, "Rhythms and the Measurements of Geologic Time," *Bull. Geol. Soc. America*, vol. xxviii, 1917), of which perhaps the chief is the diastrophic deformation of the earth's crust. There may be a minor astronomical component contributing to the rhythm of climatic change, but we are as yet far from disentangling its effects from those of the more dominant factors.

I am, Sir,
Yours truly,
G. W. Tyrrell.

April 16, 1920.

To the Editor of "Science Progress"

A GREAT DEFAULT

From Brig.-Gen. E. H. HILLS, C.M.G., R.E., F.R.S.

Sir,—The essay by Sir Ronald Ross in your April number, entitled 'A Great Default,' exhibits in a clear light the very small amount of scientific thought that our administrative services bring to their task of government.

I do not intend to labour this point. I wish only to emphasise one aspect of the question. Sir R. Ross explains that for the most part he received no payment for his work; presumably he asked none. I suggest that this was a wrong policy on his part, and that he would have been far more likely to impress ignorant officials with its value if he had insisted on proper remuneration, and failing that had refused to proceed. To many people the value of a thing is what it costs, and when they find that scientific men are ready to give advice for nothing, they attach just about that value to it. An engineer employed as consultant by the Suez Canal Company or the Panama Canal Commission would ask and receive, as a matter of course, a substantial fee, plus all his expenses. Why should a medical expert attach a lower value to his services? and does he not, by setting this lower value, help to depreciate the estimation in which his services are held? During the war the exploiting of scientific men by government departments was carried to a point never before attempted, but was willingly accepted by many as the form in which they could best help forward a great national cause. Even in wartime it is doubtful if they were altogether wise in submitting so completely to the predatory official instincts; in ordinary times it is certain they are unwise. Science would gain, both in popular estimation and in its real influence upon the nation, if its followers would discontinue this misplaced generosity and would cease to give skilled and highly technical advice *gratis*—if, in short, they would estimate their work on a similar basis to that set by engineers, doctors, or lawyers.

I am, Sir,

April 4, 1920. E. H. HILLS.

I entirely agree with the principles laid down by General Hills, but my case was not similar to the cases which he evidently has in mind. I was dealing with an entirely new method, which had been hitherto untried, whereas engineers and others who obtain fees for their advice are generally called upon to deal with perfectly definite and known questions. If I had ventured to charge any fee to the Suez Canal people or to the Panama Canal people, they would possibly have refused me, and a great opportunity for testing my new suggestions would have been lost. In fact, I carefully considered the point exactly in the light laid down by General Hills, and determined that it was my duty to the world to abandon personal remuneration for the sake of perfecting a new life-saving instrument. Later, when I was asked to advise the Government of Mauritius, my method had already proved itself, and I therefore asked the Government to give me a fee of one thousand pounds for five months' work, but added that, if the colony was not able to find this money, I would go there without payment. In this case I was honourably dealt with; but I cannot say the same of those other Governments and Companies which have sunk to the level of using a professional man's experiences without making any attempt to pay for them. We must remember that payment to all medical men is legally (I believe) placed upon this voluntary basis. A patient cannot be forced to pay his physician's fee; but if he does not do so he is a dishonourable person. As already frequently stated in SCIENCE PROGRESS, I am now suggesting this very point to the Chancellor of the Exchequer. Some years ago, however, Mr. Lloyd George did not appreciate the little obligation.

R. ROSS.

P.S.—A case exactly *apropos* has occurred since the above was printed. The Commonwealth Government asked me to go to Australia *as its guest* in order to discuss the matter of the Northern Territory at a congress. As this involved expert medical advice, I suggested and named a fee. It was refused.—R. R.

TO THE EDITOR OF "SCIENCE PROGRESS"

AN INTERNATIONAL LANGUAGE

FROM MONTAGU C BUTLER

DEAR SIR,—I read with interest Mr. Gilbert Richardson's letter on this matter in your current issue, and agree with all he says His omissions, however, are important Certainly, as a solution of the problem, Latin cannot compete with a language scientifically constructed for the purpose. But I do not follow Mr. Richardson in his advocacy of a scheme called Ido, for though Ido would be better than Latin, it is in my opinion much inferior to Esperanto, of which it is only a poor imitation True, in the main Ido is excellent, being pure Esperanto. It is indeed advertised as being Esperanto, but "simplified" and "reformed." An examination of the improvements is interesting.

Esperanto is phonetic. Ido is not, having various letters for one sound, and various sounds for one letter. It is to the insufficiency of its alphabet that we owe curiosities like *jermo, jinjero* (germ, ginger). Esperanto obeys a regular and natural system of word-derivation. The system of derivation on which Ido is professedly based (that of "reversibility") is so arbitrary and contrary to linguistic instinct, that it is not and cannot be carried out in practice In Esperanto the accent is invariably on the last vowel but one. In Ido it is usually on the last but one, but sometimes on the last, and sometimes on the last but two. Esperanto shows the objective case by an accusative termination regularly and invariably used. In Ido the accusative is sometimes obligatory, sometimes optional, and sometimes omitted Esperanto is marvellously flexible, with a free word-order, hence it is unrivalled as a medium for translation. In Ido the contrary is the case, and an Ido translation of the *Æneid*, for example, comparable to that existing in Esperanto, is an impossibility. In Esperanto words internationally related, as *when, then*: *where, there, here*, are regularly formed in a similar manner, and easily learned and remembered. In Ido these words are an unrelated chaos. Esperanto forms the plural invariably by the addition of a plural ending to the singular form. The Ido plural is formed in several ways, and bears no relation to the singular. Esperanto has one infinitive. Ido has three, all irregularly stressed and difficult to pronounce. The hiatuses caused by these forms, and the accented monotony of their frequent occurrence, are distressing in the extreme to a musical ear. Esperanto is international in its elements, Ido is a pidgin-French. Esperanto is "a living language of a living people." It has been put to the severest practical tests and not found wanting, and is in constant use by an ever-increasing number of persons all over the world. It is not possible to give precise figures, but half a million is a conservative estimate of the number of Esperantists. Ido has comparatively a mere handful of partisans, while many of its former supporters have launched other and equally futile schemes of their own or have become Esperantists. Esperanto has grown steadily on a firm basis, so that texts written thirty years ago are as intelligible now as then. Ido has not yet passed the project stage, being subject to continual change, so that the scheme at its inception resembles its present form as little as Chaucer's English resembles that of our own time.[1] This helps to explain how it is, that while Esperanto has a very large and growing literature, both of original and of translated works, Ido, in thirteen years, has produced only a few

[1] We are promised, however, that further changes will not be made till 1924.

leaflets.[1] No one who has read Dr. Zamenhof's *Old Testament*, Dr. Bein's *Faraono*, or other similar masterpieces, can doubt that Esperanto is a literary language in the highest sense of the word. It is not claimed for Ido that it is more than a convenient code, without pretension to literary merit or beauty. Esperanto uses a small dictionary of root-words, which it enlarges by regularly used affixes. Ido labours under a very large dictionary, and a much greater number of affixes; which however are not regularly used, and are often used to no useful purpose.

In Esperanto the feminine is regularly formed from the masculine by the suffix *-in*: *Jozefo, Jozefino, patro* (father), *patrino* (mother). In Ido, "father" is *patro*, or *patrulo*; "mother" is *patro*, or *patrino*, or *matro*. In such cases as "I *hammered* the nail, I *addressed* the letter, I *gilded* the picture, my finger *bled*," Esperanto, like other languages, prefers a simple verb-form: *martelis, adresis, oris, sangis* (*is* being the ending of the past tense), though a more precise form is available when desirable. Ido by an arbitrary rule petrifies these words into *martelagis, adresizis, surorizis, sangifis*. "The January number" of this magazine would in Esperanto be *la Januara numero*; in Ido *la Januarala numero* (this is to prevent the reader from supposing that the number of the magazine is identical with the month of January !).

The Esperanto *mil okcent dudek tri* (one thousand eight hundred and twenty-three) is in Ido simplified to *mil-e-oka-cent-e-dua-dek-e-tri* ! ! Similar improvements adorn every page of an Ido text.

The reader who is interested will find a detailed study of the question in *Historio kaj Teorio de Ido* by B. Kotzin, Moscow; *Autour de l'Esperanto*, and *Histoire d'une Délégation* by Prof. C. Aymonier, Paris; and various monographs on word-derivation and other works obtainable from this office. Their criticisms, which remain unanswered, may be verified by personal investigation.

To sum up: If Ido is ever sufficiently finished and stable to be capable of a literature, it will be more fitted for the rôle of an international language than Latin. But this conclusion is of a theoretical interest only, for unless it is radically reformed, Ido itself will not bear serious comparision with Esperanto. It is in the main a re-hash of proposals fully discussed and twice rejected by an overwhelming majority 25 years ago. Esperanto holds the field, in theory and in practice, and except for the historian and the collector of curiosities there is little inducement to spend time on an unscientific and discredited plagiary.

MONTAGU C. BUTLER,
Secretary, British Esperanto Association Inc.

17 HART STREET, LONDON, W.C.1.
February 26, 1920.

TO THE EDITOR OF "SCIENCE PROGRESS"

THE GHOST-HYPOTHESIS

I—FROM C. A. RICHARDSON

DEAR SIR,—I have read with much interest the energetic (if a trifle dogmatic) reply of your correspondent ("The Writer of the Essay-Review") to my letter published in the January number of SCIENCE PROGRESS. I very much fear, however, that he has subtly avoided a definite answer to the most

[1] A number of technical terminologies and scientific works have been published in Esperanto. The pages of *La Scienca Revuo* and other similar magazines have further demonstrated its suitability for scientific purposes.

crucial points in the objections I raised, and has been tempted instead into a torrential polemic which is little more than a reiteration in somewhat expanded form of the opinions expressed in his original essay-review.

Let us first consider what appears to me a comparatively minor issue, although a large part of your correspondent's reply consists in the elaboration of it. This is the statement that spiritualistic phenomena can be successfully imitated by professional conjurors. Now, as I said in my first letter, I am quite willing to admit that this may be so in all, or nearly all, cases, and I do not think that any spiritualist would deny it. But what puzzles me completely is the precise weight attached to this argument by your correspondent. As he says, the spiritualist's reply to it is that, in spite of certain cases of trickery, "The manifestations of the conjuror and the medium may be of the same class, but this does not necessarily prove that they are always produced by the same agencies." Exactly—although I do not think that any reasonable spiritualist would draw from this the conclusion your correspondent supposes him to—namely, "You must therefore admit that some of the manifestations are genuine"—but rather the more cautious one, "You must therefore admit that the imitations of conjurors *do not disprove* the genuineness of some of the manifestations." And surely such a conclusion is justified; for conjurors can equally well imitate ordinary natural phenomena, as in the well-known mango-tree trick. I pointed out in my previous letter that your correspondent's argument would lead to such conclusions as that all rabbits are produced out of hats. This objection he entirely ignores in his reply. Yet the whole matter hinges on it. Imitation by conjurors cannot possibly establish a proof that none of the manifestations is genuine; the whole argument from it is quite beside the point. Accordingly, I do not see how we are to avoid the conclusion that the large portions of your correspondent's two communications which deal with this particular aspect of the matter are merely a beating of the air.

I am accused by your correspondent of mentioning four important supporters of spiritualism, while omitting to refer to "the hundreds or thousands of men of science who laugh at them." Such laughter may induce in those who indulge in it a comfortable feeling of superiority, but its value as scientific argument is not great. Any omission of mine was simply due to the fact that I do not know of any eminent scientist who has impartially investigated the alleged facts with the painstaking care and thoroughnesss employed by the four I mentioned (they are few of many), and has returned from his investigation with nothing but derision and utter disbelief in the genuineness of any of the manifestations. In spite of this, your correspondent may find it easier to believe that trickery is always employed, but in that case let him refrain from ridiculing those who prefer to go more deeply into the matter.

But this part of the argument is of comparatively small importance. The real crux is the combat between materialism and metaphysical spiritualism. And here I am happy to note an advance (albeit small) in the direction of philosophic clearness in that your correspondent now describes mind as the "product of Body," and not (as before) as "the secretion of the brain." But what arguments has he in favour of materialism? None that I can discover—only statements, mainly dogmatic. He speaks of "a vast mass of observations, experiments, and thoughts connecting psychological processes with the nervous system." Everyone will admit the existence of such. But he goes on to say that they all lead "to the cumulative judgment that mind is the product of Body." This is pure dogma. Many acknowledged leaders of thought, both scientific and philosophic, are convinced that they do not lead to any such judgment at all. And again he says, "While the literature of [metaphysical spiritualism] may often be astute, clever, or even occasionally instructive, it really does not weigh a straw against the

mass of evidence (referred to above)—and most men of science, especially the biologists, will agree." In view of current tendencies in thought, it is nothing less than amazing that such a statement can be made in all confidence; for not only is it merely absurd to say that the literature of metaphysical spiritualism does not weigh a straw, it is also far from being a necessary conclusion that the evidence referred to is incompatible with the theories advanced in that literature. Moreover, are the men of science, *especially the biologists*, so wholeheartedly in favour of materialism? What of Driesch and Haldane? What of the conflict (and no one-sided battle at that) which is raging round the discussion of Mechanism and Vitalism? The wise man will truly refrain from dogmatising about the philosophic beliefs of scientists. And what of modern philosophy itself, which has outgrown Berkeley's fallacies and Kant's faulty psychology? For, after all, the issue between Materialism and Spiritualism is a philosophic one. Consider a list of the most representative exponents of recent philosophic theories: William James, Henri Bergson, Bertrand Russell, R. B. Perry, James Ward, Benedetto Croce, F. H. Bradley—these represent nearly every shade of modern opinion. But their works reveal in no uncertain accents how very, very little there is to be said for the crass and pitiful materialism into which your correspondent would apparently have us relapse, and how very, very much is to be urged against it. I know of no modern philosopher of importance who could be described as a materialist, unless it were Haeckel—and it is very doubtful whether he could be accurately described as a philosopher at all.

And now as to my "tremendous admission!" hailed by your correspondent with such delighted relief, that apparitions may be telepathically originated. Certainly this origin may sometimes be, and probably often is, an earthly one. But in very many other cases the details and accompanying circumstances of the apparition strain to breaking-point the hypothesis that it is due to telepathy from the living, while, if it is due to those no longer on earth (even if telepathy be the means employed), the spiritualist theory holds. As to the many other phenomena observed, your correspondent urges that they also may all be due to some "evil-disposed telepathist." The answer to this is obvious: Judged by any criterion that the observers (or any other possible percipient) can employ, these phenomena have precisely the same status as those more ordinary groups of sense-data which constitute the happenings of everyday life. We have no more reason for regarding the former as collective hallucinations induced by an "evil-disposed telepathist" than we have for regarding the latter as so induced. Your correspondent appears entirely to overlook the fact that the actualities are the observed sense-data, and that we know just as much or just as little about the ground of these data when their sequence is familiar as when it is unfamiliar; but in both cases the data are *objective* in the only valid sense of that term. Psychical research is simply the investigation, with a view to subsequent unification, of certain comparatively unusual sequences of phenomena.

In your correspondent's letter, then, when the wrappings are discarded, I can find but two arguments, one maintained on particular, the other on general, grounds. The one is concerned with imitation by conjurors, the other with the truth of materialism. In conclusion, therefore, and by way of crystallising the issues involved, I would like to ask him two questions: (1) Does he, in spite of the objections I have drawn attention to, and of the ridiculous consequences to which his view must equally lead if pressed to its logical conclusion, yet consider that imitation by conjurors has any logically important bearing on the truth of spiritualism, and, if so, why? (2) Does he regard the modern philosophers mentioned in the above list as the "pseudo-philosophers" referred to in his essay-review; and if not, how does he propose to refute them? In particular, how would he answer the

argument (which he has ignored) to which I drew attention in my first letter, namely: " . . . We know about brains through perception alone. So far as we are concerned, a brain is simply a group of sense-data, and the perception of it *presupposes* the existence of a perceiving mind. Materialism is thus a gigantic ὕστερον πρότερον" ?

Yours faithfully,
C. A. RICHARDSON.

January 20, 1920.

II—REPLY

FROM THE WRITER OF THE ESSAY-REVIEW

DEAR SIR,—In his first paragraph your correspondent indulges in a familiar dialectical device when he states that my previous communication is a " torrential polemic " in reply to his former letter. It was nothing of the kind. As indicated by its title, it was a general Plain Statement of the case against the ghost-hypothesis, and it referred to him only incidentally. I did not discuss his " crucial points " because I could find none, and was content to leave most of his other points to the intelligence of your readers. On the other hand, his letters are professedly criticisms of mine, and yet he ignores nearly all my points and reiterates his own—which are neither new nor sound.

I quite agree with the first part of his second paragraph. The question involved is an elementary actuarial one. By well-known rules, if out of x séances y séances are shown to be frauds, then the chances that the remainder are frauds increase greatly as y increases. And this presumption is vastly increased if the unconvicted séances are of the same type as the convicted ones, as they evidently are; and is further increased if a conjuror can do all the tricks shown in both classes—of which I think Mr. Nevil Maskelyne's letter in your issue for January last will satisfy most ordinary people. Lastly, the presumption is still further increased by the inherent absurdity—even the ridiculous absurdity—of the whole ghost-hypothesis when examined in the light of the ordinary facts around us (as I showed in my previous letter); and I conclude that the chances against any séance being genuine are, let us say, millions to one (" actuarial certainty " is put only at 49,999 to 1). This is my whole case. The spiritualist retorts that even such odds do not amount to absolute proof. He is there quite right; but then he generally adds that, as we cannot absolutely prove that some séances *are not* genuine, therefore some séances *are* genuine !—in fact, nearly all his case rests on this argument—a monstrous infringement of the elementary laws of reasoning and a typical case of the *sit-ergo-est* fallacy.

But if he is not so ignorant as to make such a mistake, he says merely, " As you cannot prove that all séances are not genuine, why don't you investigate them ? " This is really almost as foolish a position as the other. If the chances are n to 1 against the truth of an hypothesis, then the chances are n to 1 that we shall waste our time in investigating it. If n is very large, as in the case of the ghost-hypothesis, then the chances are that many men might waste all their lives investigating it. Even if a man takes the risk, what then ? If he succeeds in exposing a number of séances, the spiritualist merely repeats his parrot cry and maintains that the séances which were not exposed were genuine; and if he fails in exposing others, the doubt always remains that the medium was too clever for him. And yet these people have the assurance to ask him to waste his life over the foolish enterprise, and the impudence to assert that he cannot form any conclusions on the matter unless he consents to do so. That is the whole

of your correspondent's case in his second paragraph. He appears to have the vaguest notions on quantitative reasoning, and even on the meaning of the word "proof."

His thesis about rabbits is: conjurors can produce ghosts out of boxes and rabbits out of hats; it is as absurd to suppose that therefore all ghosts come out of conjurors' boxes as it would be to suppose that all rabbits come out of conjurors' hats. What an argument! My thesis was: just as conjurors can produce ghosts out of boxes, so can any clever charlatan do so at séances—even in the presence of a score of "impartial" scientists and pseudo-philosophers, all of whom wish to believe in ghosts, and none of whom knows anything about the conjuror's art. There is this essential difference between rabbits and ghosts: we have all seen rabbits which were not produced from conjurors' hats, but few of us even profess to have seen ghosts which were not produced by conjurors or spiritualists. The world therefore refuses to believe that rabbits are generated by conjurors; but does believe that ghosts are.

In his third paragraph your correspondent excuses himself for having omitted the names of incredulous scientists when citing the names of four—shall we say—credulous scientists, by averring that he knows of none of the former who have carried out sufficient investigations. I have just exposed this clever expedient for rigging the jury. He ignores my argument that only those people who wish to believe in ghosts are ever likely to waste their time in investigating them.

In his next paragraph he is able to cite no less than two biologists who, he says, are in favour of his views. He admits "Berkeley's fallacies and Kant's faulty psychology," and then brings up seven modern philosophers in support. Perhaps the day will come when these also will be convicted of fallacies and faulty psychology—not to mention faulty reasoning. On the other hand, he doubts whether Haeckel is a philosopher at all, and does not mention Comte, Spencer, Huxley, etc. Those who are on his side are philosophers; those who are against him are not. My view is that the former are pseudo-philosophers, and that the latter are men of science. He himself has recently written a book called Spiritual Pluralism, in which he defends the theory that "the texture of the Universe is through-and-through spiritual"; but it appears to me to be a characteristic work of pseudo-philosophy, full of long and vague words and of *sit-ergo-est* arguments. Of course my own philosophy is in general agreement with another recent book, Mr. Hugh Elliot's admirable *Modern Science and Materialism*—except that the last word does not accurately focus my conclusions. Mr. Richardson talks of "crass and pitiful materialism." The very use of these adjectives shows that he has not yet grasped even the fundamental principle of correct reasoning, impartiality. It is the characteristic delusion of the spiritualists that truth should appear hideous to them while they rave over the beauty of their own obi.

In his penultimate paragraph he tries to explain away his previous fatal admission that, after all, the appearance of ghosts to certain people may be due only to telepathic suggestion from without. The scientific reader will enjoy the skilful verbiage by means of which he tries to wriggle away from the pin. We cannot deny that his explanations may just possibly or conceivably be true; but he evidently thinks that because they *may be* true, therefore they *are* true—*sit-ergo-est* again. At the same time he seems to be quite unaware of the fact that there is a very simple and much more natural explanation of the alleged manifestations both of ghosts and of telepathy—that they are due merely to lying.

With regard to his two final questions, I think that he must be an exceptionally ingenuous person to imagine that I or anyone else would frank with our signatures two such statements containing implications with which

we do not agree. They remind us of questions asked in the House of Commons by well-meaning but uneducated Members who wish to ventilate a grievance. What he puts down as facts in the conclusion of his paper are really only conjectures, and, I think, extremely improbable ones.

Mr. Richardson, having written a long and wordy book without proving anything, ought to know that there are some matters which cannot be any more definitively discussed in the correspondence columns of a magazine; and I can only wonder that he should have insisted on wasting so much of your space and of my leisure on discussions which are notoriously always futile. At the same time I sympathise with him. He and those who agree with him wish to persuade themselves that they are above the order of nature and the risk of death, and employ every possible dialectical artifice in order to do so. But the facts are against them. Whether there is any hope in the unknown will not be ascertained in our life-time; but, so far as I can perceive, there is none to be got from that form of superstition which he and others support.

Yours faithfully,
THE WRITER OF THE ESSAY-REVIEW.

May 10th, 1920.

*** *The Editor can afford no more space for this discussion.*

TO THE EDITOR OF "SCIENCE PROGRESS"

LATIN OR IDO?

FROM H. W. UNTHANK, B.A., B.Sc.

DEAR SIR,—In Mr. G. H. Richardson's interesting letter on "Latin or Ido?" he expresses the wish to know what educational experts would say of Ido as a means of mind-training.

I send you herewith some expert views on Esperanto—a far more widely known language than Ido.

Would it not be possible to get together a body of scientific men to investigate this question of an international language, which is becoming more pressing every day?

Yours faithfully,
H. W. UNTHANK.

March 3, 1920.

NOTES

Awards for Medical Discovery (R.R.)

OUR previous number contained a full account of the report of the Conjoint Committee of the British Medical Association and the British Science Guild in connection with this subject. These bodies asked the Government to receive a Deputation on the subject; and finally Mr. Balfour, Lord President of the Council, interviewed the Deputation, consisting of the members of the said Conjoint Committee named in our last issue, together with several medical and other Members of Parliament, on March 2. The Deputation was introduced by Sir Watson Cheyne; and Sir Clifford Allbutt and Sir Richard Gregory also spoke. A good account of the proceedings will be found in the *British Medical Journal* for March 6. Sir Watson Cheyne pointed out that scientific workers were assisted by scholarships, etc., while doing their work, but that after it was done there was at present no provision for them, although, excited by the interest of their investigations, they had often neglected to make any provision for themselves. Sir Clifford Allbutt described particularly the conditions under which medical scientific work is done. He himself had been chairman for some years of the Scientific Relief Committee of the Royal Society, and knew how often even very distinguished scientists are in need of help. It was desirable to attract a great many more potential workers, but, if the Treasury adopted the proposed scheme, he feared that the expenditure which it would have to meet would not be large, as the kind of intellectual research which would be benefited by it was scarce. Sir Richard Gregory said that there should be a fund for some time for making suitable awards to be considered as payments for results achieved and not as grants for favours to come. The scientific worker, unlike the worker in literature or art, may not dispose of his achievement to the public for profit. In reply, Mr. Balfour admitted his interest in the matter, and promised to lay the proposal of the Deputation before the Prime Minister. At the same time he suggested that there might be considerable difficulties in selecting for the proposed awards the men who had actually made a given discovery. He made a very good suggestion that wealthy men in this country might do more to meet the point raised at the Deputation. After Mr. Balfour had spoken, I was allowed to say, with regard to the difficulty of selecting men for the proposed prizes, that our suggestion was to give a number of pensions of five hundred pounds or a thousand pounds each, so that men who had done really definite work were not very likely to be left out in the cold. The selection of candidates was simply a matter of thorough examination of details, by the methods used by the Royal Society and the Nobel Committee. Mr. Balfour said that he thought what was proposed was in the nature of an addition to the Civil List Pension Fund. Any such action taken with regard to the medical profession must also be extended to the other branches of scientific research.

Since then we have not heard further from Mr. Balfour as to what has been decided, but the two bodies concerned have asked him not to allow the matter to drop.

It is not advisable to go further into details at present; but I should

add that I am taking further action in another direction in the hope of getting some final result. It appears to me that the Council of Medical Research is likely to be the chief opponent of the scheme because, very naturally, they want to get as much money as possible in order to pay for current researches. I should not quarrel with them for that at all; but, as I have said before frequently, it would be honest in the nation to pay for benefits received before asking medical men for benefits to come. I am not in favour of too much money being granted for current researches because I fear that many of such lead to very trifling work.

Professor Bose's Remarkable Work.

Many of our readers will have followed the recent controversy which has revolved around the work of a distinguished Indian scientist, Professor Sir J. C. Bose, F.R.S. By a most ingenious apparatus the Professor has contrived to magnify enormously the slow growth, movements, etc., of plants; such slow movements are of course invisible in nature, but Professor Bose has shown that it is possible by means of his apparatus—the crescograph—to follow carefully the growth movements of plants.

Some doubt as to the reliability of Professor Bose's work was expressed by Professor Waller, of the Imperial Institute, at a meeting of the Royal Society of Medicine, which had been previously addressed by Professor Bose.

Subsequently Professor Waller took the regrettable step of writing a letter about this subject to *The Times*. Professor Bayliss, of University College, London, then wrote a letter to *The Times* inviting Professor Sir J. C. Bose to bring his apparatus to the University College Laboratory, and there demonstrate it before a committee. The latter examined the crescograph and saw it recording, and the members of the committee wrote the following reply in *The Times* of May 4:

Sir,—Sir J. C. Bose kindly agreed to demonstrate to us his "crescograph" on Friday afternoon, April 23, in the physiological laboratory of University College, London.

In accordance with the results given by the application of various tests, we are satisfied that the growth of plant tissues is correctly recorded by this instrument, and at a magnification of from one to ten million times. We saw in particular that a flower-bud in active growth, if treated by immersion in a solution of potassium cyanide for some hours, no longer gave a movement of the recording spot of light. We conclude that such movement, when shown by a similar bud in the active state, is not due to accidental stretching or to undetected effects of currents of air, radiant heat, etc. We agree that the instrument correctly records changes of length in the growing tissue, or, indeed, of any substance attached to the lever of the instrument, however such changes may be produced. Naturally, under the conditions of the experiments, it was impossible for us to analyse completely the complex effects produced by the passage of an electrical current.

We are, etc.,

W. M. Bayliss (Professor of General Physiology in University College, London).

V. H. Blackman (Professor of Plant Physiology in the Imperial College of Science).

A. J. Clark (Professor of Pharmacology in University College, London).

W. C. Clinton (Assistant Professor of Electrical Engineering in University College, London).

F. G. Donnan (Professor of General Chemistry in University College, London).

Rayleigh (Professor of Physics in the Imperial College of Science).

To the Editor of "The Times"

SIR,—Although we were unfortunately prevented from being present at the demonstration on April 23, we have seen elsewhere similar demonstrations by Sir J. C. Bose, and agree that the growth of plant tissues is recorded by the crescograph, and that changes in the indications of the instrument record when the plant is treated in such a way that its growth would naturally be modified.

We are, etc.,

W. H. BRAGG (Professor of Physics in University College, London).
F. W. OLIVER (Professor of Botany in University College, London).

Subsequently, at a meeting of the Royal Society, Professor Waller gave an exhibition of "growth" movements in a wet violin string. Professor Sir J. C. Bose was present and replied to Professor Waller's criticisms. Professor Vines, of Oxford, Professor Farmer, of London, and Professors Bayliss and Donnan spoke on behalf of Professor J. C. Bose. The meeting was somewhat unsatisfactory and inconclusive, but the weight of evidence seemed to favour Professor Bose.

We sincerely hope that Professor Bose will continue his interesting researches. As the President of the Royal Society remarked, apart from the question of plant growth, the making of an instrument capable of magnifying movements up to several million times is no mean achievement. We do not know how to regard Professor Waller's interest in this discussion. While we recognise clearly that new discoveries must be put to crucial tests, it will be admitted by most of those who have followed the whole matter of the crescograph that Professor Waller has unwittingly placed himself in a somewhat trying position. To clear away the obscurities surrounding this subject will need further work on the interesting material unearthed by Professor Bose.

The Poet and the Phoneticians (Sir R. Ross)

That excellent body the Society for Pure English, of which I have the honour to be a member, has issued as its Tract No. II an essay on English Homophones by the Poet Laureate (Clarendon Press). Homophones are different words which have the same sound, such as *air*, *ere*, *e'er*, and *heir*, and Mr. Bridges gives a list of them and some pertinent remarks about them. He thinks that the frequency of them is a sign of degeneracy in the language; but, in my opinion, this fear is rather exaggerated. They really cause little confusion in the spoken language, because, even if the context does not enlighten us, we are always at hand to explain ourselves—as in the case of the good lady who got a little sun and air at Brighton. But they are a plague in the written language; and even Mr. Bridges does not seem to have fully grasped the fact that they are the chief cause of the extremely irregular spelling of our monosyllables, and therefore of the agonies of millions of children from London to San Francisco and farther. For the constructors of our curious orthography—which is really much more efficient than our spelling-reformers would have us believe—rightly held that in script, where there is no one at hand to explain, much error might be caused by spelling homophones in the same way. For example, how would Shelley have written the verse about Milton—

. . . but his clear sprite
Yet reigns o'er earth; the third among the sons of light;

or Dryden the line describing the Trojan fleet

Entering with cheerful shouts the watery reign,

if *sons* and *suns*, and *reign* and *rain* had been spelt alike, as our reformers desire? *Hail, Holey Light* and *Cease, O clamorous Mews* are other monstrous puns which the said ingenious gentlemen would foist upon us. As a reformer myself, I find it difficult to deal with homophones without modifying our principle of one sound one sign—yet these are only a few examples which occur to me *currente calamo*. But while I agree that *air*, *ere*, and *heir* should be distinguished, I see no reason why all of them should have been spelt wrong, as they are.

I think that Mr. Bridges is not concerned so much with collecting the homophone carrots in his back-garden as in throwing remarks over the paling at those enemies of the human race who live in the adjoining premises—the fonertishnz. He is aghast at the horrors of English prernunsicishn, as disclosed in the Phonetic Dictionary of the English Language by Herr Hermann Michaelis of Briebrich and Mr. Daniel Jones, Lecturer on Phonetics at University College, London (Hachette & Co, 1913) This work seems to have been designed to teach German commercial travellers how to pass in England for natives and even for members of English Universities, not to mention of our aristocracy; and it is a good book because it succeeds in its object—of showing foreigners, by means of the International Phonetic Association's alphabet, how exactly slipshod English may be mumbled in familiar conversation. But nevertheless it reminds me of the Chinese tailor who, when told to copy a pair of trousers, put in the patches as well; or, more expressly, of the German who before the war aspired to teach his countrymen the art of fashionable golf, and therefore showed them, not how to play well, but how to press, slice, and adopt the correct stance!

The Poet Laureate admits the competence of the book for its project, but quarrels with the authors because of their delusion that their pronunciation is the ideal one; and he has fully established his point. The language is certainly often spoken as they indicate; but also in many other ways, depending upon locality, class, race, individuality, and even momentary occasion. Thus, I myself am conscious that I use different pronunciations over the table, through a telephone, and to a large audience. Which of all these dialects, or shades of dialect, is the correct one, even—let us say—at Oxford? I have been interested in the subject for forty years, and have indeed frequently tried to solve the great problem of "the average pronunciation" by a means which I have not seen mentioned in books on Phonetics—by going (wickedly enough) to church in order to hear the massed pronunciation of the congregation in responses or singing. It has been a revelation. Persons who employed the worst Michaelis-Jones speech in conversation were now heard to space their unstressed vowels in a way which would have shocked the lexicogrerferz extrordnrly. Yet the change was evidently unconscious. The truth is that, as Mr. Bridges emphasises, the pronunciation indicated in the written language is the standard pronunciation (when the spelling is normal), and the Michaelis-Jones language is merely the result of hurried verbalisation. He gives us a useful analogy in the case of handwriting—which we know too well varies from a beautiful calligraphy to utter scrawl: one might as well argue that the scribble is the correct way of writing because it is the most common—and in fact there is actually a type of fool who writes badly on purpose, in order to show his own "character," just as other people drawl or drivel their speech.

This alleged scientific establishment of the despotism of a degraded pronunciation by means of phonographs and phonetic type is now a very old pose, and was, I think, started by Henry Sweet and R. J. Lloyd at the end of last century. I knew the latter and discussed the question with him often; and though he died in Switzerland early this century his views persist in the Dictionary. I for one agree with few of the doctrines laid down in the preface of that book. The authors say that the spoken language is far more

important than the written language, and is the true language. Is it? The written language is the crystal, the spoken dialects are merely a series of impure solutions. The written language is the vertebral column without which speech melts away like a jelly-fish on the beach. Lloyd used to argue that everyone should write in the I.P.A. alphabet and in his own pronunciation; but this would mean the breaking up of the language into innumerable dialects—which is not to the interest of humanity. We do not wish the Tower of Babel to be restored; and the Poet Laureate has done a service in calling attention to the mischievous disintegrative tendencies of some of these wild theories of phoneticians. Proper pronunciation ought to be taught in the schools, rather than the writing of bad pronunciation; and when the authors of the Dictionary actually find fault with modern teachers for inculcating the former I think (and Mr. Bridges decides) that their argument is pernicious. The authors say, "It is a regrettable fact that most pronouncing dictionaries definitely encourage many of the modern spelling-pronunciations. We feel that such artificialities cannot but impair the beauty of the language." And they would have us say *iksept, ikstrawdnri, awfn,* and *seprit*! There is no accounting for tastes—or arguments.

But the breaking-up of our far-flung language owing to the inadequacy of our spelling is a real danger. The pronunciation of most of our words of southern origin can generally be gathered from the spelling by those who know the rules—which are, I believe, never taught in the schools; but our words of northern origin, especially the monosyllables, are so badly spelt that no one can pronounce them at sight. Reform would be easy; but unfortunately there is so little intellectual life in this country that the inertia of stupidity remains unconquerable. I have before me as I write the Poet Laureate's excellent tract on English Pronunciation; and I wish he could be induced to lead a movement towards amendment. The Simplified Spelling Society has worked hard; but it has tried to force on the public a system which is, in my opinion, by no means the best possible. I myself would advise quite another line of action—chiefly the return to some old styles of spelling which have been superseded by worse ones of to-day. See also my article in SCIENCE PROGRESS, October 1913. But this is a seprit biznis.

Modern Literary Criticism (R.R.)

Some two years ago I ventured to criticise modern critics for some of their dogmas,[1] and I was glad to see lately that Mr. Alfred Noyes took up the same task in his lecture at the Royal Institution on January 24th. One must be careful because we observe that some of these gentlemen are quite supreme in their art. Thus the *Athenæum* admitted that Lord Fisher is a great man, but not so great as itself when the judgment of poetry is concerned; and apparently the modern literary critics are the best who have ever existed. As I pointed out, and as Mr. Noyes complains, their task at present is to lash the Victorian Age. Almost every review pours scorn on the wretched writers of that time—chiefly because they showed evidence of rhythm, design, object, and even such a shameful thing as knowledge. Thus one of the heaven-born declared a few months ago that "the last word on the mannered subtleties of the late Victorian poets was said by Charles Sorley, whose style he [Sorley] thus compliments and condemns: 'It teems with sharp saws and rich sentiment; it is a marvel of delicate technique; it pleases, it flatters, it charms, it soothes; *it is a living lie.*'" Mr. Noyes mentions

[1] SCIENCE PROGRESS, July 1917.

another author who cannot write grammatical English, but who wipes out the reputation of Tennyson. A third celestial says about George Elliot that, "of all the major Victorian reputations, hers is the most faded. . . . We, who have learned that art has nothing to do with ethics, and have acquired a subtlety in the dissection of the human heart beyond the wildest dreams of the Victorians, do well quietly to depose her." So, then, poetry need not contain sharp saws, rich sentiment, or delicate technique; it need not please, flatter, charm, or soothe; and it must have no moral teaching. It must in short be modern. But why depose the Victorians only? For if these things be true, poetry began only a few years ago—and will end a few years hence.

Yet another critic says that "the last five years have produced a body of poetry to match whose volume and merit we must go back to the first ten years of the seventeenth century." We admit the volume, but—— Well, very few people read these great works; but I have had to read some of them, and must confess to certain qualms. Certainly there are numerous pretty little flowers of verse peeping out here and there among the barren rocks of politics, of which our journals are mostly composed; and books of verse (evidently issued at the expense of the authors) are poured forth incessantly. What is the quality of them? The pieces are almost entirely lyrical—or not even that, because many of them possess no rhythm. For example:

> "But what satisfaction do you think there is
> In a black printed word?
> I tell you, we envy the painters and carvers."

Apparently, in order to write poetry, all we have to do is to place successive groups of words on different lines, each beginning with a capital. Our poets are so democratic that they hate the discipline of rhythm just as many of our working men seem to hate the discipline of work. But they abandon not only rhythm, but every other supposed rule of verse; and the more "natural" they show themselves to be, the more do the critics seated on Parnassus clap their jewelled hands at them. We must be "novel," "natural," and "sincere"—that is all. How curious; because some of the greatest poems in literature show none of these qualities.

We admit that many modern poems contain a pretty or fine phrase, or even a fine figure; but the beauty of the phrase or line seldom extends to the whole stanza, much less to the whole piece; and it is only an occasional glimpse of beauty. It is that very naturalness, that very want of constructive art, which ruins them. The other day I lent Prof. G. H. Clarke's *Treasury of War Poetry* (Houghton Mifflin Company, 1919) to a friend. He said he was sorry to see that the only verse it contained with *invention* in it was my "Apocalypse"—in which I had represented Humanity before the war as a heroic figure defying the Ocean of Fate until a gigantic hand emerges and draws him down! I admit that from the modern point of view this is a shameful poem, because nothing of the kind ever really happened, and it is a living lie. "But," my friend added, "oddly enough, I still remember your out-of-date poem, while I have forgotten all the others!" That is precisely the point. We remember inventions, we forget natural things. Nature is beautiful, but manifold. The flowers are beautiful, but innumerable. Art is not nature. Art is beautiful too—but unique. And the uniqueness generally lies in the invention—which is the stamp of the human spirit impressed upon the formless metal of nature. Art seeks the beauty, not of nature, but of the spirit. The vision lives, the reality dies. The defect of most of the verses lauded by our modern critics is precisely that they are not living lies. They are dead truths—like the pebbles at our feet.

As for the critic who can see no relation between art and ethics, surely he is thinking, not of art, but of journalese art. Real art is only philosophy

teaching us by means of images instead of syllogisms; but, under the rule of the critics, art is becoming a voice crying in the wilderness—about nothing.

Another critic writes, "The strongest thing in our contemporary verse, both English and American [is] its power to find poetry in the commonest doings of ordinary life"; and he praises—

> "Drowsily come the sheep,
> And they pass through the sheepfold door:
> After one comes two,
> After one comes two,
> Comes two and then three and four.
> First one, then two, by the paths of sleep,
> Drowsily come the sheep."

Grammar also is of no consequence. Really, has the British Brain softened altogether in a kind of second childhood?

I am glad, too, that Sir Reginald Blomfield, R.A., spoke to the same effect before the British Academy on May 5th last regarding art critics, whom he rightly accused of creating the Impressionist and Cubist humbugs. The poets praised by the literary critics are precisely of the same order—literary Impressionists and Cubists. The motive of both is to attract dull people by novelty, regardless as to whether the novelty is associated with beauty or ugliness; and the critics praise them for the same reason. Mr. Frank Swinnerton and Sir Henry Newbolt have also recently criticised critics. On the whole, I wonder whether criticism has ever fallen to a lower level than it has reached now in England.

The Proposed University of Science and Technology.

We have received the following Note from the Imperial College Students' Committee:

A movement is on foot to secure for the Imperial College of Science and Technology the status of a university with the power to confer degrees in its own subjects or faculties. The proposal has been the subject of a Deputation to Mr. Balfour, as Lord President of the Council, and to Mr. Fisher, as President of the Board of Education, from the Governing Body of the College, and it has the unanimous support of the Rector and Professors of the Imperial College. It is also supported, as far as can be ascertained in any organised way, by the overwhelming majority of the past and present students of the Imperial College, and by a large and influential body of leaders of industry, vitally interested in the welfare and development of science and technology upon a national scale.

The present position of the Imperial College is somewhat anomalous. It was constituted, in 1907, of the existing Royal College of Science (itself the successor of the Royal College of Chemistry founded in 1845), the Royal School of Mines, and the City and Guilds (Engineering) College, all situated within a stone's throw of each other at South Kensington. These three institutions (while retaining certain autonomous powers) are the constituent colleges of the Imperial College of Science and Technology. By the terms of its charter the main purpose of the Imperial College is defined to be "to give the highest specialised instruction and to provide the fullest equipment for the most advanced training and research in various branches of science, especially in its application to industry." It is important to bear in mind these two features of the design, that the College was to be "imperial" and to be based on science, "especially in its application to industry." But

the Imperial College, being merely a college and not a university, has no power to grant university degrees in science. The Imperial College and its constituent colleges have each its diploma, of deservedly high repute, but they are merely diplomas, and not degrees. In view of the increasing demand for professional men possessing university degrees, and of the facts that the university degree is recognised as a hallmark and has a commercial value in the industrial and professional worlds, a very large number of the students of the Imperial College are driven to add a university degree to their college diploma, notwithstanding that, in the views of many competent judges, the diploma may represent at least as high, if not a higher, standard of scientific attainment. The university degree most accessible to them is that of the University of London, either the "external" degree or the "internal" degree; and the Imperial College is recognised as a "school," but is not a constituent college, of the University of London, so that students of the Imperial College may be recognised as "internal" students of the university for the purpose of taking the "internal" degree of that university. The drawbacks of such an irregular relationship between the Imperial College and the University of London will be dealt with later. Here it is only necessary to draw attention to the fact that the Imperial College, though a "school" of the University of London, is not and never has been under the control of the university. The College is "recognised" by the university, but is not of it.

The proposal, then, as was stated at the outset, is to give to the Imperial College the status of a university with powers to confer degrees in its own faculties. It is not advocated that such an academic innovation should be more than tentative or not strictly limited. The normal type of university, embracing a great number of faculties, would still remain and ought to be the predominant and prevalent type. All that is claimed is that it would be of national and, as will be shown, imperial benefit to introduce variety in the university world by the establishment in London of one institution granting degrees in science and technology, not to supersede but to supplement the normal type.

To some educationists this seems a startling innovation, but it is not without precedent. Zürich, Freiburg, Leipzig, Delft (Holland), and some American cities have all technological institutes granting degrees. London, as the centre of the Empire, for reasons which will appear presently, is in urgent need of a scientific and technological institution having the power to grant degrees. Such an institution would be the natural crown and centre of scientific and technological education for the whole Empire. Moreover, a great national effort is being made to promote scientific and industrial research throughout a wide range of British industries. The Committee of the Privy Council for the Promotion of Scientific and Industrial Research, now a definite Government department, is fathering and fostering the establishment of co-operative research associations for definite industries or groups of allied industries. The creation of a university of science and technology of the highest type and widest scope, such as the Imperial College would be, must exercise an important and beneficial influence on this industrial research movement, by pouring out a stream of the most highly trained technologists and researchers to carry out the detailed work of the research associations, and by raising the standard and status of scientific and technological education and research in the country.

As to the question whether the character and standing of the Imperial College of Science and Technology qualify it to receive university rank, if it be granted that it is desirable to establish a university of science and technology, there can scarcely be any doubt. The Imperial College was styled "imperial," with deliberate intention, from the first. By its charter it is charged "to give the highest specialised instruction and to provide the

fullest equipment of the most advanced training and research in various branches of science, especially in its application to industry." Its responsibilities, growing from day to day, to the industries of the Empire are manifest in its title. From its charter it is clear that it is set to perform real university work of the highest order in science and technology, and, as has been well said, the proposal to raise it to the rank of a university with power to confer degrees in its own subjects is merely a proposal to recognise *de jure* the status that exists *de facto*. On the basis only of the pre-war annual expenditure, the Imperial College is as large as Manchester University, larger than Liverpool University, much larger than Birmingham and Leeds Universities, half as large again as Sheffield University, and twice as large as Bristol University or Durham with Newcastle University. Its claims in science and technology, whether viewed from the range and standard of its subjects or from its equipment, are at least as good as those of any existing university in Great Britain.

Not only so, but there is a definite need which the Imperial College is peculiarly marked out to fill, but which it cannot do adequately unless it has the status of a university with the power to confer degrees. A large and increasing number of students from the Colonies and from the Overseas Dominions, after completing their courses in the Colonial or Dominion universities and technical colleges, go to Europe or America to take up what is essentially post-graduate scientific work, especially in its application to industry. The courses of the Imperial College completely satisfy their needs in this direction, better probably than those of any university in the United Kingdom; but the College, in its present status, cannot give to such Colonial graduates who go through the full post-graduate courses anything more than the College diploma. On the other hand, as has been said, Zürich and some American and German cities have institutes of technology granting degrees. It has already been pointed out that, in the industrial and professional worlds, the university degree is recognised as a hallmark and has a commercial value. The consequence is that there is a growing tendency among these Colonial graduates and scientific students to go to America instead of to England, so that they may have a veritable and recognised technological degree, and not a mere diploma, to show for the work they do; and the Imperial College is thus being starved of a type of student it was deliberately charged, at its foundation, to receive and train. The loss and even the danger to the Empire of such a tendency is obvious. As the Imperial College Overseas Students' Committee say, "The establishment of the Imperial College as a degree-conferring centre of higher technological education would result in the influx of a great number of students from overseas universities, and would thus be the means of developing a vast brotherhood of technical men, of immeasurable value to the Empire." On the Imperial College has been cast a special duty to consider imperial, as well as metropolitan, needs. It is obviously hindered in the performance of this duty if the highest award it can give for work of the most advanced character, equivalent to Honours standard in the universities, is a diploma, whereas technological institutes in America, Switzerland, and Germany can grant degrees.

The objection that it is undesirable to have more than one university in London will hardly bear strict examination. Greater London, with a population which may be taken as from 7,000,000 to 8,000,000, is rather a nation than a city, a nation as large as Canada, twice as large as Switzerland, half as large again as Scotland. Is it seriously put forward as an unquestionable axiom that the needs for higher education of such a population can adequately and best be met by a single university? New York and Chicago have each two universities, Washington has three, not including the Catholic university; the West Riding of Yorkshire, with a population of 3,000,000, has two universities, namely those of Leeds and Sheffield;

and Manchester and Liverpool, serving a combined population of 5,000,000, have each a university. Professor Perry, in his presidential address to the Educational Section at the Australian meeting of the British Association in 1914, boldly expressed the view that a number of separate universities would be better for London than a single university consisting of federated colleges. To propound, in view of all these facts, as a sort of sacrosanct dogma, that for London there must be one and only one university, savours rather of an academic megalomania than of a balanced educational perspective.

The Imperial College and the University of London should be set free to work out each its own future independently of the other. They have divergent aims and interests, each worthy in its own way, and it is an injurious mistake to force them into an unworkable *mésalliance*. The argument against the control of the Imperial College by the University of London was well stated in the report of the Departmental Committee of 1906:

"Industrial and commercial conditions are constantly altering; the character and relative importance of manufacturing processes, and even of entire sections of national activity, are subject to unforeseen variation. An institution which is to keep in touch with these interests must be correspondingly elastic. Its organisation must be free from all impeding trammels founded upon experience of the well-tried and comparatively little-changing track of an education regulated and rightly regulated by other aims. . . . Its governors must be in a position to govern with a single eye to the fitness of the institution for its proper function. . . . A system of control and organisation common to the new institution (i.e. the Imperial College) and the University could not be formulated without such compromise as would seriously imperil the efficiency of both."

National Union of Scientific Workers.

Report of meeting of London Branch, held at Imperial College Union, Tuesday, May 11, 1920.

At a meeting of the London Branch of the National Union of Scientific Workers, held at the Imperial College Union on May 11, Dr. Atkinson, who presided, said that the continued progress of the nation depended upon the attention paid to scientific work, and the attention paid by scientific workers to the conduct of affairs of the nation.

Dr. Evans, in his address, reminded his audience that Newton, as master of the Mint, had contributed in no small measure to the prosperity of his country by standardising our coinage, thus brilliantly disproving the assertion that the eminent scientist could not be a practical man, and that though high scientific ability might be wasted in a purely administrative post, there were many which were fundamentally scientific as well, and these must be held by men who understood and could trust science.

The country must assist Research in the right way. So far the Department of Scientific and Industrial Research had over-encouraged the commercial side, and had given such powers to the Industrial Research Associations that they declared themselves unable to interfere in the notorious Frink case, where the Glass Research Association, receiving, as a key industry, 75 per cent. of its funds from the Government, appointed an unqualified Director of Research. The constitution of the Department must be reformed, and more power given to the Advisory Council, which should be elected by energetic and democratic bodies such as the N.U.S.W., which aimed at embracing every genuine scientific worker throughout the length and breadth of the country.

Major Church, Secretary of the N.U.S.W., who took the place of Professor

Farmer at short notice, dealt with the economic question, and the way in which the Union could act. He spoke of the scandalously low salaries being paid at the present time to scientific workers, both in Government establishments and by private firms, giving actual figures. These salaries would be paid as long as people would take them. There must be organised protest and where this failed action should be taken, not by strikes, but by the professional method of the boycott of firms which underpaid their staffs. This was legal only for a Trade Union, and accounted for the Union having registered as such. The N.U.S.W. had considered a scale of minimum salaries which they believed to be fair, and which they were attempting to establish. He believed that such a policy loyally carried out would be successful; but it needed funds, a large membership, and an understanding with all sectional bodies that there would be no undercutting. The Union had now members all over the Empire, and its recent activities in connection with Income Tax abatement had aroused widespread interests. It was not the only Union, but the only "National Union" of workers in all branches of pure and applied science, and it was anxious to establish relations with all bodies representing sectional interests.

Mr. Lobb, in moving a vote of thanks, said it was the object of the Union to get representation on every body that controlled scientific work or workers, and that the economic position would then achieve itself.

Mr. Coates, seconding, emphasised the non-use of the strike by the Union, and its approval of the boycott as the proper professional attitude for the scientific worker where the employer was obdurate. (See also p. 127.)

A Deputation to the Air Ministry

We have only just been told that a private deputation, headed by Mr. Bernard Shaw, and consisting of the Presidents of many learned Societies, of the Mayors of watering-places, and members of the general public, waited last year on Mr. Winston Churchill in his capacity as Air Minister. Mr. Churchill was supported by the two Archbishops, the President of the Publicans' Association, and others. Mr. Shaw pointed out that from the time of Shakespeare, who declared that the rain rains every day, the British weather had been bad; but why, just during the years of the German air-raids, had it been allowed to clear up so beautifully? Then, shortly after our gallant airmen had put a stop to the raids in May 1918, it immediately thickened again, so that for six months after October 1918 the people of London literally scarcely saw the sun a dozen times, and really lived under an incessant and oppressive pall of gloom. If the sky ever cleared, it was only at night, and next morning the old hideous procession of dripping vapours was sure to begin again. The ancient tag that—

"The Briton's mind, you'll often find,
Is, like his climate, groggy;
For when it's dull it's very very dull,
And when it's fine it's foggy"—

was justified, for when the barometer rose for a day or two a mist would immediately spring up from the earth. The old religious exclamation, "By'r Lady!" was the only one which in his opinion covered the situation. The Archbishop said he was glad that Mr. Shaw could quote Scripture, but that he felt sure his etymology was faulty; and Mr. Churchill assured them that Mr. McLeod Foggarty, the clerk in charge of that department, had always done his best. That gentleman then explained with diagrams how

it was due to delay in telephone messages to the Water Board that the clearances ordered by him for the mornings always occurred at night. He protested against the insinuations regarding air-raids. In fact the dull weather had been expressly ordered early in 1916 in order to prevent the raids, but, owing to strikes and delays in the Ministry of Munitions, had not been delivered until after the Armistice. The President of the Publicans' Association claimed that the English climate was the best in the world; and Mr. Churchill was understood to sum up by remarking that, of all the deputations which had ever waited upon him, this one was; and the meeting then broke up.

Notes and News

The final list of appointments to the Order of the British Empire issued on March 30 contained the following names, which will be of interest here:

G.B.E.—Dr. A. E. Shipley, F.R.S., Vice-Chancellor of Cambridge University.

K.B.E.—Prof. I. Bayley Balfour, F.R.S.; Prof. W. H. Bragg, F.R.S.; Dr. S. F. Harmer, F.R.S.; and Dr. J. E. Petavel, F.R.S.

C.B.E.—Prof. H. L. Callendar, F.R.S.; Dr. C. C. Carpenter; Mr. F. H. Carr (Chief Chemist, Messrs. Boots & Co.); Prof. F. G. Donnan, F.R.S.; Mr. W. P. Elderton; Mr. A. P. M. Fleming; Prof. P. F. Frankland, F.R.S.; Dr. F. W. Edridge-Green; Sir W. H. Hadow (Vice-Chancellor of the University of Sheffield); Prof. W. A. Herdman, F.R.S.; Mr. A. R. Hinks, F.R.S.; Prof. J. C. Irvine, F.R.S.; Prof. T. M. Lowry, F.R.S.; Mr. W. Macnab; Dr. R. A. O'Brien (Director, Wellcome Physiological Research Laboratory); Mr. J. E. Sears, Mr. F. J. Selby, and Dr. T. E. Stanton, F.R.S. (of the National Physical Laboratory); Mr. G. Stubbs (Government Laboratory).

O.B.E.—Miss W. C. Cullis, D.Sc.; C. V. Drysdale, D.Sc. (Chief Research Assistant, Admiralty Experimental Station, Parkstown); J. J. Fox, D.Sc. (First-class Analyst, Department of Government Chemist); R. W. Whytlaw-Gray, Ph.D. (Chemical Warfare Dept., Ministry of Munitions); F. C. Lea, D.Sc. (Professor of Civil Engineering, University of Birmingham); C. C. Mason (Managing Director, Cambridge Scientific Instrument Co.); G. T. Morgan, F.R.S. (Professor of Chemistry, Finsbury Technical College); J. E. Myers, D.Sc. (Lecturer in Chemistry, Manchester University); R. B. Pilcher (Secretary, Institute of Chemistry); H. Stephen (Lecturer in Chemistry, University of Manchester); W. M. Thornton, D.Sc. (Professor of Electrical Engineering, Armstrong College); S. H. Trimen (Chief Chemist, Egyptian Government Laboratory).

M.B.E.— C. W. Bailey (Chief Chemist, H.M. Factory, Langwith); D. Burton (Chemist, High Explosives Dept., Ministry of Munitions); H. T. Calvert, D.Sc. (Explosives Supply Dept., Ministry of Munitions); W. B. Edwards (Principal Assistant Chemist, Chemical Inspection Dept., Ministry of Munitions); A. T. Etheridge (Research Chemist, Woolwich); A. Forster (Research Chemist, Woolwich); A. E. Garland (Principal Assistant Chemist, Inspection Department, Ministry of Munitions); R. Genders (Research Chemist, Woolwich); W. P. Paddison (Research Chemist, Woolwich); C. J. Peddle (Chemist, Derby Crown Glass Co.); F. H. Rolt (National Physical Laboratory); W. L. Turner (Research Chemist, Woolwich); H. E. Watts, Ph.D. (Explosives Supply Dept., Ministry of Munitions).

The French Academy of Science has elected Prof. A. A. Michelson, of Chicago, foreign associate member in succession to the late Lord Rayleigh; Sir J. Dewar and Prof. A. Fowler corresponding members in succession to Prof. P. Blaserna and Prof. E. Weiss; and Sir J. Larmor corresponding member in the section of geometry.

Sir E. Rutherford, Sir J. J. Thomson, and Prof. T. W. Richards of Harvard have been elected members of the Royal Danish Academy of Science.

The following appointments have been made by certain of the scientific societies during the last quarter:

Royal Astronomical Society.—President, Prof. A. Fowler; Secretaries, Dr. A. C. D. Crommelin and the Rev. T. E. R. Phillips.

Physical Society.—President, Sir W. H. Bragg. Secretaries, Dr. D. Owen and Mr. F. E. Smith, F.R.S.

Institute of Chemistry.—President, Sir. H. Jackson.

Chemical Society.—Dr. H. R. Le Sueur succeeds Prof. S. Smiles as one of the honorary secretaries.

Geological Society.—President, R. D. Oldham. Secretaries, Dr. H. H. Thomas and Dr. H. Lapworth.

Marine Biological Society.—President, Sir E. Ray-Lankester. Chairman of Council, Sir A. Shipley.

Optical Society.—President, Mr. R. S. Whipple.

Malacological Society.—President, G. K. Guide. Secretary, A. E. Salisbury.

Society of Antiquaries.—President, Sir. C. Hercules Read.

Institution of Civil Engineers.—President, M. J. A. Brodie.

Mr. F. E. Smith, F.R.S., has been appointed Director of the Scientific Research and Experimental Department of the Admiralty, under the control of the Third Sea Lord. It is intended that this department should keep the Navy in touch with outside scientific establishments, and, by working in close association with the Naval Staff, ensure that the work of the various naval experimental stations should proceed with full knowledge of the latest scientific methods and discoveries.

The Founder's medal of the Royal Geographical Society has been awarded to Mr. H. St. John B. Philby for his two journeys in South-central Arabia during the years 1917–18. The Patron's medal goes to Prof. Jovan Cvijic, Rector of the University of Belgrade, for his studies of the geography of the Balkans.

We regret to note that the deaths of the following scientific men have been announced during the past quarter: Dr. J. G. Bartholomew, the cartographer; Capt. E. W. Creak, C.B., F.R.S., Superintendent of the Admiralty compass department; Dr. G. E. Fisher, Professor of Mathematics, University of Pennsylvania; Prof. A. K. Huntingdon, lately Professor of Metallurgy at King's College, London; Prof. Charles Lapworth, Professor of Geology in the University of Birmingham; Mr. J. S. MacArthur, industrial chemist; Prof. J. A. McClelland, Professor of Experimental Physics in University College, Dublin; Dr. Rudolph Messel, F.R.S., President of the Society of Chemical Industry; Prof. W. Pfeffer, For. Mem. R.S., the plant physiologist, famous for his work on Osmotic Pressure; Lucien Poincaré, Vice-rector of the University of Paris; Dr. J. E. Reynolds, one-time Professor of Chemistry in the University of Dublin; Dr. P. A. Saccardo, the distinguished mycologist and professor emeritus of Royal University of Padua; L. T. O'Shea, Professor of Applied Chemistry in the University of Sheffield; Mr. H. C. Stewardson, chief clerk and assistant treasurer of the B.A.; Sir A. Stuart, Professor of Physiology in the University of Sheffield; Prof. Waldemar Voigt, of Göttingen, well known for his work on Magneto Optics.

A fund is in course of collection for the erection of a memorial in Westminster Abbey to the late Lord Rayleigh. Donations should be sent to the Hon. Treasurers, Sir R. T. Glazebrook and Sir A. Schuster, at 63 Grange Road, Cambridge.

The fund for the rebuilding of the Laval University of Montreal, which was recently destroyed by fire, now amounts to $3,500,000.

The Carnegie Corporation of New York has allotted $5,000,000 for the use

of the National Academy of Sciences and the National Research Council of the United States. Part of this sum is to be used for the erection of a suitable building to house these two organisations in Washington. The National Research Council, unlike the corresponding departments in England, Australia, and Canada, is not supported or controlled by the Government.

Mr. A. G. Webster, in a letter to *Science* (April 9), refers to the very excellent manner in which the French are organising the University of Strasbourg, where, he states, Prof. P. Weiss will have the best facilities in the world for the study of magnetism.

Convocation at Oxford, on March 2, decided that Greek should no longer be compulsory at Responsions for candidates in *any* pass or honours school.

Nature (May 6) discusses the bearing of the increased cost of printing and paper on the publication of Society Proceedings and Scientific Journals. It is quite obvious that the very limited funds which are available will not stand the strain much longer, while it is even more obvious that the members of these societies cannot afford to pay increased subscriptions. *Nature* itself has been forced to double its price, while SCIENCE PROGRESS, with its modest increase of one-fifth, has long ceased to be a commercial proposition. In the same number Prof. Bayliss deals with the restrictions on the import of scientific apparatus from abroad. This forces scientific workers in many cases to purchase high-priced and less satisfactory appliances of home manufacture. Mr. A. E. Miall writes also concerning the impossible cost of the laboratory equipment, which is so badly needed now that science teaching is becoming of more importance in the school curriculum. The Government seems quite able to realise that a pound is worth about eight shillings when it goes into the pockets of a coal-miner; but seems to expect its purchasing value to be three times as great when it is to be employed for scientific purposes, either for equipment or for the salary of the unfortunate teacher, who remains more than ever the only cheap item in the science laboratory.

Every zoologist who has had to teach zoology will have met with the amœba difficulty. The older earth culture methods at best produce rather small amœbæ, which failed to satisfy the critical first year medical. Sister Monica Taylor, of Glasgow University, has put all zoologists under a debt to herself, for she has invented a culture method whereby one can produce enormous quantities of large amœba proteus, and keep the cultures going steadily for months, or even years. To pots or flat jars of rain-water one adds a few grains of boiled wheat. The latter soon grows moulds, which bring in their train many micro-organisms. Such jars are inoculated with individuals of *A. proteus*, which must be found in ditches or ponds. After a few weeks it will generally be found that the scum at the bottom of the vessel is swarming with amœbæ. From time to time a few more boiled wheat-seeds are added. It is also well to place a small piece of green water-weed in each jar.

We find all kinds of London water will produce positive results, though the tap-water is not very good and should be boiled. References to Sister Monica's papers are: *Proc. Roy. Phys. Soc. Edin.*, xx, and *Nature*, April 22, 1920.

Dr. J. Brontë Gatenby has been entrusted with the task of bringing out a new edition of the *Microtomists' Vade-mecum*. Collaborating with him in this task, and in cases writing special sections, are Prof. W. Bayliss, Dr. Drew, of the *Imperial Cancer Research Review*, Dr. Da Fano, of King's College, London, and Mr. H. M. Carleton, of the Histology Laboratory, Oxford. It has been planned to have new sections on the theory of staining, on Protistology, on tissue culture and cell, manipulation apparatus, such as that of Bridges, etc. There will be special treatment of the techniques of mitochondria, fat, Golgi apparatus, and also a new treatment of the neurology section, by Dr. Da Fano. Workers in all branches of zoology, botany, and

histology are requested to communicate to any of the above-mentioned collaborators such new techniques as they may have invented, and such older techniques as are not included in the 1913 edition, but which they consider should be printed in the coming edition.

The National Union of Scientific Workers, 19, Tothill Street, S.W.1, held a public meeting at Birkbeck College on April 28, under the chairmanship of Mr. H. G. Wells. Professor F. Soddy, of Oxford, gave an address on "The Public Support of Scientific Research."

Mr. Wells gave a characteristic speech of somewhat revolutionary type. He deplored the neglect of science under the present political system, and said that unless some change took place everyone should hope and work for revolution, which could not do harm to the present status of scientific men. He drew attention to the great debt the world owed to science, and pointed out that scientific workers had never had any reward—they had established neither great families nor country seats. He declared that politicians of to-day were wasting the money provided by science. Mr. Wells bitterly attacked the present system of endowments, and said that politicians had their own friends to look after before they could attend to such matters as scientific endowments.

Professor Soddy, who received a great welcome from the audience of scientific men and students, gave a fine address, pointing out the far-reaching and beneficial changes introduced by science. During the war the ignorant politicians squealed to be saved by sciences. He pointed out that one great discovery might change human destiny. Without pure science a nation must decline. Professor Soddy condemned the short-sighted policy which believed that everything in science should be delegated to economic and money-making purposes—one could not have fruit without the tree. He mentioned, among several of the great debts owed by the world to science, the work of Ross on malaria: in a detailed manner he explained the imperfections of the present system of Research Endowments and the manner of their dispensation. He mentioned the peculiar circumstance of the appointment of an unqualified foreigner as head of the Glass Research Association.

He stated that the ordinary scientific worker was less well organised than the land-labourer. To make the world safe for democracy scientific men should make themselves master in their own house.

Following this speaker there were several addresses by members of the audience. Professor Sir William Bragg advocated a cautious policy, and took exception to some of Professor Soddy's remarks on the Research Association.

We feel sure that by far the majority of scientific workers, while not hoping for a change in our present constitution, will agree that there is room for a great deal of improvement in the status of the scientific research man, whether he be in a Government Medical Service or in a University. We would like to see proper recognition and rewards for discovery, and adequate remuneration for the scientific workers of this country.

We regret to record the death of a well-known parasitologist and authority on tropical diseases. Everyone who has worked on any branch of tropical medicine is acquainted with Castellani and Chalmers's *Textbook on Tropical Medicine*. Dr. Albert J. Chalmers was a very assiduous and successful student of tropical medicine; he became Director of the Pathological Laboratory at Khartoum, which has become famous for the splendid work which has emanated from that source.

Dr. Chalmers died at Calcutta at the age of fifty years. He formed one of a band of Englishmen, many of whom are still with us, who worked enthusiastically without adequate remuneration and recognition, and whose observations have brought great benefit to mankind.

The *Journal of the British Science Guild* publishes the report of the Microscope Committee of the B.S.G. They give the specifications of three types of microscope supposed to cost, one something under £10, the second £15–£20, and a more costly research worker's outfit at £25–£35. The Committee notes especially that:

"It is only by specialising on a few models that the above prices can be made profitable, and even then only by securing a very large output of high and uniform quality.

"All available information would seem to indicate that the day of the small maker of either stands or lenses has passed. The high standard of excellence now insisted upon, together with the low prices to which buyers of microscopes have become accustomed as a direct result of foreign competition, would seem to show that the successful manufacturers of the future must be corporations of sufficient size to make it possible for them to utilise scientific guidance to the full, and to avail themselves to the greatest possible extent of special and labour-saving machinery for their work."

Now, in the case of the second type of microscope price, £15–£20, there are three objectives—16 mm., 4 mm., and a 3 or 2 mm. oil immersion. The writer lately found that second-hand medium size stands with only a 16 mm. and a 4 mm. objective were selling at from £12–£15. The latest American instruments of Type 2 are now quoted at £15–£23, without an "oil"; but these still seem to be of better workmanship than those we produce here. The Committee strike the right note when they declare that mass production of a few types is the only hope for our manufacturers.

Sir William Osler, Bart., M.D., F.R.S.

In the death of Sir William Osler, Medical Science has lost one of her greatest sons, and the Oxford University School of Medicine, one who was possibly the greatest of its heads. To all medical men Osler was familiar through his book, *Principles and Practice*, and his editorship, with Dr. MacCrae, of the well-known *System of Medicine*.

William Osler was born at Bondhead, Ontario, the sixth son of the Rev. Featherston Osler, a Cornish clergyman who had gone to Canada as a missionary; young Osler began his medical studies at the University of Toronto, and proceeded thence to McGill University, where he took the M.D. degree. His first European experience was at University College, London, from which he took the Membership of the Royal College of Physicians. Afterwards he studied in the hospitals of Berlin, Leipsic, and Vienna, returning to his native Canada as Professor of the Institutes of Medicine at McGill. In 1889 he became Professor of Medicine at Johns Hopkins, Baltimore. America enjoyed the benefits of Osler's genius for fifteen years; here he wrote his *Principles and Practice*, and it was also at Baltimore that he first introduced the unit system which is just now being adopted by several London hospitals.

In 1905 Prof. William Osler came to Oxford as Regius Professor of Medicine, subsequently being made a Baronet. Had he been a brewer, or a backstairs political jobber, one feels sure that he would have been made a Baron. At Oxford Sir William Osler came as a fresh breeze; the good but often narrow-minded Dons could not understand how a man who used the terms "to make things hum," or "to get busy," could really be intellectual! However, many of them soon learnt that he was their master not only in his own subject, but in their own. Osler was a literary as well as a scientific genius—he loved all books, and he seemed to know a great deal about every subject.

To the younger men in the scientific departments at Oxford, Sir William was a well-known figure. At any odd moment he would appear and say,

"Show me something"; the much flattered recipient of this order would then demonstrate a new preparation, or a new instrument, or tell some new fact. Osler would commend or criticise, but always with a humorous twinkle in his eye. Once he stopped the writer of this article, and in the middle of the street informed him that he considered that "all cytologists had well-developed imaginations."

Those who know his *Principles and Practice* will have noted how his gentle wit appears here and there between the lines. Soon after his appearance at Oxford, there were many good stories of his witty sayings whilst he took his students round the wards.

Osler's activities went beyond the pale of literature and medicine. He was wont to address religious meetings for men, and during the time the cadet corps were in existence, he did a great deal in this way at Oxford. A lecture by Osler on the History of Medicine was a special event looked forward to by many; he had a splendid collection of historical, scientific and medical works which were brought out at each lecture. His publications, covering a wide field in medicine, literature, and religion, ran to several hundreds in number.

Called to a consultation at Glasgow at about the time of the railway strike, he was finally stranded at Newcastle, and caught a chill on his motor journey back to Oxford. Complications set in, and on the evening of December 29 this great man passed away. Our sympathy is with his widow, Lady Osler, who has proved such a worthy mate to her distinguished and much-beloved husband.

An Innovation

A thing almost unheard of hitherto in Britain happened on June 8, when a number of private gentlemen gave a complimentary dinner to the "Royal Army Medical Department, and the eminent civilians attached to it during the war," in appreciation of their services to the country, Lieut.-General Sir Alfred Keogh, G.C.B., being the guest of the evening. Medical science and practice labour, as we know, generally with very little recognition from the country—though plenty of it from our Sovereign; and a tribute such as this one has been very much appreciated, not only by the medical profession, but by men of science. We should, therefore, like to record here a full list of the hosts who gave us this fine banquet, and this still finer compliment. They were: The Viscount Burnham, the Earl of Derby, the Earl of Donoughmore, the Viscount St. Davids, Lord Desborough, Sir John Ellerman, the Earl Fitzwilliam, Sir Alan Hutchings, Sir Heath Harrison, Vesey C. M. Holt, Esq., Lord Harris, Lord Inchcape, the Viscount Knutsford, the Marquess of Lansdowne, Sir Walter Lawrence, Lord Lee of Fareham, the Earl of Midleton, Sir Wm. B. Peat, Sir Ivor Phillips, Lord Queenborough, Sir Samuel Scott, the Marquess of Salisbury, Lord Somerleyton, Major-General the Rt. Hon. J. E. B. Seely, the Earl of Scarborough, Sir Arthur Stanley, the Rt. Hon. H. J. Tennant, Lord Edmund Talbot, Sir Edward Ward, Lord Wavertree. The Earl of Midleton, K.P., presided, and made a very eloquent and appreciative and appreciated speech, and was supported by Mr. Winston Churchill and Earl Haig. Sir Alfred Keogh, Sir John Goodwin, and Sir George Makins replied. Probably few dinners during the season have been more thoroughly enjoyed by everyone. The occasion opens certain vistas to the vision. Would it not be good if, instead of the eternal political banquet, our aristocracy would sometimes follow the example set by the members of it mentioned above, and show to the people of Britain that some of them at least remember the existence of those humble things the Sciences and the Arts?

Science and Poetry

On June 4, Colonel Sir Ronald Ross delivered a Friday evening discourse at the Royal Institution on Science and Poetry. He deprecated the notion that "would fasten the blight of Indian caste upon us; that would make us either literary men or scientific men, either business men or professional men, either tinkers or tailors." He reminded the members of the Institution that it has always been interested both in science and art, that it has listened to Coleridge and Campbell on poetry, that Tennyson and Browning attended lectures there, and that it is "not only the right but the duty of the spirit to explore every direction, if only to learn the limits of things." He did not agree with "the dictum that every great poet must be the professional poet, that is a literary man; and that every man of science should concern himself only with test-tubes and microscopes." Still more did he abhor the superstition that every branch in every kind of science should be further subdivided. This was not the teaching of history. Michelangelo and Leonardo da Vinci combined many pursuits. When Peter Paul Rubens was Ambassador in England, an English courtier called upon him and found him seated at his easel. "So His Excellency the Ambassador plays at being a painter," exclaimed the courtier. "No," replied Rubens, "His Excellency the Painter plays at being an Ambassador." Goethe commenced not only a literature, but the theory of evolution. He wondered in what witch's cauldron of folly the absurdity was brewed that poetry and science are enemies. Shelley tasted several sciences; the poems of Coleridge were flowers that peeped out from among the rocks of his philosophy; and Keats had already nearly summed up the matter in his apothegm, "Beauty is Truth, Truth Beauty." Nearly all our great modern poets, especially Arnold, followed science more or less closely. "Indeed, it is never among the greater poets that we notice any antipathy to science. It is the lower type of what may be called literary poetry which, like much of our purely literary philosophy, endeavours to attack science."

Conversely, he said, many men of science have written verses, and sometimes very good verses. Francis Darwin, Edward Jenner, and Sir Humphrey Davy were poets; and the lecturer quoted some fine verses from the last named. He then went on to describe "the encyclopædic course of study" which Davy had undertaken, and referred to his own excursions in similar lines during youth. He attributed these divagations, not to the pursuit of knowledge, nor to vanity, but to the "fury of youth," which makes every young man climb the first mountain he sees, which makes the explorer, the inventor, and the philosopher; which has been implanted in us by the evolution of ages in order to perfect the human race. "It is the force which leads us step by step out of the jungles, ever towards the final godhood of man." He then proceeded to show the connection between science and art, and traced that connection through great literature. "These forces are to the mind what the great Calculus is to Mathematics: Science, the Differential Calculus, which separates, subdivides, and analyses; and Poetry, the Integral Calculus, which sums up."

He had been asked to give some of his own essays; and he proceeded to read various poems from his *Fables* and his *Philosophies*, and traced the evolution of his ideals in literature, science, and philosophy. The last "is derived from Epicurus, through Lucretius, Comte, and Spencer, culminating in the high and pure philosophy of the science of to-day." Regarding his work in medicine, he said. "Personally I much prefer literature, mathematics, and other studies, and am not a biologist, much less a medical man, by any natural proclivity." The lecture will appear in the *English Review*.

ESSAYS

A PSYCHOLOGICAL GEOMETRY (F. R. Hoare, B.A., F.R.A.I.)

It is at first sight somewhat strange that the quantitative and statistical methods from which so much was hoped in biology a generation ago should so far have proved more fruitful and suggestive when applied to the apparently much less measurable phenomena of the mind. Yet the hopes that the early biometricians rested on them have certainly not been realised and their present exponents in biology have had to make practically a fresh start with new groups of phenomena and different objectives. In psychology, on the other hand, their recent developments derive in unbroken succession from the work of such pioneers as Binet, who first employed systematic mental tests and measured the results.

One of the most purely mathematical of these developments was originated by Professor Spearman in a paper published in 1904 in the *American Journal of Psychology*. Its latest and most curious extensions are described in two papers by Mr. Maxwell Garnett published in the *Proceedings of the Royal Society* (A, 96, 1919) and the *British Journal of Psychology* (May 1919) respectively. What follows is largely an account in non-mathematical language of as much of Mr. Garnett's description as can be so expressed.

It is necessary to note the preliminary mathematical assumptions. In the first place, any mathematical analysis of the numerical values obtained by the measurement of mental phenomena must start by expressing any such series of values in terms of a system of factors. These factors may, if we please, be taken to represent mental qualities underlying and accounting for the values. In the absence of further evidence, at least as many factors must be allowed for as there are values measured, and allowance must also be made for the possibility that each value is influenced to some extent by every factor.

A definite test of interdependence is given by the mathematical concept of correlation. Two factors (or qualities) are said to be correlated when their numerical values vary in some sort of relationship with each other (for example, that of simple proportion, though it is commonly a much more complex one); moreover, their degree of correlation can be measured. The aim of mathematical analysis is to find the irreducible minimum of independent factors (factors whose correlation is zero) in terms of which all the values can be expressed. These factors, if they are not by nature or definition constant in value, are called independent variables.

It was argued by Spearman in the paper referred to that, if a series of values fulfils certain mathematical conditions, a first step towards reducing the number of factors can be taken by expressing each value in terms of one special factor (representing a quality contributing to that value and to that alone) and an independent factor (representing some quality of the individual that contributes to all the values). It is said that measurements previously recorded and others specially made (*e.g.* by Burt and Webb) do, in fact, fulfil these conditions, and Dr. Webb is quoted as asserting that, on the basis of a vast mass of material, the existence of the " general factor "

has been established with an exactitude "such as to rival the niceties which physical measurements reveal." This factor has been named "general ability" and is referred to as *g*.

It should be noted here that in handling the numerical element in all these calculations, allowance has to be made (systematically) for the inevitable margin of inaccuracy in the observations on which they are based, and, when a result is said to be exact, it is meant that by the usual statistical calculus it has been shown to fall within an assigned limit of probable error.

To obtain these results a considerable variety of tests is required, for if the tests are confined to a group of similar qualities a discrepancy appears and it becomes clear that some *tertium quid* has ceased to be negligible. It is said that by combining suitable qualities (choosing those whose interdependence is shown by their "high correlations"), and analysing the resulting discrepancy, it is possible to isolate and define the discrepant element. This proves to be a second independent factor, a "group-factor," important within the group but without appreciable effect on other groups of values.

Now, it has been shown that, if certain mathematical conditions are fulfilled, any three values can be expressed in terms of two independent factors and two only. Thus, if within a group of mental qualities three could be found whose values fulfilled these conditions, they could be expressed in terms of the general factor and the group factor alone, without any specific quality entering as a factor into any of them. This provides a more rigorous test for membership of the group. Qualities can be taken and tested in threes in this way until all qualities suspected of belonging to the group have been subjected to the test.

So far it has been possible to speak only in the most general terms of the mathematical side of the discussion. At this point, however, the argument, in Mr. Garnett's hands, receives geometrical treatment, exceedingly interesting and to some extent capable of verbal description. By adopting a certain mathematical artifice (the conversion of measure of correlation into direction cosines) it is possible to represent geometrically both the correlations of qualities and the conditions for the dependence of three qualities on two factors (or qualities) only. Thus, the correlation between two qualities can be represented by the angle between two radii drawn from a point, two qualities with no correlation being represented by radii at right angles to each other. A third quality can be represented by a third radius drawn from the same point but not necessarily in the same plane. It most conveniently happens, however, that the geometrical equivalent of the conditions for three qualities depending on two factors only is that the three radii representing them shall lie in one plane; in this case the correlations between the three qualities taken in pairs are represented by the angles between the first and second, the second and third, and the first and third respectively.

Thus, let us take three correlated qualities, two being specific qualities within a group and one being *g*. Let us suppose that we know mathematically that they depend on two independent factors only, the general factor, or *g*, being one of them and the group-factor being the other, and we wish to find out the nature of the group-factor. If we represent the general factor by one radius we can represent the unknown group-factor by a radius at right angles to it, since by hypothesis they are independent (that is to say, their correlation is zero). We can represent the two specific qualities by two other radii whose angles with the *g* radius will represent their correlations with *g*. Then, if one of these radii lie close to the unnamed radius, there is a strong presumption that the quality it represents is akin to the unknown factor. Such a diagram has been drawn, and on

it a group-factor is found to lie between "Originality" and "Sense of Humour" and to be nearer the latter. On this account it has been named "Cleverness"—not, perhaps, a very happy choice. All qualities within this group are therefore to be regarded as made up in varying proportions of "general ability" and "Cleverness" and of these alone.

A further step can be taken if the radii are drawn with definite lengths, corresponding to the values of the qualities as measured on a uniform scale. It will then be found, pleasingly enough, that lines drawn at right angles from the ends of the radii will all intersect at a point. It should therefore be possible to represent the whole mentality of an individual, so far as it is contained in one group of qualities, by a single point whose position can readily be defined by measuring its distance from the two co-ordinates, that is, from the radii at right angles that represent the general and group-factors respectively. Conversely, if this point be known, it should be possible to ascertain the value of any specific quality in the group by drawing a perpendicular from this point to a radius whose angles with the co-ordinates represent the known and presumably universal correlations of that quality with the general and group-factors.

In other words, we could predict the proportions that different qualities will bear to one another in any given case. Thus, in Mr. Garnett's words, when "Cleverness" (that is, the unfortunately named group-factor) "is at a maximum . . . Sense of Humour is nearly at a maximum. . . . Originality and Quickness are much above the average; but Ability (*g*) is only equal to the average." When Sense of Humour is "the most exceptional quality . . . Cleverness, Originality and Quickness are all much above the average, but Ability is slightly below the average. This does not of course mean that very able men (men with very high *g*) may not have a great Sense of Humour; but only that, the greater their Ability (*g*) the greater must be their Cleverness to produce a given degree of Sense of Humour." Moreover the strength of the group-factor may be estimated from the values of the more measurable specific qualities closely related to it; thus "Cleverness may be recognised in practice—as, for example, when interviewing a candidate for an appointment, to whose general Ability (*g*) testimonials or examination results bear witness—by noting his sense of humour, general tendency to cheerfulness . . . or quickness of apprehension."

It is asserted that two well-marked types of mind are represented by the preponderance of *g* and "Cleverness" respectively. The distinction is equated with William James's division of men of genius into the thinkers, or analysts, on the one hand, and the poets and artists, or men of intuitions, on the other, and is illustrated by Schiller's phrase: "The constraint which your intelligence imposes on your imagination." Other writers also are invoked to confirm the reality of this distinction. Mercier, for example, in an essay on "Cleverness and Capacity," has distinguished between these qualities and argued that, unlike cleverness, capacity can be educated. (Mr. Burt, however, has contended that *g* is innate.) McDougall, in his *Physiological Psychology*, has argued that the mental differences between these types, especially in respect of their methods of reasoning and of associating ideas, are due to differences in the constitution of the brain and of the neural systems.

Dr. Webb has isolated another group-factor, independent of "Cleverness" and general ability. It is said by him to mean "consistency of action resulting from deliberate volition or will," and has been named "Purpose" by Mr. Garnett. Since it is a third "independent variable" we can represent it by a radius perpendicular to the other two co-ordinates, thus constructing a three-dimensional figure. In this figure, by proceeding as before, we can obtain a point in space, measured from the three co-ordinates, that represents the mentality of an individual in respect of two groups of

qualities. This point will lie nearest to the group-axis that represents the group-factor predominating in the person concerned. Two well-marked types of character are said to be denoted by its nearness to the one or the other axis. For example, the type in which "Cleverness" predominates is said not only to possess less "Purpose" (as is shown by lack of thrift, of logic, of conscientiousness, and so on), but in actual fact to be at the same time more witty, cheerful and artistic.

These three independent variables are not likely to be all the factors concerned in the determination of an individual mentality, but it is contended that when they are known the mentality can be defined with very considerable completeness. It is certainly true that to assent to this is not necessarily to underestimate the variety of mental types, for an enormous diversity both qualitative and quantitative can be obtained by combining three independent factors of this kind even when the range of variation of each is small. It is of course irrelevant that three is the largest number of independent variables that can be graphically represented as co-ordinates.

It is not intended to enter here upon a discussion of the validity of these results, but it may throw some light on the possibilities they contain to consider for a moment the lines of speculation that would be opened up if it were assumed that these independent variables had more than a mathematical or mythological existence. In the first place, if they were regarded as psychical entities it is quite certain that it would not be long before attempts were made to identify them with the units of other sciences. The physiologists, assuming them to be hereditary, would give them a footing as full-fledged Mendelian "characters," or in the chromosomes, or perhaps in the secretions of the ductless glands. The sociologists would rediscover them as factors in social evolution, as the causes (or perhaps the effects) of modes of life and types of society. In the second place, the possibility they afforded of immensely simplifying psychological description would make them invaluable for the purpose of classification. It would almost certainly be found that the points in space representing different individuals were not distributed evenly over the diagram but tended to congregate in patches. Thus "types" would become sharply distinguished, each definable in three terms only; it would need but one more assumption to make the classification genetic, and then "hereditary strains" would emerge, out of which "races" would be created just in time to save anthropology from the confusion in store for it when the cephalic index shall have been thoroughly discredited. We should then have the pleasure of seeing the mythical "Aryan race" or the "Iberian" rehabilitated as the race "$x = 7$, $y = 6$, $z = 5$." And, of course, experts on criminals, idiots and children would find their "types" fall into line. Indeed, only the psycho-analysts would be left out of the fun.

REVIEWS

Collected Scientific Papers. By J. H. POYNTING, Sc.D., F.R.S. [Pp. xxxii + 768.] (Cambridge: at the University Press, 1920. Price 37*s*. 6*d*. net.)

THE collected papers of the late John Henry Poynting, Mason Professor of Physics in the University of Birmingham, have been published in book form with a view to perpetuate his memory, a committee formed in 1914 to consider how best to establish a suitable memorial having decided that this was the best form which it could take. A fund was opened for this purpose, and the volume under review, produced with the customary excellence of the Cambridge University Press, is the result.

The work has been edited by two of Prof. Poynting's colleagues, G. A. Shakespear and Guy Barlow, who have corrected a few arithmetical and other mistakes. All the important corrections are indicated in footnotes. The papers have been arranged in groups, so as to bring together all the papers on kindred subjects; within each group the papers have been arranged in chronological order. The popular presentations of subjects previously treated strictly scientifically have, however, been collected together with various addresses and other articles of a popular nature, in the last section of the volume. The papers have been preceded by biographical and critical notices by Sir Oliver Lodge, Sir Joseph Larmor, Sir J. J. Thomson and G. A. Shakespear.

In Part I are collected the papers dealing with the balance and gravitation, of which the most important is the *Phil. Trans.* paper of 1892, "On a Determination of the Mean Density of the Earth and the Gravitation Constant by means of a Common Balance." Poynting's method of determining the mean density was to measure the attraction between two known masses by finding the increase in the weight of one of them when the other was brought underneath it. Merely regarded as a development of the technique of accurate weighing, this work was of great importance. The manner in which disturbing causes were successively tracked down and eliminated should prove very instructive to students of physics. Although the experiments were commenced in 1878, it was not until the end of twelve years' work that Poynting obtained a result with which he was satisfied. By that time, however, he tacitly admitted that the compact torsion-balance apparatus designed by C. V. Boys was more suitable for the purpose. His final determination of the mean density, 5·49, is nevertheless entitled to a relatively high weight in any discussion of the most probable value to be deduced from the various determinations which have been made.

Part II contains the papers on electricity, and of these the two *Phil. Trans.* papers of 1884 and 1885, "On the Transfer of Energy in the Electromagnetic Field" and "On the Connection between Electric Current and the Electric and Magnetic Inductions in the Surrounding Field" are the most important. They form, in fact, Poynting's most valuable contribution to physical science. Their importance is very succinctly summarised by Sir Joseph Larmor: "Nobody before Poynting seems to have thought of tracing the flux of energy in a medium *elastically transmitting* it, and where the whole process is therefore exposed to view. The line of flow is a *ray* in

optics; thus it includes a dynamical aspect of that conception *added on* to and of course consistent with the Huygenian or rather Young-Fresnelian one. The electric and optical ray is implicitly in Maxwell's equations and is only a corollary to them. But in any other kind of elastic transmission, *e g* waves in an elastic-solid medium, a corresponding theory can be worked out I take it this *idea* is Poynting's main contribution, and it clarified many things, especially electrical." Poynting appears not to have noticed that the energy vector is indeterminate theoretically, yet although other forms have been proposed, the balance of evidence seems to be in favour of Poynting's vector, which is generally accepted as correct and has assumed fundamental importance in electrical and optical theory.

The third important series of papers by Poynting were concerned with radiation, the pressure of light and related subjects. These papers are collected together in Part III. Here again, Poynting's delicate manipulative skill, which had been so well shown in his work on the mean density of the earth, proved invaluable. In conjunction with Dr. Guy Barlow he succeeded in establishing the existence of the tangential force produced when light is reflected from a surface on which there is some absorption. They also proved the existence of a torque when light passes through a prism, and further demonstrated the existence of the recoil from light of a surface giving out radiation. Other papers were concerned with certain theoretical aspects of radiation. The most important of these is a *Phil. Trans.* paper of 1903 on "Radiation in the Solar System: its Effect on Temperature and its Pressure on Small Bodies." In the first part of this paper the temperatures of the planets are determined, assuming the fourth-power law of radiation; as regards Mars, Poynting arrived at the conclusion that the temperature of Mars was so low that life as we know it was not possible on its surface. This conclusion was contested by the late Prof. Lowell; Poynting, however, replied to the criticisms (as the present writer considers, successfully) in a later paper, in which he confirmed his previous results. In the second part of the same paper he considers the effect produced by radiation on the orbits of small bodies round the sun and shows that they will be retarded in their motion by the reaction of their exchanges of radiation so that they will ultimately fall into the sun. He also shows that the effect of radiation on the particles of Saturn's rings might make them—if of suitable size—repel instead of attract one another.

The succeeding parts of the volume, dealing with Light, Miscellaneous Papers, Statistics, and Addresses and General Articles, are of lesser value from the permanent scientific view-point, although several of the addresses and general articles contain well-written and interesting accounts not only of those branches of physics in which Poynting had particularly specialised, but also of several related subjects. That there is a certain amount of repetition and overlapping in these is, of course, inevitable. The editors generally decided, however, when in doubt as to whether or not to include a paper, that it was wiser to include it.

Poynting's writings were always clear, simple and lucid. Their publication in book form will be found of great value by students of the science of physics, to which Poynting contributed so much that is of permanent value.

H. S. J.

MATHEMATICS

Mathematics for Engineers: Part II. By W. N. Rose, B.Sc. (The Directly-Useful Technical Series.) [Pp. xiv+ 419, with 142 figures.] (London: Chapman & Hall, 1920. Price 13*s*. 6*d*. net.)

The problem before the author of a text-book on the Calculus for engineers is to reconcile the two necessities of a logical treatment of the subject and the elimination of such purely theoretical matter as is unnecessary for the

practical student. In this respect Mr. Rose, in the second part of his *Mathematics for Engineers*, has been more successful than most of his predecessors.

This second part, with the exception of two chapters on Spherical Trigonometry and Mathematical Probability, is devoted entirely to the Differential and Integral Calculus. Though the volume is complete in itself it assumes a knowledge of certain principles, such as the properties of the Exponential and Hyperbolic Functions, dealt with in the first volume, which are usually comprised in any textbook on the Calculus. This allows the thread of the argument to be better maintained, but prevents the book being so readily comprehended by a student not possessing such previous training.

The author claims that the treatment is based upon algebraic principles, graphical proofs or constructions being utilised for amplification or explanation of the subject. This may have been the author's intention, but we do not feel that the object has been attained. The introduction of the ideas of differentiation and integration is based very largely upon graphical methods —and, as far as that goes, the exposition is clear and adequate—one simple and little-known method of graphical differentiation being given. On the other hand, the development of analytical rules is treated with considerably less care and is not free from error. For instance, in obtaining the rule for differentiating a power of x, the proof given, though valid only when the index of the power is a positive integer, is not qualified in any way, and the result is in fact tacitly assumed for any index. Although allowing that for practical purposes elaborate demonstrations are not necessary or advisable, we believe that the passing over of such points is not to the advantage of even the practical student.

A short treatment of the simpler forms of Differential Equations and of Harmonic Analysis is a useful adjunct.

The most valuable feature of the book is the field covered by the examples, which are chosen from all branches of engineering. A very large number of these are worked out in full, and, besides serving as illustrations, provide a useful training in the application of the Calculus in practice.

The book should prove of considerable value not only to the student of engineering, but also to the practising engineer, as a work of reference. It supplies a need for a treatise on Mathematics from a practical standpoint, comprehensive in its range, whilst omitting the less essential and more academic parts with which the average technical student need not be concerned.

Bevan B. Baker.

A Treatise on the Mathematical Theory of Elasticity. By A. E. H. Love, M.A., D.Sc., F.R.S., Sedleian Professor of Natural Philosophy in the University of Oxford. Third Edition. [Pp. xviii + 624, with 75 figures in text.] (Cambridge: at the University Press, 1920. Price 37*s*. 6*d*. net.)

The second edition of this standard treatise on the theory of elasticity was published in 1906: the first edition was then so substantially altered that practically a new book was the result. No such extensive alterations have been deemed necessary in preparing the present edition, which is essentially the second edition revised where necessary to bring it up-to-date by the incorporation of new researches. The most important of the additions are an appendix to Chapters VIII and IX, which deals with Volterra's theory of dislocations and a new chapter at the end of the book, following the chapter on the general theory of thin plates and shells, dealing with the equilibrium of thin shells. The numbering of the articles in the second edition has been retained, the new articles being specially numbered. This

plan will be found advantageous in classes where various students may have the two editions in use at the same time.

Prof. Love's treatise is so well known that detailed comment on its contents is not necessary. The science of elasticity has so many applications, both in physics and in engineering, that so complete an account of it as is given by Prof. Love is invaluable, not merely as a textbook, but as a standard work of reference. Detailed references to the original authorities are given throughout which will enable students who may desire to pursue any particular matter further to find the relevant literature. Standard works, to retain fully their value, must be kept up-to-date, and it is a matter for general satisfaction that Prof. Love has found it possible to bring out the present third edition.

H. S. J.

ASTRONOMY

The Foundations of Einstein's Theory of Gravitation. By Erwin Freundlich. Authorised English Translation by Henry L. Brose, M.A. Preface by Albert Einstein. Introduction by H. H. Turner, D.Sc., F.R.S. [Pp. xvi + 62.] (Cambridge: at the University Press, 1920. Price 5*s*. net.)

Mr. Brose became interested in Einstein's Theory of Gravitation whilst interned in Germany as a civilian prisoner of war, and there made his translation of Dr. Freundlich's booklet dealing with the foundations of the theory. Dr. Freundlich, of the Berlin Observatory, was the first scientist to endeavour to put the theory to the test: he had planned to take photographs during the total solar eclipse of August 1914, with a view to determining the amount, if any, of the deflections of light rays by the sun's gravitational field. The outbreak of war prevented him carrying out his plans. It is evident, therefore, that he has been interested in the theory from its inception.

His exposition of the fundamental ideas underlying the theory is the best that we have yet seen. He has explained in a very clear manner the two fundamental postulates of continuity and of causal relationship between such things as lie within the realm of observation, and has shown that the principles of classical mechanics cannot be brought into accord with these postulates. It is then shown how Einstein built his theory around this framework. The relationship of the theory to the ideas of Riemann is particularly well brought out. The mathematical development of the theory is sketched but without introducing analysis, so that the volume will be found intelligible to all who are conversant with the methods of reasoning of the exact sciences.

The principal criticism of the work as an exposition of Einstein's theory is that the exact meaning of the interpretation of the fundamental principle of equivalence and of the assumptions involved in it is only very briefly dealt with. It is a difficult matter, however, to discuss the limitations of the principle without introducing the mathematics of the theory.

The present translation has been perused by Dr. Freundlich himself, so that its accuracy can be admitted. Mr. Brose deserves the thanks of scientists in general for making this exposition accessible to English readers.

H. S. J.

CHEMISTRY

Alcohol: Its Production, Properties, Chemistry, and Industrial Applications. By C. Simmonds, B.Sc. [Pp. xx + 574, with numerous illustrations and diagrams.] (London: Macmillan & Co., 1919. Price 21*s*. net.)

Mr. Simmonds's excellent volume on Alcohol has appeared at a very opportune moment, for not only is the eternal controversy between "The Trade"

and Prohibitionists being fought with renewed vigour, but the question of using alcohol as a source of motive power to replace or supplement petrol is one of great and growing importance, and, lastly, the use of methyl and ethyl alcohols in the organic chemical industries is one which demands much attention just now.

The subject is discussed from many points of view, historical, chemical, commercial, and physiological, so that the book should without doubt form a standard work of reference on all matters connected with alcohol.

That Mr. Simmonds has determined to keep right up to date is shown by the fact that a special section is included on methods of production of alcohol from acetylene, though some figures as to yields and costs would have been of value, and it is of interest to note that the Swiss Federal Government are interested in the erection of a plant for the synthesis of about 28,000,000 gallons per annum of ethyl alcohol from calcium carbide. This affords a somewhat striking example of the manner in which modern organic chemistry is capable of revolutionising an age-old industry, and we may yet see the fermentation industries of the world put out of business by the advent of synthetic alcohol, just as the natural indigo and madder industries have been driven out of the world's markets by the synthetic products.

The author has, very wisely, inserted a chapter on the preparation and properties of methyl alcohol which will be of considerable value to those who have to deal with the subject. Chapter VI also, on the analytical chemistry of methyl and ethyl alcohols, will repay study.

The printing and general arrangements are clear and concise, and the text is reasonably free from printer's errors (" diethyl-aniline " on p. 366, line 13, is obviously a misprint for diethyl-amine). The book should be assured of a place in all chemical libraries.

F. A. MASON.

Industrial Gases. By H. C. GREENWOOD, O.B.E., D.Sc., F.I.C. [Pp. xvii + 371, with 23 illustrations.] (London: Baillière, Tindall & Cox, 1920. Price 12*s.* 6*d.* net.)

DR. S. RIDEAL could not have made a happier choice, in his selection of an authority on the above, than the late Dr. Greenwood; and no better memorial to the author could be desired than his treatise on the manufacture and technical manipulation of gases.

Published in November 1919, *Industrial Gases* comes at a peculiarly appropriate time. Great Britain, France, Italy, and America are thinking seriously of their combined nitrogen supplies, and it behoves the future workers of the embryo industry to make themselves *au fait* with the thermodynamic principles forming the basis of technical gas reactions.

Perhaps the most important part of the book is its introduction. This chapter clears up considerably the fog which hovers over one's ideas as to the " specifications " to which a perfect gas should conform. One begins to understand why many important natural gases satisfy only approximately our definitions of a perfect gas; and due account is taken of the corrections to be introduced in our handling of the gases of commerce. The energy requirements for the compression of a gas both isothermally and adiabatically (Boyle's Law is assumed to apply) come up for discussion; while in the sub-section on Heat-Interchange reference is made to the work of Porter, Stanton, and Josse, important inferences being tabulated.

The section on air naturally includes an account of the Joule-Thomson effect, and its application in the Linde process to the liquefaction of air. One cannot help thinking, however, that in the statement of Joule's experiment, the conditions under which the experiment is carried out should be specified. We can obviously make the final temperature very different

from the initial, by giving heat to the gas or taking heat from it. The system during the experiment must be thermally isolated.

Nitrogen, free and combined, is given its due consideration. The relative costs of nitric acid, produced from Chili nitrate, by the arc process, by the cyanamide process, and by the method of Haber, are found to be £20, £12, £12, and £7 per ton respectively. If the last figure were increased by £50 per cent. it would be a much more valuable estimate. The author's low figure for nitric acid via Haber and ammonia oxidation may be accounted for by the fact that in his section on Hydrogen he takes the price of this gas at 1*s.* 9*d.* per 1,000 cubic feet. This figure is believed to be too low, and 2*s.* 6*d.* per thousand would be a nearer approximation. It is to be remembered that the cost of production of hydrogen is a vital factor, and every increase, or reduction, of 6*d.* per 1,000 cubic feet in the cost of the gas affects the cost of synthetic ammonia to the extent of nearly £2 per ton; with a corresponding increase in the nitric acid derived therefrom.

Sections are devoted to oxygen, argon and its allies, and ozone. The oxides of carbon, sulphur dioxide, and nitrous oxide all form interesting sections. The subject of asphyxiating gases is somewhat out of place in a book of this nature; and while all the sections are authoritative, this does not apply when the author enters, to the extent of four pages, the domain of organic chemistry.

While much has been written in recent times on the subject of fuel generally, the concise survey of the manufacture and applications of gaseous fuels will amply repay attention. Nowadays we hear much of nationalisation; and its advocates point to the uneconomic utilisation of our source of power—coal. The author of *Industrial Gases* quotes facts and figures which undoubtedly incline one to the miner's point of view. Our sources of power are not being developed; particularly does this apply to coal-gas; and *Industrial Gases* quotes the work of Hocking as to the gain in efficiency, in furnace operations, in changing from coke to a gas-fired system.

The fixation of atmospheric nitrogen is no longer a problem, and Dr. Greenwood has contributed largely to its solution. In *Industrial Gases* he has placed much of his expert knowledge at the disposal of industry.

G. J. Jones.

The Use of Colloids in Health and Disease. By Alfred B. Searle. [Pp. vii + 120.] (London: Constable & Co., 1920. Price 8*s.* net.)

While there is much in this monograph that may be read with profit, it is obvious that the physiological knowledge of the author is not up to the standard of his chemical and physical knowledge. Those parts of the work which deal with the properties and modes of preparation of colloidal solutions of various kinds are well done. But the numerous errors with regard to biological phenomena suggest that the author is insufficiently equipped to arouse the reader's confidence in the correctness of his interpretations of the effects of colloidal preparations on the living organism. He would be well advised to study an elementary textbook of physiology before publishing a second edition. This being so, his conclusions must be received with caution. Many instances might be quoted, but two or three will serve to illustrate the point. The use of the word "isotonic" in several places is not that universally understood and is indeed unintelligible. The process of digestion as described on p. 42 conveys the impression that the products are in the colloidal state, whereas in fact colloidal food materials are converted into the diffusible crystalloidal form and their passage into the blood-stream made easy. There is only very meagre reference to those phenomena in which colloidal properties as such play an important part, such as the formation of lymph, the secretion of urine, and the maintenance

of the normal volume of the blood. On the other hand, we find such things as the electrical charge of particles, their oxidising and reducing properties indicated as of great importance, although there is at present no satisfactory evidence that this is the case.

It is remarkable that the author appears to be so obsessed with the importance of the colloidal state in itself that he overlooks one unquestionable characteristic of matter in this state, which makes the use of powerful drugs like arsenic, iodine, etc. in colloidal solution of value. This is the comparative slowness of reaction, due to action from the surface only. Thus a small effect may be produced, although exerted for a prolonged time. The fact suggests that what is really active is the very small amount of substance in true solution gradually dissolved by the tissue fluids from the colloidal particles. A more exact account of the work of the late Henry Crookes would have been welcome, especially as the toxic properties of metals were described by Naegeli in 1893. If it were true, as stated on p. 75, that colloidal metal sols are rapidly fatal to bacterial parasites without any action on the host, an immense advance in therapeutics would have been made. The author can scarcely expect the colloidal state to be regarded as the ideal one for administration of alkaloids (p. 105) in view of Dale's work on cocaine, of which he appears to be unaware. Mr. Searle propounds the view that a drug must be converted into the colloidal state before it is efficient, and makes the extraordinary statement that this happens more rapidly by intramuscular than by intravenous injection. Moreover, in this connection, although it is undoubtedly true that all cellular reactions occur in heterogeneous systems, it must not be forgotten that the most powerful actions are exerted on these systems by electrolytes and other substances in true solution.

There are some minor slips which might be corrected in a future edition, such as the attribution to Tyndall of Faraday's discovery of the scattering of light by suspended particles in colloidal solution.

The work on the whole contains much that is of interest, but would be greatly improved by a more critical treatment of the evidence of alleged physiological actions, and especially of the clinical cases, most of which have no value as evidence. It is to be feared that the author accepted without question what he was told.

W. M. B.

Laboratory Manual of Elementary Colloid Chemistry. By EMIL HATSCHEK. [Pp. viii + 135, with 20 illustrations.] (London: J. and A. Churchill, 1920. Price 6*s*. 6*d*. net.)

THE appearance of this little book is significant. It shows that colloid chemistry has at last arrived at that stage in its development in which it is no longer regarded as an exceedingly specialised branch of chemistry, but is in fact to be included in the normal advanced chemical curriculum. The manual is admirably adapted to the purpose which its writer has had in view, namely, "to supply accurate and very detailed directions for carrying out the fundamental operations" of colloid chemistry.

Since the book is the first of its kind, and has been written by one who is himself an authority on the subject, it may be well to indicate its general scope by enumerating the subjects dealt with. These are: Dialysis, ultrafiltration, optical examination, suspensoid sols and suspensions, emulsoid sols and gels, emulsions, cataphoresis, coagulation, protection, viscosity, adsorption, capillary analysis, and the Liesegang phenomenon.

In general only laboratory glass apparatus is required, stress being rightly laid on the necessity of employing resistance glass. The greater part of the course can therefore be given with ordinary laboratory equip-

ment. The larger and more specialised pieces of apparatus are practically confined to the centrifuge, the ultrafilter, and of course the microscope, preferably with an ultracondenser in addition.

No doubt as time goes on practical work in colloid chemistry will go beyond the limits set by the present form of Mr. Hatschek's manual. It would add considerably to its value if the second edition were to contain some directions for carrying out in as quantitative a manner as possible a determination of the rate of coagulation (a subject still very undeveloped), some comparative experiments to demonstrate the use of ultrafilters, something on endosmose, and a few additional notes on cataphoresis, *e.g.* the necessity of using a medium of the same conductivity as that of the sol or emulsion, to maintain constant potential gradient, and the meaning of the iso-electric point and its determination.

The book can be warmly recommended. W. C. McC. LEWIS.

A Course of Practical Chemistry for Agricultural Students. Volume II, Part I. By H. A. D. NEVILLE, M.A., F.I.C., and L. F. NEWMAN, M.A., F.I.C., School of Agriculture, Cambridge University. [Pp. 122.] (Cambridge: at the University Press, 1919. Price 5*s*. net.)

THE exercises presented in this volume form part of a work which when complete will cover the whole of the practical course in agricultural chemistry for students taking the degree in agricultural science at Cambridge. This whole work will be complete in three volumes. Volume I will deal with the chemistry and physics of the soil, and will cover the first year's course. The second year's course on the chemistry of foods is provided for by Volume II. This is to be issued in two parts: the first part, that here considered, deals with the qualitative aspect of pure organic chemistry as far as it is essential for agricultural students; while the second part, not yet issued, will deal more closely with quantitative determinations and analyses of foods. Volume III will deal with more specialised matter, such as the chemistry of fungicides and insecticides, and food preservatives.

The chemistry in the volume under review here is entirely qualitative. The subject-matter includes exercises on the simpler organic groups of substances: alcohols, with phenols; aldehydes, ketones and acids; esters, amides and amines; and carbohydrates and proteins. There are also sections devoted to the questions of purification of organic compounds, enzyme action and the determination of the class to which an organic substance belongs.

After the exercises given in each section of the book a number of notes are added in order to make clear the experiments described in the exercises.

About one-third of the book consists of blank leaves on which the student can add additional exercises or notes. The value of this provision is obvious to anyone who has had experience of practical classes.

There can be no doubt of the value of this book to classes in agricultural chemistry, and the complete work will obviously become the standard laboratory manual in this subject of ever-increasing importance.

W. S.

Practical Physiological Chemistry. By SIDNEY W. COLE, M.A. [Pp. xvi + 401.] Fifth Edition, completely revised and enlarged. (Cambridge: W. Heffer & Sons, 1919. Price 15*s*. net.)

THE fifth edition of this well-known book has been so completely revised and enlarged as to be almost a new book. Three fresh chapters have been added to the earlier portion of the book, so that instead of starting with the proteins, the first chapter is now devoted to the properties of solutions; following on a description of the general properties of colloids comes the only

reasonable account of the subject of "the concentration of hydrogen ions" to be found in any English textbook known to the reviewer. The subject is treated with admirable lucidity both from the theoretical and practical points of view, and descriptions are given of the apparatus and methods employed in measuring hydrogen on concentration by means of indicators; throughout the subsequent chapters this subject is kept before the reader in such a manner as to make him realise its practical bearing in problems of physiological chemistry and biochemistry. Chapter II, entitled "The Proteins," contains substantially the same as the first chapter of the old edition with the exception of the nucleoproteins, which are dealt with in the third chapter together with nucleins and nucleic acid. Chapter IV, which is the only other new one, is entitled "The Preparation and Properties of Certain Amino Acids"; here will be found valuable and in many cases first-hand information concerning the preparation of the more important amino acids set forth with all essential practical details. The arrangement of the remaining ten chapters and appendix more or less follows that of the previous edition, but throughout a number of improved methods have been introduced to replace older ones which have become obsolete; new and valuable features are also sections on bacterial decompositions in the intestine, on autolysis, and on oxidases and peroxidases. While written primarily for medical students, the author has not lost sight of the interests of other workers in biochemistry, and to all of these the book can be confidently recommended as a most valuable laboratory manual and adjunct to their library.

P. H.

Notions Fondamentales de Chimie organique. Par Charles Moureu, Membre de l'Institut et de l'Académie de Médecine. [Pp. viii + 552.] Sixth Edition. (Paris: Gauthier-Villars et Cie, 1920. Price 16 frs.)

In the preface to the first edition published in 1902 the author stated that his object in writing this book was to set forth the principal theories of organic chemistry and to introduce the student step by step to the more important transformations of matter as a preparation for the study of the larger textbooks. It must be acknowledged that the author has been most successful in carrying out the task he has set himself. The book contains a remarkable amount of information for its size and is replete with novel methods of interpreting the mechanism of reactions; the style throughout is simple and the method of presentation is clear, and a perusal of the pages of the book will repay many a teacher. While it cannot be denied that many branches of the subject have been treated somewhat briefly, sound judgment has on the whole been exercised in the selection of the material.

The book is divided into seven chapters dealing with general theories, hydrocarbons, oxygen compounds, nitrogenous compounds, organo-metallic compounds, heterocyclic compounds and dyes respectively, but a mere recital of these conveys a very imperfect idea of the amount of information contained in them. The practice of quoting the names of authors without dates or references is regrettable, but this may, of course, have been necessitated by considerations of space.

P. H.

GEOLOGY

Petrology for Students. An Introduction to the Study of Rocks under the Microscope. By Alfred Harker, M.A., LL.D., F.R.S. Fifth Edition. [Pp. viii + 300, with 99 figures.] (Cambridge: at the University Press, 1919. Price 8*s.* 6*d.* net.)

For more than twenty years this textbook has been widely used in this country, and the part which it has played in the training of so many British

petrologists ensures for this revised edition a hearty welcome. In the eleven years which have elapsed since the last edition appeared, a considerable volume of work on the petrography of the British Islands has appeared, and most of this is incorporated in the edition under review. The increasing importance which is attached to the alkaline rocks has led the author to allot separate chapters to the nepheline-syenites and the phonolites, which formerly were included with the syenites and trachytes respectively. A minor change which will be received with less favour is the transference of the teschenites from the dolerite chapter to the section on gabbros.

So many of the rarer rock-types have been found in this country in recent years that few of them are now unrepresented by British rocks; this has enabled the author to omit many of the descriptions of foreign occurrences, and to replace these by illustrations drawn from local sources. It is a question whether the former has not been carried too far; for example, in the section on diorites, no mention is made of the Cortlandt rocks, some of which have practically attained specific rank. Those parts of the book which are included under the heading "Leading Types," however, form a very complete and concise account of the petrography of these islands.

The section dealing with sedimentary rocks has not undergone such alteration, but the chapters on metamorphism have been to a great extent rewritten. The former gives the reader some impression of the lack of petrographical knowledge concerning sedimentary rocks, especially with reference to the argillaceous types. In this latter connection, more emphasis might have been laid on the occurrence of kaolinitic minerals in fireclays and similar rocks, for there is abundant chemical and microscopical evidence against the view that Hutchings' results are generally applicable.

The illustrations of microstructures, which formed such a useful feature in previous editions, are supplemented by many additional drawings. While the book fulfils adequately its function as "a guide to the study of rocks in thin slices," its utility would be greatly increased by the introduction of chemical analyses of the more important types. Petrology, in the modern sense of the word, includes much more than the mere description of rock sections, and a proper understanding of the relations of the various rock species must be based, as much as possible, on quantitative data, both chemical and mineralogical. A future edition might well contain such data, together with an account of the quantitative microscopical methods which are now coming into vogue.

A. S.

A Handbook of Mineralogy, Blowpipe Analysis, and Geometrical Crystallography. By G. Montague Butler, E.M. [Pp. x + 311, vi + 80, viii + 155, with 89 + 107 figures.] (New York: John Wiley & Sons, 1918. Price 16*s.* 6*d.*)

This book is a reissue, in one volume, of three previously published books on mineralogy, blowpipe analysis, and crystallography respectively. As the last of these was reviewed in this journal recently (Science Progress, **14**, 163, 1919) it need not be further considered at present. In the first part, a brief account of the various mineral species is given in a somewhat tabular form, the details being arranged under the usual headings, such as hardness, colour, fracture, crystal form, and so forth. Only such information as is likely to be useful in the field identification of minerals is included, but the photographs of specimens and the drawings of crystals which accompany many of the descriptions are so small and indistinct as to be quite useless. The author's evident attempt to standardise the nomenclature suggests that, for the sake of consistency, "topaz" and "turquoise" should be translated

into "topazite" and "turquoisite" respectively, especially as such forms as "galenite" are adopted.

In the second part of the book, an account of the methods of blowpipe analysis is given. While this follows the usual lines, the descriptions, despite their brevity, are clear, and form a useful introduction to the subject. The only obvious omission is the general test for silicates; the one which is given in the book is applicable only to those silicates soluble in nitric acid.

In order to be logical, the second and third sections should have preceded the first, as it would be futile for a student to attempt to utilise the data in the latter without, at least, a partial knowledge of the former. As a whole, the book is curiously unequal, the first two sections being much better than the third, which contains far more detail than is required, or is likely to be attained, by the class of student for whom the book is intended.

Misprints are rather numerous, while the errata slip which is inserted at the beginning of the first part refers to the section on Crystallography.

A. S.

BOTANY AND AGRICULTURE

Forests, Woods, and Trees in Relation to Hygiene. By AUGUSTINE HENRY, M.A., F.L.S. [Pp. xii + 314 with 28 photographic illustrations, 21 maps and plans, and 1 other figure in the text.] (London: Constable & Co., 1919. Price 18*s*. net.)

A LESS ambiguous title for this book would have been "The Afforestation of Water-catchment Areas," for it is this subject that forms the writer's main theme and occupies about three-quarters of the text.

It is the introductory chapters which are more especially concerned with the hygienic influences of forests, and these embrace an interesting account of the afforestation of pit mounds, in which due credit is given to the admirable work of the Midland Reafforesting Association, and a rather too brief summary of the influence of forests on climate.

Prof. Henry emphasises the importance of trees in affording shelter from cold wet winds which reduce the vitality of plants and animals alike. Quite rightly, too, the author lays stress on the psychical influence of trees, an aspect too frequently lost sight of in estimating the value of different environments in relation to health. The beneficial effect of trees in towns is probably due much more to this cause than to either their shelter effect or their action as a dust screen.

In reference to the chief subject of these pages, the author has amassed a large amount of statistical and other data, respecting the water-catchment areas of Great Britain, which is set forth in detail. These gathering-grounds number 266 and represent a total area of over 928,000 acres, of which rather less than a fifth is actually owned by the towns concerned or public companies. The universal acquisition, by the respective authorities, of the entire catchment ground is strongly urged as a necessary preliminary to the abolition of all habitations from such areas and their afforestation.

A number of arguments are brought forward in support of this policy, as, for example, the absence of pathogenic bacteria from woodland soils, the reduced chances of pollution by flood water, and the diminution of silting up of the reservoirs. It is also pointed out that the rainfall over woodlands is higher than that over open country. In this connection, however, it must be noted that the local augmenting effect of forest on precipitation diminishes with the altitude, and, as Prof. Henry admits, only the lower parts of the gathering-grounds could be profitably planted up.

The area available for afforestation is estimated at from 10 per cent. to 70 per cent., so that a combination of forestry and grazing would in most cases be necessitated.

With regard to pollution, the forest litter and humus would doubtless serve as a filter for the flood water from the higher ground, but with 30–90 per cent. of the latter devoted to grazing we fail to see how the presence of the woodland belt would in any way mitigate the not infrequent pollution to which the author refers, viz. the presence of the dead bodies of sheep in the streams feeding the reservoirs. With respect to the financial aspect, very little data is given, but from such as is available it would appear that the pre-war cost of planting was from £8–£12 per acre, figures which would need to be more than doubled at the present day. The accumulated debt on this expenditure and the capitalised value of the lost grazing rentals would represent a considerable sum, and even assuming that the present high prices of timber are maintained, it is open to doubt whether Scots pine would show a cash profit, though Douglas fir might well do so, but its growth would be restricted to sheltered spots.

The author's statement that "there is one means by which water-catchment areas can be effectually guarded against pollution and at the same time be put to a profitable use, and that is afforestation," may then perhaps be regarded as rather optimistic.

But the argument for such afforestation rests on the general soundness of the policy and cannot be gauged entirely in terms of the cash return. A national reserve of home-grown timber is undoubtedly necessary, and its location here, as the author shows, would bring with it advantages the actual monetary value of which it is not easy to estimate. E. J. S.

The Fungal Diseases of the Common Larch. By W. E. Hiley, M.A. [Pp. xi + 204 with 73 plates and diagrams.] (Oxford: at the Clarendon Press, 1919. Price 12*s.* 6*d.* net.)

This work, emanating from the Oxford School of Forestry, deals historically and critically with all the undoubted fungal diseases of the common larch, and incorporates the results of investigations, on some of the commoner of these diseases, undertaken by the author, at the instigation of Sir William Schlich, and with the assistance of grants from the Development Commissioners and the Interim Forestry Authority.

The introduction deals with the structure of the normal plant; the larger portion of the book is concerned with the individual diseases, their symptoms, the method of attack of the causative fungus, and also with the special means of preventive action.

Reasons are given for the view that large larch-cankers are caused by infection passing to the main trunk from a dead branch on which the canker fungus is growing saprophytically, and it is suggested that dead branches and branches that are likely to die should be cut off from young trees. Only experience can show whether this is an economical proceeding.

In view of the prevalent idea that calcareous soils increase the liability to canker, one is disappointed at finding no mention of this theory, especially as the author advocates growing larch mixed with beech.

The experiments on pp. 110 and 111 do not seem to prove that bacteria are responsible for the suppression of the heart-rot fungus in unsterilised soil cultures, and it is difficult to understand the ground on which it is stated that soils f. and g. both arable (see pp. 109 and 111) contain no organised remains.

The views, here expressed, that the heart-rot fungus (*Fomes annosus*) and the honey fungus are unable to enter uninjured roots seems well founded, and the discussion as to methods of treatment helpful. In the case of the honey fungus, however, treating stumps with sulphuric acid seems rather drastic, and some evidence of the efficacy, in actual practice, of infecting healthy stumps with harmless fungi would have been welcome.

Leaf diseases are regarded as considerably less harmful than is the case in other conifers because of the deciduous nature of the larch.

The illustrations are numerous and excellent, and the book is written in a clear and interesting manner, but the writer shows a tendency to make too facile interpretations.

E. M. CUTTING.

Peach-Growing. By H. P. GOULD, Pomologist in Charge of Fruit Investigations, Bureau of Plant Industry, U.S. Department of Agriculture. (Rural Science Series) [Pp. xxi + 426.] (New York: The Macmillan Company, 1918. Price $2.00 net.)

IT will come as a surprise to many to learn that the peach is the most important of the stone fruits cultivated in the United States, far surpassing in its annual yield the plum and cherry, which are the next in order of importance. Thus in 1909 the value of the peach crop was nearly $29,000,000, while the plum and prune crop for the same year was worth little more than one-third of this, and the cherry crop only about one-quarter. A large proportion of the peach crop is preserved by drying and canning, and it is in either the dried or canned condition that most of the American peaches that reach this country are brought here.

The works in the Rural Science Series usually reach a high standard, but this work appears particularly well done. After an introductory chapter dealing with the history of the peach and its introduction into America, the extent of the peach industry throughout the world is dealt with. In the third chapter are considered the various factors, environmental and economic, which determine the location of peach orchards, and in the next seven chapters the aspects of peach growing which are more particularly horticultural are discussed in considerable detail. The topics dealt with include propagation, planting, fertility of the soil, tillage, pruning, and the interplanting of crop plants between the fruit trees. A later chapter deals with thinning the fruit.

The chapter concerned with the control of insect and plant pests is a long one, for the peach is attacked by a large number of both animal and plant parasites. Thus the author describes nineteen peach insects and the same number of diseases caused by fungi and bacteria. After a consideration of irrigation, adverse temperatures, the cost of growing peaches and peach varieties, the book concludes with two chapters dealing respectively with picking and packing the fruit and with transportation, storage and marketing.

From what has been said it will be clear that the ground has been well covered, and the book presents the principles and methods of peach-growing both comprehensively and successfully.

W. S.

Manual of American Grape-Growing. By U. P. HEDRIK, Horticulturist of the New York Agricultural Experiment Station. (The Rural Manuals.) [Pp. xiii + 458.] (New York: The Macmillan Company, 1919. Price $2.50 net.)

THE growing of grapes forms now a very important industry in North America, and in some States large tracts of country are occupied by vineyards. As the title indicates, this work is particularly designed for readers in America, where the conditions of grape culture are in general very different from those which obtain in this country. The varieties cultivated in North America are for the most part derived from native American species of grape, and are grown in vineyards out of doors. The cultivation of the European grape under glass is, however, discussed, and the chapters

on the "European Grape in Eastern America" and on "Grapes under Glass" will repay careful perusal by those interested in the cultivation of grapes under glass in this country.

The work deals with the cultivation of the grape in North America in all its aspects. After an interesting chapter on the history of the domestication of the grape, the environmental factors determining the distribution of grape-growing regions are considered. The various horticultural questions of propagation, pruning, training and fertilisers are next adequately dealt with; while further chapters are devoted to pests of grape vines, marketing, products of the grape and grape-breeding. The volume concludes with a long chapter of 118 pages devoted to a description of different varieties of the grape.

The book thus forms a complete manual of grape-growing in America, and constitutes a useful addition to the literature of fruit-growing.

W. S.

On the Interpretation of Phenomena of Phyllotaxis. By A. H. Church M.A. [Pp. 57, with 18 figures.] (Oxford: University Press, 1920. Price 3*s*. 6*d*. net.)

This work constitutes the sixth number of the Oxford Botanical Memoirs edited by Dr. Church. It consists of an exposition of the author's Equipotential Theory of Phyllotaxis, which, as he truly says, "should be capable of transference to the theory of the construction of all lateral growths included in living organisms under the term 'appendages.'"

A summary of the theory is made from the sources in which Dr. Church has more fully stated them, the points in favour of the theory are placed before us, and also the difficulties in the way of its acceptance. A widening of the base of its applicability is also attempted in a short review of the position and formation of the appendages in the Lower Plants, e.g. the Algæ, and also by a consideration of certain features in the organisation of some of the Foraminifera. Attention to these was first drawn by Van Iterson, and the author claims them as falling into line with his general thesis.

The whole forms a very stimulating presentation of phenomena of basal importance in living organisms, and is a valuable addition to this admirable series.

E. M. Cutting.

Productive Agriculture. By John H. Gehrs, B.S., M.S., Associate Professor of Agriculture of the Warrensburg State Normal School, Warrensburg, Mo. [Pp. xii + 444.] (New York: The Macmillan Company, 1918.)

The author has set out to write a book suitable for elementary students which shall treat of agriculture from the point of view of production, so that special stress is laid on such questions as the use of more prolific varieties, the improvement of the soil, the use of pure-bred stock, farm management and economical feeding of stock. The contents are divided into five sections: farm crops, animal husbandry, soils, horticulture and farm management. These questions are dealt with particularly from the point of view of the north-central region of the United States. The book is thus not specially fitted for use in this country; nevertheless, the author has succeeded in including so much fundamental information into a small compass, that the greater part of the book may be read with profit by students of any northern temperate region. The absence of any reference to barley, except in the preface, will probably be regarded as the most serious deficiency from the English point of view.

The book is well and brightly written; there is scarcely a superfluous word in it.

W. S.

ZOOLOGY

The Evolution of the Dragon. By G. Elliot Smith, M.A., M.D., F.R.S. [Pp. xx + 234 with 26 plates.] (Manchester: at the University Press. Price 10s. 6d. net.)

The introduction by Dr. W. H. R. Rivers of a new method which increased enormously the objectivity of the information which can be elicited from primitive people, and its application to Melanesia, established for the first time the complexity of the history of culture in the Pacific and showed how the introduction of a group of customs into an island may be due to small bands of conquerors, sailing about and imposing themselves as a ruling class on the people already there. At the same period Prof. Elliot Smith, during his residence in Cairo, enjoyed a remarkable opportunity of becoming personally acquainted with all the details of the process of mummification throughout the history of that strange custom in Egypt, and was able to study at first hand the associated funeral customs.

Subsequently Elliot Smith was able to show that degraded copies of the characteristic mastaba tomb of the early dynastic Egypt were widely spread round the Black Sea and Eastern Mediterranean, and were connected with the dolmens and chambered tombs of Western Europe and India. The more magnificent royal tombs, the Pyramids, are represented in similar forms in Mesopotamia, India, Cambodia, Polynesia, Peru and Mexico. Subsequent investigations showed that the distribution of these and analogous stone monuments was in reality much wider, covering all those districts where an ancient civilisation is found.

Prof. Elliot Smith was then able to show that the remarkable mummies made in the Torres Straits reproduced exactly the practices of a definite late period, but whereas in Egypt these details had a functional meaning, being necessary for the preservation of a lifelike appearance, in the Torres Straits they are entirely meaningless, because the accidents which they were intended to avoid in Egypt could not occur under the different conditions in Australasia. An example, somewhat gruesome perhaps, but striking, is that in Egypt the embalmers, to restore the shrunken mummy to its lifelike form, stuffed it with mud through slits made in definite positions, the Torres Straits practitioners cut the slits in the skin in the same places, but made no attempt at stuffing. Elliot Smith urged that the great series of parallels between the mummies of Ancient Egypt and modern Torres Straits—all of which in Egypt had a functional explanation, whereas in the East they were useless—was only to be explained by a spread of culture across the globe from Egypt to Australia. This case illustrates the character of Elliot Smith's argument and is of great importance from the standpoint of method.

A few years ago it was believed by all British and American ethnologists that many customs had developed independently to identical form in separated regions. This "evolution of culture attributed to the similarity of the human mind" was supposed to be analogous to biological evolution. We now know that in the evolution of animals very similar forms may evolve independently, but it is characteristic of such homeomorphs that, although the general effect may be similar, the details always differ in the forms of independent origin. In allied animals the details agree, although the forms viewed as a whole may be very different.

Applying this well-established biological principle to culture, we should expect to find that widespread customs, were they of multiple origin, would vary greatly in detail, whilst presenting a uniform appearance. In actual fact we find a diverse appearance built up of detail which is identical from Egypt to Mexico.

In the book under review Elliot Smith shows the absolute identity in

detail of the customs and beliefs associated with a life-giving elixir all over the world, in the Ancient East, in China and India, and in the ancient civilisation of Central America, an identity masked by local differences of artistic expression.

The Chinese Dragon, a great synthetic wonder-beast, built up from a reptile and a bird, with deer's antlers and spots, holding a pearl, living in a rain-cloud and controlling the weather, is shown to have been concocted by syncretism, and by misunderstandings of a whole series of beliefs, all of which separately can be traced back to Western Asia, where their origin can in some part be explained.

The same dragon with the same artistic form associated with the same stories occurs in Central America.

Thus the occurrence all over the world of a vast series of beliefs and customs identical in detail, however much they may appear to differ in general effect, implies a spread of culture from an evolutionary centre in the Ancient East, undergoing changes and acquiring local characteristics in other places and finally distributed practically throughout the world.

This spread can only be brought about by migrations of men, similar to the piratical raids which established Malay kingdoms over the East Indies, and it remained only to discover an adequate motive for these voyages of discovery. That outburst of exploration from Western Europe which in the fifteenth and sixteenth centuries led to the discovery of America and the East Indies, was inspired by a search for spice, for precious stones and for gold, as well as by a love for adventure. Mr. W. J. Perry has, by a most ingenious method, shown that the much earlier voyages which spread civilisation round the world had the same end. He has shown that throughout the continents the old centres of civilisation are on the sites of old gold workings or of washings for precious stones. In those cases where evidence of ancient mining are missing we find that the colony was on the site of a pearl or jade fishery. Primarily these jewels were valued, not for their own beauty, but because they were supposed to contain in abundance that elixir vitæ, without which neither man nor god can live.

Thus the love of gold and precious stones which originated in Egypt or in the Ancient East, and was there founded on their magical uses as carriers of fertility and life, led to their acceptance as currency and provided the material basis on which all civilisations have been built up.

D. M. S. W.

The Physical Basis of Heredity. By Prof. T. H. Morgan. [Pp. 305, with 117 figures. Monographs on Experimental Biology.] (Philadelphia and London: J. B. Lippincott & Co., 1919. Price $2.50 net.)

One of the most fascinating fields of modern biological work is undoubtedly that dealing with the subjects of heredity, particularly from the cytological aspect. Not merely does it possess intrinsic interest, but it is also a branch of investigation that has made enormous strides during the last two decades. The rediscovery of Mendelism in 1900 led to a fresh outburst of experimental breeding and genetics. After a few years it became inevitable that investigators should turn to study the nuclear mechanism for an explanation of the phenomena they were obtaining from their breeding experiments. This line of inquiry yielded results probably far beyond the expectations of its founders, and of the many workers on such problems Professor Morgan and his co-investigators stand in a pre-eminent position. The work has gone on at such a pace and in such diverse places and directions that it has been impossible for the general biologist or student to keep up with it, unless he happens to be working along similar lines, and naturally it will be years before the work is assimilated into ordinary textbooks. Much of it is of fundamental im-

portance to our conceptions of the process of heredity, and no person more fitted than Professor Morgan could have been found to deal with the subject. The zoologists and botanists of the English-speaking world are under a debt of gratitude to the author for the successful way in which he has performed his task of condensing this mass of material and presenting it in a comprehensive way within reasonable limits.

The book, as the reader is informed on the cover is intended as " A college text for use in courses in genetics presenting the fundamental aspects of heredity from the latest developments in the field of biology." Few biologists could be found competent to criticise the subject-matter of this volume, and for ourselves we are content to express our gratitude for having such a multitude of facts presented within so small a compass and with such a wealth of illustration. On the other hand, however, its very catholicity has necessitated a briefness of treatment and terseness of statement that makes it difficult to read. For persons like the general reader and the student, who are not thoroughly conversant with the complex terminology employed, it is hard work to follow the arguments presented. Technical terms are used freely, sometimes with inadequate explanation and sometimes with none at all, and again when, in the opinion of the reviewer, they are unnecessary since they are irrelevant to matter in hand. Again, we find statements of deductions put in brief form as if they were obvious truisms, whereas they are only obvious to those familiar with the work. It must indeed be difficult for the specialist to realise the ignorance of the ordinary reader on details of his own particular line of work, and we fear that Professor Morgan has not taken this factor sufficiently into account in his treatment of the subject.

On p. 63 about two-thirds of the way down in an annoying printer's error—the duplication of a line to the exclusion of another; and in the second line of the table given at the bottom of p. 76 the figures in the end column appear to have been reversed. We also note that in the literature list O is placed after P and that several recent papers referred to in the text are omitted.

These are comparatively trivial matters, however, and we wish to emphasise the excellence of this book and its indispensability to all who wish to make themselves acquainted with the striking advances that have been made in the cytological study of heredity during the past few years.

C. H. O'D.

Inbreeding and Outbreeding. Their Genetic and Sociological Significance. By E. M. East, Ph.D., and D. E. Jones, Sc.D. [Pp. 285 with 46 figures. Monographs on Experimental Biology.] (Philadelphia and London: J. B. Lippincott & Co., 1919. Price $2.50 net.)

This volume deals with the question of Inbreeding and Outbreeding mainly from the point of view of the horticulturist, although certain examples are chosen from the animal kingdom and the general bearing of these interesting problems discussed. The last chapter, about 20 pages, treats briefly of certain aspects of the matter as it is or may be applied to man, more especially in connection with the mixture of races. The book is particularly useful where it deals with the application of pure line selection to the breeding of Indian corn and the increasing vigour that results from heterosis, subjects in which the authors have done a great deal of research.

When we read such statements as " Galton measured the inheritance of groups of individuals to their progenitors and failed [*sic*] because his method could not take into account the true relationship between the germinal constitution and the body characters of an individual," we feel that the outlook of the work is somewhat limited. Indeed the authors' own attitude could almost be summed up in a phrase used in connection with the variability in sexual and asexual reproduction, " He is left with only one reasonable

hypophysis to account for everything, *Mendelian segregation and recombination.*" The book is marred in places by clumsily worded sentences, *e.g.* "There is no question but that animals behave the same as plants in heredity."

It is, however, a useful work, especially from the point of view of plant breeding and in setting forth the experimental results of the subjects it handles. C. H. O'D.

A Laboratory Outline of Embryology, with Special Reference to the Chick and the Pig. By Prof. F. R. Lillie and C. R. Moore. [Pp. 66.] (Chicago: The University of Chicago Press, 1919. Price 35 cents.)

This little book has already proved its value and needs no introduction to embryological laboratories. The present is a revised edition giving directions and a provisional time-table mapping the work out so as to make it cover about sixty hours. It is assured of a continued support.

C. H. O'D.

A Laboratory Manual for Elementary Zoology. By L. H. Hyman. [Pp. 149.] (The University of Chicago Press, 1919. Price $1.50 net.)

This is a manual for use in the practical classes in elementary zoology in the University of Chicago. The course commences with a study of the frog in some detail and then passes on to the lower forms, because, the author says, this method has been found more successful from the point of view of teaching than starting with the lower forms first, and with this we are in entire agreement. It contains full instructions and a number of useful hints. While expressly intended for the work in Chicago, it is not without interest in other colleges where a similar course is carried out.

C. H. O'D.

Problems of Fertilisation. By Prof. F. R. Lillie. [Pp. xii + 278 with 10 figures.] (Chicago: The University of Chicago Press, 1919. Price $1.75.)

This little book is, to use the author's own modest phraseology, a discussion of the problems of fertilisation, and is a result of his own studies in this field. It is a thoroughly interesting and stimulating piece of reading. The past history of the various problems treated is given together with a lucid and critical exposition of our present knowledge on them. As the author points out, it is not possible to treat of these subjects in an ordinary textbook manner, and the book gains much by not attempting to do so. A very just balance is held between what may be termed the biological and physicochemical explanations that have been put forward, and both are subjected to careful examination and criticism. The value of the work is not alone in the actual facts and theories with which it deals, but also in the manner in which it leads up to the problems awaiting solution.

It is unnecessary to call attention to the biological importance of the phenomenon of fertilisation or to the writer's eminence in this field of work, for both are well known. Suffice it to add that this excellent book should be in the hands of all biologists who wish to learn the present position of the investigations along these lines, and also it should be read by all students for the inspiration and suggestions that it contains. C. H. O'D.

Lectures on Sex and Heredity. By F. O. Bower, J. Graham Kerr, and W. E. Agar. [Pp. vi + 119.] (London: Macmillan & Co., 1919. Price 5s. net.)

This little book contains six lectures delivered by the authors at Glasgow University in the years 1917–18. It contains no new facts, but aims at a simple and lucid exposition of certain of the aspects of Sex and Here-

dity. In both plants and animals the origination of sex is carefully traced from such forms as Euglena or Copromonas to the highest examples. This part of the essays, while merely being a re-presentation of old facts, is skilfully written and is well worth reading. The zoologist will be certain to enjoy the section on the growth of pollen grains. The lecture on " Some Modifications of the Reproductive Process as Adaptations to Life upon Land " embodies Professor Graham Kerr's essay previously printed in his volume on *Embryology, Part II, Vertebrates*, and need not be commented upon here. The section on " Heredity in Man " contains the well-known facts about the " Brachydactylous Family," the " Insane Father," and the " Pairs of Brothers." This book will prove of value to the student of zoology as well as to the layman who would like to read about the origin of sex in both plants and animals. J. B. G.

The Buzzard at Home. By ARTHUR BROOK (" British Birds " Photographic Series). [Pp. 15, with 12 plates.] (London: Witherby & Co. Price 3*s*. 6*d*. net.)

THIS book is apparently the first of a new series of photographic books to be brought out by Messrs. Witherby. The text is much shorter and the illustrations fewer than is the case in the beautiful " Home Life " series by the same publishers, but like these it is very readable and admirably produced.

The book is one of a photographic series, and therefore one must assume that the photos are the main feature. If the success of the volume is dependent on these, there can be no doubt as to its achievement. They are excellent, every one of the dozen. The Buzzard must be considered one of our rare birds, and as such, these pictures of the species, male, female, and young, will have a strong appeal to all bird-lovers. The nesting sites chosen are usually of the most inaccessible kind, and Mr. Brook is to be congratulated on having successfully overcome the many difficulties involved.

The text, to be frank, is disappointing. It is pleasingly written, but it adds little to our knowledge of the habits of this interesting bird. The securing of photographs is, of course, an interesting hobby, and the resulting pictures have their appeal to a large circle of bird-lovers and artists; but it seems such a great pity that the collectors of these photographs, after they have taken all the trouble of erecting a hide and surmounting in many cases extraordinary difficulties (the present is an example), are content with this very limited result. There is but a single allusion in the volume to the cries and calls of the species. Notes on the behaviour of the young are very scanty. There is no attempt at a description of the changes of plumage as the birds mature, the rate of growth and so on. The author spent a few hours at the hide one day and a few on another, but of course, like the vast majority of bird photographers, was there only in fine weather. Rain makes good photos impossible, but in many cases it makes the birds themselves more interesting. A night was never spent on the spot, yet the early morning and the late evening are the most instructive hours at the nest. These remarks are, unfortunately, applicable to about 90 per cent. of this type of book. That serious observation can be combined with good photography, and to great advantage, is shown by such notes as those on the Sparrow Hawk by Mr. J. H. Owen, that have from time to time appeared in *British Birds*. The notes are the best of their kind, the photos unsurpassed for beauty and technique. It is only in the exceptional cases such as this that the bird photographer can be said seriously to further our knowledge of ornithology.

That there is a large circle of book-lovers who delight in good photographs and want nothing more is proved by the fact that there are so many books catering to their special taste on the market. To these the present volume can be heartily recommended. WM. ROWAN.

Aquatic Microscopy. DR. ALFRED C. STOKES. Fourth Edition, revised and enlarged. [Pp. ix + 324 and 198 illustrations.] (New York: John Wiley & Sons, 1918. Price 10s. 6d. net.)

THIS is a delightful book which every biologist will read with pleasure. The author aims at providing the young American microscopist with a means of identifying and studying the animals he finds in ponds and pools. Dr. Stokes pleads guilty to "leaping scientific hedges and trampling on scientific classification in a manner that will dismay the learned botanist and zoologist," but the reviewer wishes that more beginners' books were written in such an interesting manner as *Aquatic Microscopy*. Inclined at times perhaps to be discursive and anecdotical, the author gives a good account of the microscope and its parts. He then proceeds to describe some of the aquatic plants, which, from his own experience, serve as haunts for certain desirable aquatic animals. His account of the classificatory differences between desmids and diatoms is rather mixed and obscure.

Throughout the book we find "Keys to Classes and Genera" of various forms: for instance, the author's "Key to Classes and Genera of Aquatic Worms" cheerfully groups together Chironomus larva, Chætonotus, Turbellaria, and Oligochæta. After all, to the beginner the Chironomus larva is as much a worm as Chætonotus. On p. 195 we find an example of the greatest imperfection in this book: the author figures the larva of Chironomus and also the form Chætonotus; he describes the figure of Chironomus larva as "greatly enlarged" (really × 4 or 5), while no clue is given as to the size of the figure of Chætonotus. We recommend that the author rectify this in his new edition. At the end of the volume is a useful glossary. Although this book is written for the American student, it will assuredly commend itself to English microscopists. The illustrations are new and cleverly executed.

J. B. G.

ANTHROPOLOGY

An Introduction to Anthropology. A General Survey of the Early History of the Human Race. By the Rev. E. O. JAMES, B.Litt. [Pp. iii + 259.] (London: Macmillan & Co. Price 7s. 6d. net.)

IT has been remarked that the conflict between "Religion and Science," that is, between many theologians and many scientists, which characterised the latter half of the nineteenth century, has been succeeded by a singular aloofness and indifference on the part of each group of the erstwhile protagonists towards the activities of the other. The clergy continue their good work in their parishes, and the scientists push forward their researches in their laboratories; but neither know much of the doings of the other, and both have lost interest in fundamentals.

Most of the clergy, moreover, have proceeded as though nothing of importance happened in the nineteenth century. It has merely become the fashion quietly to ignore the first chapters of Genesis. This condition of indifference is certainly more deleterious than the worst bitterness of the old controversies. And hence one turns with interest to a work on general anthropology of which the author is a clergyman. And one finds that here is an author who at least does not ignore the issues. "The Old Testament," he remarks, "begins with the time when man became a herdsman, no longer dependent on the chase for his existence. But inasmuch as later cultures can only be rightly understood when viewed in the light of that which has produced them, civilisation being a product of evolution, the theologian as well as the scientist should commence his investigations with a survey of prehistoric man." This is refreshing. And, furthermore, Mr.

James devotes several pages of his long chapter on the "Religion of Primeval Man" to a discussion of the relation between the cruder religions and that in which he believes. Apart from this, the book has the merit that the author displays but little bias, except perhaps in his discussion of monogamy, and unlike many works on anthropology it is not marred by any feverish anxiety to prove a special theory.

The author therefore has the right frame of mind; but unfortunately his knowledge of the principles of zoology and geology is gravely deficient. The introduction deals with the history of the theory of evolution, and the six chapters relate respectively to the origin and antiquity of man, the culture of primeval man, the manners and customs of primeval man, the religion of primeval man, the beginning of civilisation, and the distribution of races. The work thus covers a wide field, both in social anthropology and in physical anthropology and human palæontology. The author designed the work for serious students working for a diploma in anthropology, and the publishers describe it as a "manual for students." In accuracy of information it falls seriously below the standard required by students. The social and archæological sections are superior to those touching upon zoology and geology. In the latter subjects the author blunders badly. He does not understand zoological terminology, and uses such words as species, genus, race, variety, etc., indiscriminately, quite oblivious of the fact that these words have definite and well-understood meanings. This may be the explanation of the glaring and fundamental misstatement on page 215 that it is "the generally accepted view among anthropologists that Pleistocene man was specifically one." Very few anthropologists now hold that view. In geology, too, he fails to grasp elementary facts and principles. He does not seem to understand that the supposed proof of the early Pleistocene date of the Galley Hill skeleton is upset by the supposition that it was a burial; and he quietly raises a long-sunken continent (in the Indian Ocean) to facilitate the migrations of human races. This feat is worthy of the Theosophical Society. There are numerous lesser mistakes.

Mr. James is also unable to write grammatical English. He constantly writes sentences with the subject in the plural and the verb in the singular, and makes other mistakes which a schoolboy should recognise. It is surprising to meet such plural forms as "Rhinoceri" and "Hippopotamuses" (p. 28). Lastly, the carelessness with which the proofs were corrected—or left uncorrected—is execrable.

It is most unfortunate and disappointing that an author who started with such good intentions and such admirable candour did not equip himself with an adequate preliminary knowledge of zoology, of geology, and of the English language.

A. G. T.

MEDICINE

Dementia Præcox Studies. A Journal of Psychiatry of Adolescence. Published by the Society for the Promotion of the Study of Dementia Præcox, 30 North Michigan Avenue, Chicago, Illinois. President of the Society, H. C. Stevens, M.D.; Vice-President, George Mitchell, M.D. Edited by BAYARD HOLMES, M.D. In one volume, with photographs, charts, and tables. [Pp. 272.] Price $5.

THERE is no disease more painfully interesting than insanity, no disease is older, and there are Biblical records of it from Babylonian to early Christian times. No disease is more transmissible in some of its forms, and no disease is more curable in its earlier stages, as statistics prove in regard to those who promptly come under treatment. Short of actual death there is no disease that throws more consternation and terror into the heart of

a family than insanity, and no disease is more costly in its treatment or in its custodial care.

The great waste of mental and nervous energy implied by its incidence and the loss of productive power as well as the enormous expense in its maintenance in this and other countries have set teachers of the young, social economists, and other thinkers, to sound notes of warning as to its prevalence, and our own press has recently urged the establishment of special institutions for its study and the provision of modern means to cure it in its early stages. The fact that there are nearly 130,000 certified insane persons in this country and over 220,000 in the United States of America, of whom about one-fifth are young people between the ages of twenty and twenty-five years—and this proportion appears to be increasing—has created considerable anxiety and alarm. In consequence a Society for the special study of this form of mental disorder has been started in America, and Dementia Præcox, the distinctive term applied to this type, has of late received very definite recognition and attention, particularly as it is a variety that tends to become incurable and chronic, unless diagnosed in its earliest stages and before the symptoms have become confirmed.

It is becoming realised more and more that health rather than material wealth is a people's greatest asset, as upon its health, its sanity, and its vigour depend its progress, its prosperity, and its destiny. Indeed, health and not wealth is the basis of a people's ambitions, aspirations, and achievements.

Griesinger knew of this disease, and Esquirol described it as "acquired imbecility," but it was known as far back as 1672 in this country, when Willis, the anatomist, described its symptoms, and later, in 1772, Sydenham referred to cases presenting definitely diagnostic features of dementia præcox. Nevertheless it was not known to be a recognised entity until Kraepelin gave it the name by which it is now most commonly known. The Medico-Psychological Association in its nomenclature, which is accepted throughout the mental hospitals of Great Britain, refers to it as Primary Dementia, and this term accords better with the onset of the disease, which is a dementia originating and beginning as a first and characteristic illness rather than a dementia occurring in young persons as implied in dementia præcox, for no age is immune to its incidence. Certain symptoms, such as reserve, silence, and depression, occurring in young people and foreshadowing dementia præcox have doubtless been regarded as adolescent melancholia, and certain other symptoms of this disease, such as catatonic stupor and mutism, have been included in the description of catalepsy, whilst the impulsive excitement and automatic violence occasionally met with in these cases have caused the disease to be described as mania, and so it has been mistaken frequently for other conditions, each with a different prognosis.

Thus it is a comprehensive disease. It is believed that not less than one-fifth of all the admissions into American mental hospitals suffers from it. We think that the proportion of one in ten would more accurately represent its general incidence in this country; and yet one-third of the youth of this country certified as insane between twenty and thirty years of age suffers from it; and one in every 100 families has a sufferer from it also. Dementia præcox is thus seen to be a most crippling disease, and one of the first importance. Although the clinical term applied to the disease indicates no etiological or pathological factor, it is nevertheless ascertained to be distinctly inherited, an ancestry of insanity, alcoholism, nervous diseases, or these combined being found to occur in a large proportion of the cases investigated. The Society formed in America for this purpose has been impressed by the fact that 15,000 youths suffer from it annually, and it has therefore endeavoured to encompass three objects: (1) to make a diagnosis possible before mental deterioration appears, which is the only chance of

recovery; (2) to make the treatment for this reason effective; and (3) to make prevention of the disease possible.

Problems of the Mind are always alluring, but they are also elusive. We have had them explained to us in a strange and weird vocabulary from Vienna. The emotions are to-day "ab-reacted," they may become "sublimated" or "converted," and the "affect" is "suppressed" or dissociated. A mysterious endo-psychic censor exercises a constant domination, even during sleep, over the thoughts and emotions, which are sometimes expelled as by a "cathartic." Our mental processes are said to be so intimately connected with the reproductive system that an invariable correlation is believed to exist between sex disturbances and mental disorders, a view which is certainly not within the experience of most British observers. It is maintained by this same group of continental workers and those who support them in this country that even dementia præcox is due to incidents which have been actively forgotten by repression after conflict, and that these memories dominate the unconscious mind and cause a dissociation of the normal "complexes"—a complex being a group of ideas with its emotional tone. The strong desire to realise the natural physiological relation between the mind and the body has been the reason for collecting and publishing the material incorporated in this volume. The Editor points out that in the 400 mental hospitals of the United States, dementia præcox implies an annual expenditure of £5,000,000, that each year contributes a total of 20,000 crippled youths permanently damaged, and that their total number in mental hospitals or asylums is approximately 130,000, each patient being maintained for an average period of fifteen years after his or her admission. The present volume is the first outcome of this serious effort to study one form of mental disease with thoroughness, and probably with one exception—viz. our own study of cancer as an Imperial investigation—the work of this society stands alone, and the volume presented is unique.

Dementia præcox may now claim to be a mental disease responding to a definite clinical picture. It attacks youths, broadly speaking, between the ages of fifteen and thirty years, and these emerge from families whose members belong to all grades of society and who would appear to be otherwise mentally well-endowed. Its characteristic mental symptom is "unemotionalism," and the group is described as a silent army of insane adolescents filing into homes which harbour the hopeless, all the boys and girls passing from apparent mental health into confirmed mental decay.

The volume under review is a faithful record of work done in the chemical, physiological, and psychometric laboratory as well as at the bedside and in the wider fields of morphology and biology; in fact every means of investigation which can throw any light upon the nature of the disease has been employed, not only with the object of relieving the sufferer, but also in the hope of kindling an interest in others and encouraging social workers, scientific explorers in other branches, and inquirers into human welfare also to help and to contribute to the relief of this class, which is believed to swell the list of juvenile suicides, of young delinquents, of much of the nervous breakdown among young people as well as of much maladjustment and human wastage.

It has been already foreshadowed and may now be stated definitely that there are two opposite views as to the cause of dementia præcox. One school holds that all abnormal mental states and disturbed mental conditions are due to some disturbances of the subconscious complexes, that there has been a repression of the natural tendencies and the instincts, giving rise to a conflict of ideas on a group of ideas with their emotions, which has caused a dissociation of the personality resulting in the mental breakdown. The opposing school regards this disturbance as primarily caused by some

organic or structural change within the organ of the mind, viz. the cerebral cortex. The war has tended to the former view and to favour the psychogenic origin of mental disorders, the horrible sights, for instance, and the awful sounds and the terrifying personal experiences having proved too great a mental stress; and the effort to disregard personal danger, to control the will and to neglect and to suppress the natural tendency to self-preservation being eventually outbalanced. Those who take this view assert that mental states such as anxiety and worry are the cause of epilepsy, of recurrent insanity as well as the condition of dementia præcox, but the editors of this Journal do not accept the psycho-genetic origin of mental diseases. They hold strongly to the belief that diseases of the mind are primarily the result of organic disease of the body, and their society has undertaken to support this view by propaganda and the publication of research work embodied in a number of scientific papers which all tend to show that dementia præcox is based upon some organic changes in the nervous tissue.

The Editor, Dr. Bayard Holmes, who is the Director of the Psycho-pathological Research Laboratory in the Cook County Hospital, is the chief contributor. He asserts that dementia præcox is responsible for 25 per cent. of the total admissions into mental hospitals in America, and for 60 per cent. of the total under care, that there is one case in every fifty families, and that many adolescent prisoners in reformatories and penitentiaries suffer from it; also that many vicious, erratic persons and prostitutes are similarly afflicted.

Dr. Holmes considers the diagnosis may be made from the "mummy" attitude, cyanotic cold hands, dilated veins in the upper eyelids and behind the ears, that in these cases adrenalin injected intravenously does not cause a rise of blood-pressure, that the tongue may be increased in size as also the thyroid, that there is a rise of pressure in the cerebro-spinal fluid to 150–300 (from 60–125 normal) millimetres of water, that the automatic excitability shown in association with the spasmo-philic tendency are, with the mental inertia, the mutism, mannerisms and the intellectual decline, pathognomonic symptoms. He agrees as to its hereditary origin, and he points out there is intestinal stasis especially near the cæcum, for which removal of the appendix and intestinal lavage have effected definite relief. There is no mention made by the Editor of a very important chemical, analysis of the brain in cases of dementia præcox made by the late W. Koch. Compared with the normal brain there is, according to this observer, a condition of marked metabolic deficiency in this disease which is expressed in a definite variation in the neutral sulphur fraction, *i.e.*, as sulphur in non-colloidal water-soluble combinations not precipitated by barium chloride direct. Special mention is made of two other papers contributed by Laura Forster and M. Kojima, working in the Claybury Laboratory and based upon some cases among others under the reviewer's care clinically. These authors have both shown that the ovaries and the testes in cases of dementia præcox suffered from parenchymatous degeneration, but whether primary or secondary is uncertain; the possibility is that their physiological insufficiency reacted upon a nervous system already prone to decay.

J. Retinger, employing the method of Abderhalden, which is based upon the discovery of specific defensive ferments in the body; *e.g.* when foreign proteins are introduced into the body, they cause the development of ferments which destroy them. These ferments can be identified by chemical tests; and it is argued similarly, by analogy, that altered functions may also cause the appearance of defensive ferments whose presence can be ascertained by chemical research. In this way a katabolism of the cerebral cortex and of the sex and other glands has given rise to the appearance of defensive ferments within the body.

A joint paper by S. C. Fuller and R. M. Chambers insists that a helpful clinico-diagnostic test of dropping adrenalin into the eye and causing mydriasis enables a distinction to be made between dementia præcox and other forms of insanity, notably from manic depressive insanity.

G. B. Hassin endeavours to correlate physical and mental states, which is in opposition to the teaching of Brodmann, the anatomist.

C. T. La Moure states that a recovery of 2 per cent. occurs in this disease, but Holmes gives the proportion as 10 per cent., which we venture to submit is too sanguine a calculation. La Moure points out that most cases of dementia præcox come from the towns and not from rural districts, and that the average duration in hospital is about sixteen years. Mental tests upon the Rossolimo formula of testing nine central mental faculties, and carried out by H. C. Stevens, show the Will-power to be deficient and that there is an increased suggestibility, a diminished constructiveness, and a weakened power of observation in all cases of this disease. J. R. Ernst suggests Appendicostomy and "colonic" irrigation as a successful surgical treatment, and B. Holmes agrees, but amplifies the treatment by suggesting normal saline intravenously and glucose also, as well as enemata and baths to aid elimination. He also suggests calcium lactate in gr. x doses. He asserts that cases of the stuporose kind recover completely after repeated lumbar punctures, because it relieves the increased pressure of the cerebro-spinal fluid; also the venous engorgement and lumbar puncture helps to re-establish the normal arterial circulation. The treatment mentioned is further supplemented by personal attention to industrial training, so as to kindle an interest and thus neutralise the tendency to mental stagnation and chronicity. There are many valuable suggestions conveyed in the papers collected in the studies. The volume deserves to be read by all who are interested in scientific medicine and to be studied by those whose duty it is to care for and relieve this most hopeless and depressing illness that can afflict the youth and the adolescent.

ROBERT ARMSTRONG-JONES.

The Physiology of Muscular Exercise. By F. A. BAINBRIDGE, M.A., M.D., D.Sc., F.R.C.P., F.R.S., Professor of Physiology, University of London. [Pp. 215, with 22 diagrams.] (London: Longmans, Green & Co. Price 10*s*. 6*d*. net.)

PROF. BAINBRIDGE'S *Physiology of Muscular Exercise* is a well-balanced account of the adaptations of the animal body to increased muscular activity.

It deals with the muscles themselves as well as with the associated and co-ordinated circulatory, and respiratory, changes which accompany the inception, and continuance, of exercise, and follow the return of the organism to the state of rest.

The twelve chapters, each of which is concisely and clearly summarised, are followed by an extensive bibliography.

The book will be attractive to clinicians, and to athletes, who are interested in the physiology of exercise, and may be cordially recommended to such as a clear exposition of the author's views on the subject.

L. S.

Food Poisoning and Food Infections. By WILLIAM G. SAVAGE, B.Sc., M.D., D.P.H. Cambridge Public Health Series. [Pp. viii + 247.] (Cambridge: University Press. Price 15*s*. net.)

THAT food may be the vehicle of poisoning and infection is very well known, but there is, even amongst medical men, much misconception of the real

causes of diseases due to food. Too frequently the investigation of outbreaks is not carried beyond the identification of the offending article of food. This is unfortunate. It is only upon the foundation of a mass of exact evidence as to the actual toxic agents concerned that sound economical methods of prevention of contamination can be built. Dr. W. G. Savage contributes to the Cambridge Public Health Series a monograph on *Food Poisoning and Food Infections* which is an authoritative treatment of this subject, based upon a critical analysis of about 112 outbreaks in this country investigated by the author, and supplemented by the reports of some Continental workers.

Food poisoning is largely bacterial in origin. Dr. Savage finds that the bacteria responsible are almost exclusively of the Gaertner group, though an exception is found in B. Botulinus, the organism of Botulism. The author examines the popular belief that poisoning is due to putrefactive changes in food, but finds no clear evidence to incriminate either the degradation products of protein or the specific toxins of the putrefactive bacteria themselves. Whereas tainted food is possibly quite justifiably suspect, the cause of its harmfulness has not yet been put upon a scientific basis. The popular phrase " ptomaine poisoning " is grossly misleading.

The responsibilities of public health administration are emphasised in chapters dealing with food preservatives, with sources of contamination —both chemical and bacterial—and with methods of investigating outbreaks. Food as a means of transmitting infectious diseases is considered, whilst anaphylaxis is suggested as an explanation of food idiosyncrasy.

The work is written in an attractive way and can be recommended as a clear authoritative guide to this important subject.

R. K. C.

MISCELLANEOUS

Bacteriology and Mycology of Foods. By Fred Wilbur Tanner, M.S., Ph.D., Associate in Bacteriology, University of Illinois. [Pp. vi + 592.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 27*s.* 6*d.* net.)

This work contains a great deal of information useful to those who have to do with the scientific investigation of food problems, and it is undoubtedly a valuable work, but it should be made clear at the outset that there is a great deal more in it about bacteriology than about food. The first ten chapters deal very largely with bacteriological technique, some of the subject-matter being very well presented. The last chapter, the fifteenth, deals with epidemics. Out of the whole of the book there are thus only four chapters actually concerned with the bacteriology of food, and these four chapters occupy less than one-third of the whole book. Milk is fairly adequately dealt with, and between twenty and thirty pages are devoted to eggs. Out of this book of 592 pages, however, less than five pages are devoted directly to meat and meat products, although there are, of course, incidental references to meat elsewhere in the book. Information on the bacteriology and mycology of plant products is confined to the chapter on food preservation in general.

The book will thus be useful to those who have to undertake the examination of food from the bacteriological or mycological point of view. The chief criticism to be levelled at the book is in regard to its title, which appears to be too comprehensive. As it stands, workers on meat, fish, fruit, and vegetables are likely to be disappointed with the contents.

W. S.

Food, its Composition and Preparation. A Textbook for Classes in Household Science. By Mary T. Dowd and Jean D. Jameson, Teachers of Household Science, Washington Irving High School, New York City. [Pp. viii + 173.] (New York: John Wiley & Sons; London: Chapman & Hall, 1918. Price 6*s.* net.)

It is stated by the authors in the preface to this book that it is designed to supplement laboratory work and to give students a clearer conception of the relation between the cost of foods and their nutritive value. It is, however, rather difficult to find evidence in the text of the second of these stated aims of the authors, for what is said on the question of cost of foods does not account for 1 per cent. of the book.

The title of the book would indicate that two subjects are dealt with: the composition of food and the preparation of food. As regards composition of food there is a fair amount of information of the sort exemplified by the following quotation (p. 85): "Butter contains 84 per cent. of fat, about 12 per cent. to 13 per cent. of water, a little curd, and nearly 2 per cent. of salt." The following quotation is still more typical (p. 124): "Lobster is highly prized for its sweet flavor which is due to the large amount of glycogen that it contains. It is very expensive, as about 50 per cent. of it is refuse, and is considered indigestible mainly on account of the coarseness and density of its fibres."

As regards preparation of food, it is definitely stated in the preface that no recipes are given, which means that details in reference to the preparation of food for the table are purposely omitted. This is to be regretted, as the information that is given on the preparation of food is rather general in character, as, for example (p. 95): "Cheese is made from the curd of milk which undergoes processes of 'ripening,' coagulating, removing whey, salting and pressing."

The accuracy and depth of some of the pieces of scientific information presented in this work may be judged from the following citations. "There is very little difference chemically between sugar made from the sugar cane and that made from the sugar beet" (p. 15); "Starch is formed in all plants" (p. 18); "Hot water at first merely causes the starch grains to swell, thereby stretching the cellulose covering until so thin that the water will pass through" (p. 20); "It is desirable that the cell walls of the starch grains be softened . . ." (p. 29).

The two following statements do not appear completely consistent with one another: "In the light of the latest scientific investigations there is no difference in the nutritive value of boiled and unboiled milk" (p. 93); "Such processes as canning, drying, boiling (in some cases, as for example milk), as well as long keeping and too great refining diminish the vitamines" (p. 128). And again compare: "Egg yolk also contains a number of different proteins, including a large percentage of *vitellin* and *lecithin* . . ." (p. 97) with "In the roe is found *lecithin* which is a phosphorized fat" (p. 122).

It is difficult to understand why the authors presume a difference between refrigeration and freezing. Apparently by the former they mean cooling to a temperature just above the freezing-point, the process known in the meat industry as "chilling." In another place a distinction is drawn between cold storage and freezing as methods of food preservation. Presumably frozen mutton is not to be regarded as held in cold storage.

In many places the language might be improved. "It may be soaked over night and baked the same as the navy bean" would stand some modification, nor can one approve of "what is commonly known as vegetables are the plant products that have a very high water content." On p. 150 we have the inevitable "back of"; "Back of each of these natural food choices is a principle."

One wishes that the authors had decided to write a book actually dealing with the preparation of food and had left the scientific treatment of the principles of the study of food to the physiologist and biochemist.

W. S.

Food Supplies in Peace and War. By SIR R. HENRY REW, K.C.B. [Pp. vi + 183.] (London: Longmans, Green & Co., 1920. Price 6*s.* 6*d.* net.)

THIS little book gives an interesting and concise account of the principles which govern the food supply of nations, mainly illustrated in the light of the experiences during the recent World War. It is to be hoped that these pages will be read even more widely by the man in the street than by political economists, for it is particularly the former who will derive the maximum benefit from the study.

Sir Henry Rew is firm in his opinion that British agriculture before the war stood in no need of apology; unfortunately, the prevalent view was that farming in Great Britain was decadent, and only a comparatively few experts shared the author's opinion that the British farmer was more skilful and successful than the foreign farmer, with whom he was sometimes unfavourably compared. The public should be taught such facts, and by reading this volume they will learn to appreciate the magnificent response which the farmers of this country made to the call for increased production.

The whole evolution of the system of Government food control, from its simple beginnings to the final complex machine governing production, distribution, sales, and rationing, is clearly and simply described.

Those who have compared the efficiency of the rationing systems adopted by the various European nations will realise the truth of the author's statement that the compulsory rationing of food in this country was accomplished without serious difficulty owing largely to the good sense and public spirit displayed by the people generally.

From a scientific point of view, many parts of the book are of interest. The fallacy of the assumption that if a supply of wheat is assured all will be well is pointed out, and it is shown that the experience of the Central Empires has taught us that comparatively small deficiencies, such as shortage of dairy products, may seriously affect the health and morale of a nation.

The need for further research on these questions is obvious, and scientists will welcome another voice helping to sound a warning against the neglect of scientific investigations by the State.

J. C. D.

Animal Foodstuffs. Their Production and Consumption, with Special Reference to the British Empire. By E. W. SHANAHAN, M.A., D.Sc. [Pp. viii + 331.] (London: George Routledge & Sons, 1920. Price 10*s.* 6*d.* net.)

THIS volume represents a valuable addition to the series of monographs by writers connected with the London School of Economics and Political Science, and is a skilful and very extensive analysis of an important subject. Apart from a short introduction and historical survey, the book is divided into three parts, which deal with production, consumption, and production and consumption within the British Empire. A great deal of labour has ben spent in compiling the mass of data which the author presents, and he is to be congratulated on having, by means of a careful classification of his material, produced a readable work on a statistical question.

Naturally, it is mainly from the statistical aspect that the subject is viewed, but the book will be a storehouse of information for many who are

interested from other points of view. The author's review of the general economic condition leads him to believe that the comparative lack of undeveloped fertile regions, the increase in white meat-eating population, and the general tendency for the consumption of meat per head to rise, will lead to there being a shortage of animal foodstuffs and a rise in their cost in the near future. He points out, however, that this shortage may be met in part by an increased consumption of fish and dairy produce, and that it may be followed within a decade by a period of comparative abundance, provided favourable conditions present themselves. Turning to the conditions within the British Empire, he shows that the deficiency tends to be even more marked than when the world generally is considered, and that it can only be remedied by a great increase in the labour and capital devoted to agricultural production. Otherwise, we will inevitably become increasingly dependent on foreign sources.

This monograph is a valuable contribution to our knowledge of a most important subject.

J. C. D.

Telephonic Transmission, Theoretical and Applied. By J. G. Hill, Assistant Staff Engineer, General Post Office, London. [Pp. xvi + 398, with 196 diagrams and illustrations.] (London: Longmans, Green & Co., 1920. Price 21*s.* net.)

The erection of a telephone circuit between any two places at the present day resolves itself into much more than the mere provision of a pair of connecting wires between the two instruments: it requires extensive preliminary calculations to determine not only the sizes of wire and the form of the circuit, but also to predetermine its speaking qualities.

This book by Mr. J. G Hill, Assistant Staff Engineer, General Post Office, London, treats of such design, not merely from the purely theoretical standpoint of the propagation of the telephonic currents along the lines, but also from the practical point of view which involves considerations of cost and economy, and of the elements of standardisation which are so essential in a large system, but which necessarily often introduce modifications into the theoretically best solution of any given problem. The book therefore differs considerably from others dealing with the theory of such transmission, and its arrangement is consequently somewhat unusual.

For instance, the author first deals with the case of direct current transmission along an infinite line, and it is not until Chapter V that the essential features of telephonic transmission—depending as it does upon the use of alternating currents of many frequencies—are introduced.

The author has endeavoured to dispense as far as possible with the use of "higher mathematics" in the treatment of his subject, and thus to overcome some of the mysteries of differential equations and similar functions, and to substitute in their place a simpler treatment. This treatment, together with the more classical method of developing the transmission formulæ, the author has relegated to appendices—an arrangement which, in the reviewer's opinion, does not tend to enhance the clarity of the book, since one at least of these appendices must be read in its place in the opening paragraphs of Chapter II in order to obtain a connected account of the subject.

In Chapters VI to IX the formulæ particularly applicable to telephone lines are developed in detail, and the method of calculating the various transmission constants of such lines is given, while in Chapter IX some fifty-five pages are devoted to the problems of loaded telephone lines particularly with reference to British Post Office practice. Chapters X and XI cover very important ground in describing various methods of measurement of the constants of telephone lines, and the uses of Standard Cable in such

measurements. As regards the measuring apparatus described, it is perhaps a pity that so little attention is given to the triode valve in this connection. The description of these valves is left to the last chapter of the book, and there their many applications in the laboratory and test-room are barely touched upon.

From the point of view of the practical engineer, Chapter XII is perhaps the most important one in the whole book, as there the effect of "cost problems" upon the construction of telephone lines is treated extensively. Apart from its other good points, this chapter alone would make the book valuable.

The development of a satisfactory telephone repeater has long taxed the ingenuity of telephone engineers, and the modern development of the triode valve bids fair to provide a satisfactory solution. This aspect of the subject of telephonic transmission is dealt with in the last chapter, but here the author seems to be treading on less familiar ground. For instance, the statement at the foot of p. 345 would rather imply that *any* negative voltage greater than three or four volts could be used on the grid of a triode amplifier for telephone work. With a large negative voltage, however, it would seem that the repeater could scarcely be distorionless. It would also be interesting to know the author's authority for the statement on p. 356, that the "thermionic relay does not transmit such low frequency signals [17 periods per second]." It is usually understood that a triode will amplify currents of any frequency from zero upwards.

The diagrams throughout the book are clear, and a number of useful half-tone plates are also included, while the general appearance of the book is good, although the formulæ would look better if they had been printed throughout in a uniform fount of type. The volume is the first of a series of similar handbooks which are in course of preparation, and if the whole of the series maintains the standard set by this one, it will indeed be a valuable one.

PHILIP R. COURSEY.

The Foundations of Music. By H. J. WATT, D.Phil. [Pp. xvi + 239, with 10 illustrations.] (Cambridge: at the University Press, 1919. Price 18*s.* net.)

IN this book the author's treatment is fullest when dealing with concords. Thus, octaves, fifths, consecutive fifths and their prohibition, common chords, etc., occupy over half the entire work.

In discussing any controversial matters the various authorities on the subject, ancient and modern, from Aristotle to Tschaikovsky, are carefully cited. Although no entire solution is propounded for some of the age-long problems here surveyed, the facts of the case are marshalled with care and fullness and accordingly a noteworthy step is taken towards grappling with their difficulties.

Near the beginning of the book occurs a somewhat striking feature. This is what the author terms his "volumic" theory of tone. Thus we find the following passages.

P. 6: "Volume is properly used to distinguish that difference between tones of different pitch that makes the low tone great, massive, all-pervasive, and the high tone small, thin, and light."

P. 8: "The attributes of tones thus far enumerated are: quality, intensity, volume and pitch. The relations between these four are an important problem."

P. 9: "We may assume that pitch holds a central position in volume. And, as pitch is ordinal, while volume suggests a volume of parts or particles, we may go on to assume that pitch is constituted by a specially

prominent or noticeable part of the volume of sound that makes up a tone."

On p. 10 occurs fig. 1, which represents by a series of horizontal lines the volumes of the tones in an ascending series. The left end of each line is called the lower end of volume, the right end the upper end of volume. In all the lines the right or upper ends are perpendicularly over one another. The long lines representing the large volumes (or lower tones) are below and the shorter lines representing the smaller volumes (or higher tones) are above in order. Each line has a large dot (or pitch point) at its middle point to indicate the pitch of the tone in question.

If, on the above principle, a diagram is made for tones an octave apart, it is pointed out that the lower end of the line for the volume of one tone lies just above the pitch point of the tone an octave below.

In this way it is sought to explain the degree of consonance of various intervals.

Taken in a fanciful or metaphorical sense these notions may prove attractive and suggestive to readers of a certain type, and so possibly serve the chief end for which they were advanced. But it cannot be admitted that this volumic theory of tone, taken literally as stated, shows any correspondence with the crucial facts of the case, with which indeed it is in direct conflict. Hence, whatever merit this theory may possess on the imaginative side of the question, it can scarcely be said to form any contribution to the *science* of music.

The book closes with three chapters on outlines of instruction, the objectivity of beauty and æsthetics as a pure science. At the end, in addition to the usual indices of subjects and authors, there is a list of the eighty-three works cited.

E. H. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Euclid in Greek. Book I, with Introduction and Notes by Sir Thomas L. Heath, K.C.B., K.C.V.O., F.R.S., Sc.D., Hon. B.Sc., Oxford. Cambridge: at the University Press, 1920. (Pp. vii + 238.) Price 10*s*. net.

A book of value and interest to all scientific readers. One never knows how much in the ordinary school books of Euclid is due to the author or to the editor, and of course the Greek text is not available for more than a few. Here we have the First Book set out in full, together with a valuable historical introduction and admirable notes by a very distinguished editor.

Qualitative Analysis in Theory and Practice. By P. W. Robertson, M.A., Ph.D., Professor of Chemistry, Victoria University College, New Zealand, and D. H. Burleigh, A.R.C.S., D.I.C., B.Sc., Science Master at Eltham College. London: Edward Arnold, 1920. (Pp. 62.) Price 4*s*. 6*d*. net.

Logica Matematica. By C. Burali-Forti. Professore ordinario di Geometria analitico-proiettiva nella R. Academia Militare di Torino. Seconda edizione intieramenti rifatta. Milano: Ulrico Hoepli, Editore-Libraio della real Casa, 1919. (Pp. xxxii + 483.) Price L. 9.50.

The Mathematical Theory of Electricity and Magnetism. By J. H. Jeans, M.A., F.R.S. Third Edition. Cambridge: at the University Press, 1915. (Pp. vi + 587.) Price 17*s*. 6*d*. net.

Lectures on the Theory of Plane Curves. Delivered to Post-Graduate Students in the University of Calcutta. By Surendramohan Ganguli, M.Sc., Lecturer in Pure Mathematics, University of Calcutta. Calcutta: published by the University, 1919. (Part I, pp. x + 138; Part II, pp. xiii + 139 + 350.)

An Elementary Treatise on Differential Equations and their Applications. By H. T. H. Piaggio, M.A., D.Sc., Professor of Mathematics, University College, Nottingham. London: G. Bell & Sons, 1920. (Pp. xvi + 216 + xxv.) Price 12*s*. net.

Report on the Quantum Theory of Spectra. By L. Silberstein, Ph.D. London: Adam Hilger, 75*a* Camden Road, N.W.1, 1920. (Pp. 42.) Price 5*s*. net.

The Sumner Line or Line of Position as an aid to Navigation. By George C. Comstock, and Black Reduction Forms for Line of Position Observations. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. 70.) Price 6*s*., and Reduction Forms 40 cents net.

A Primer of Air Navigation. By H. E. Wimperis, M.A. (Cantab.), Head of Air Navigation Research Section, Air Ministry. London: Constable & Co., 10 Orange Street, Leicester Square, W.C.2, 1920. (Pp. xiv + 128.) Price 8*s*. net.

Petit Atlas Céleste, comprenant cinq cartes en deux couleurs, précédé d'une introduction sur les constellations, sur les moyens de les reconnaitre, etc. By M. G. Bigourdan, Membre de l'Institut et du Bureau des Longitudes. Deuxième édition. Paris: Gauthier-Villars et Cie, Éditeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, Quai des Grands-Augustins, 55, 1919. (Pp. 56.) Price 2 frs.

Australian Meteorology: A Textbook including sections on Aviation and Climatology. By Griffith Taylor, D.Sc., B.E. (Syd.); B.A. (Cantab.), F.G.S., F.R.G.S., Lecturer at the Commonwealth Flying School. Oxford: at the Clarendon Press, 1920. (Pp. xi + 312, with 229 figures.) Price 12*s*. 6*d*. net.

The Principles of Aerography. By Alexander McAdie, A. Lawrence Rotch, Professor of Meteorology, Harvard University, and Director of the Blue Hill University. London: George G. Harrap & Co., 9 Portsmouth Street, Kingsway, W.C. (Pp. xii + 318, with 111 figures.) Price 21*s*. net.

Trattato di Fotometria Sorgenti artificiali di luce e materiali illuminanti. By A. Coacci, addetto al gabinetto speriment, dell' Ufficio Tecnico dei Fari-Napoli. Milano: Ulrico Hoepli, Editore-Libraio della real Casa, 1920. (Pp. vii + 249.) Price L. 8.50.

Le Origini Neolatine a cura del Prof. P. E. Guarnerio. By Paolo Savj-Lopes. Milano: Ulrico Hoepli, Editore-Libraio della real Casa, 1920. (Pp. xiv + 407.) Price L. 10.

A Handbook of Physics Measurements. By Erwin S. Ferry. In collaboration with O. W. Silvey, G. W. Sherman, Jr., and D. C. Duncan. Vol. I, Fundamental Measurements, Properties of Matter and Optics. (Pp. ix + 251.) Vol. II, Vibratory Motion, Sound, Heat, Electricity and Magnetism. New York: John Wiley & Sons; London: Chapman & Hall, 1918. Price 9*s*. 6*d*. net each vol.

Intermediate Textbook of Chemistry. By Alexander Smith, Head of Department of Chemistry, Columbia University. London: G. Bell & Sons, 1920. (Pp. vi + 520.) Price 8*s*. 6*d*. net.

Practical Physiological Chemistry. By Sydney W. Cole, M.A., Trinity College, Cambridge. Fifth Edition, with an Introduction by Prof. F. G. Hopkins, M.B., D.Sc., F.R.C.P., F.R.S., Professor of Biochemistry. Cambridge: W. Heffer & Sons, 1919. (Pp. xvi + 401.) Price 15*s*. net.

Quantitative Analysis by Electrolysis. By Alexander Classen, with the co-operation of H. Cloeren. Revised, rearranged, and enlarged English edition. By William T. Hall, Associate Professor, Massachusetts Institute of Technology. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xiii + 346.) Price 17*s*. 6*d*. net.

Fuel Production and Utilization. By Hugh S. Taylor, D.Sc. (Liverpool), Assistant Professor of Physical Chemistry, Princeton University, Princeton, New Jersey, U.S.A. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1920. (Pp. xiv + 297, with 17 figures in text.) Price 15*s*. 6*d*. net.

A Foundation Course in Chemistry for Students of Agriculture and Technology. By J. W. Dodgson, B.Sc. (Lond.), A.I.C., Lecturer in Chemistry at University College, Reading, and J. Alan Murray, B.Sc. (Edin.), Lecturer in Agricultural Chemistry at University College, Reading. Second Edition. Thoroughly revised, with new chapters. London: Hodder & Stoughton; and Toronto and New York, 1920. (Pp. xii + 240.) Price 6*s*. 6*d*. net.

Histoire de la Chimie. By Maurice Delacre, Membre de l'Académie Royale de Belgique, Professeur à l'Université de Gand. Paris: Gauthiers-Villars et Cie, Quai des Grands-Augustins, 55, 1920. (Pp. xv + 632.)

Practical Plant Biochemistry. By Muriel Wheldale Onslow, formerly Fellow of Newnham College, Cambridge, and Research Student at the John Innes Horticultural Institution, Merton, Surrey. Cambridge: at the University Press, 1920. (Pp. iii + 178.) Price 15*s*. net.

An Introduction to Palæontology. By A. Morley Davies, D.Sc., A.R.C.S., F.G.S., Honorary Fellow of the Royal Geographical Society, Lecturer in Palæontology, Imperial College of Science and Technology. London: Thomas Murby & Co., 1 Fleet Lane, Ludgate Circus, E.C.4, 1920. (Pp. xi + 414, with 100 figures.) Price 12*s*. 6*d*. net.

Invertebrate Palæontology. An Introduction to the Study of Fossils. By Herbert Leader Hawkins, M.Sc., F.G.S., Lecturer in Geology, University College, Reading. London: Methuen & Co., 56 Essex Street, W.C., 1920. (Pp. xix + 226, with 16 illustrations.) Price 6*s*. 6*d*. net.

Forest Products: Their Manufacture and Use. Embracing the Principal Commercial Features in the Production, Manufacture, and Utilisation of the Most Important Forest Products other than Lumber, in the United States. By Nelson Courtlandt Brown, B.A., M.F., Professor of Forest Utilisation, the New York State College of Forestry at Syracuse University, Syracuse, New York; Trade Commissioner, United States Lumber Trade Commission to Europe, Department of Commerce, Washington, D.C. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xix + 471, with 120 figures.) Price 21*s*. net.

An Account of the Herbarium of the University of Oxford. By Prof. S. H. Vines and G. C. Druce. Oxford: at the Clarendon Press, 1897. (Pp. 55.) Price 2*s*. 6*d*. net.

The subject matter of this little volume is divided into two parts, the first of which is a reprint of the "Account" published in 1897. This, it may be remembered, gave a brief epitome of the principal herbaria which the collections comprise. They include those of Bobart (*circ.* 1670), Dillenius, Sibthorp, Fielding, the British Herbarium initiated by Lawson, and the Cryptogamic Herbarium. Part II enumerates all the more recent additions, and is mainly occupied with a list of the contributors. In addition, there are notes on other Oxford herbaria, and the whole forms a useful index for intending students.

New Zealand Plants and their Story. By L. Cockayne, Ph.D., F.L.S., F.R.S., Corresponding Fellow Botanical Society of Edinburgh, President of the New Zealand Institute, Hector Memorial Medallist and Prizeman in Botany. Second Edition, rewritten and enlarged. Wellington, N.Z.: By Authority, Marcus F. Marks, Government Printer, 1919. (Pp. xv + 248, with 99 photographs and 14 text-figures.)

An Introduction to the Study of Cytology. By L. Doncaster, Sc.D., F.R.S., Fellow of King's College, Cambridge, Derby Professor of Zoology in the University of Liverpool. Cambridge: at the University Press, 1920. (Pp. xiv + 280.) Price 21*s*. net.

The Heron of Castle Creek, and other Sketches of Bird Life. By Alfred Wellesley Rees, with a Memoir of the Author by J. K. Hudson and a Portrait. London: John Murray, Albemarle Street, W., 1920. (Pp. 216.) Price 7*s*. 6*d*. net.

Cytology, with Special Reference to the Metazoam Nucleus. By W. E. Agar, Professor of Zoology in the University of Melbourne. London: Macmillan & Co., St. Martin's Street, 1920. (Pp. xii + 224.) Price 12*s*. net.

A Junior Course of Practical Zoology. By the late A. Milnes Marshall, M.D., D.Sc., M.A., F.R.S., and the late C. Herbert Hurst, Ph.D. Ninth Edition. Revised by F. W. Gamble, D.Sc., F.R.S. London: John Murray, Albemarle Street, W., 1920. (Pp. xvii + 517.) Price 12*s*. net.

La Telefonia Senza Filo. By Umberto Bianchi. Milano: Ulrico Hoepli, Editore-Libraio della real Casa, 1920. (Pp. viii + 296, with 194 figures and 6 plates.) Price L. 10.

The Propagation of Electric Currents in Telephone and Telegraph Conductors. By J. A. Fleming, M.A., D.Sc., F.R.S., University Professor of Electrical Engineering in the University of London. Third Edition, revised and extended. London: Constable & Co., 10 Orange Street, Leicester Square, W.C., 1919. (Pp. xiv + 370, with 19 figures.) Price 21*s*. net.

Aviation: Theorico-Practical Textbook for Students. By Benjamin M. Carmina, Assistant Chief Instructor at the Y.M.C.A. Airplane Mechanics' School, Charter Member and Lecturer of the Aeronautical Society. New York: The Macmillan Company, 1919. (Pp. xi + 172.) Price 11*s*. net.

Cement. By Bertram Blount, F.I.C. Assisted by William H. Woodcock, F.C.S., Memb. Soc. Pub. Anal., and Henry J. Gillet. (Pp. xii + 283, with diagrams and illustrations.) Price 18*s*. net.

The Theory and Practice of Aeroplane Design. By S. T. G. Andrews, B.Sc. (Engineering), London. Member of the Institute of Aeronautical Engineers, Consulting Engineer, and S. F. Benson, B.Sc. (Engineering), London. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1920. The Directly-Useful Technical Series, founded by the late Wilfred J. Lineham, B.Sc., M.Inst.C.E. (Pp. xii + 454, with 305 figures.) Price 15*s*. 6*d*. net.

The Amount of Steam used by Steam Jets. By D. Brownlie, B.Sc., F.C.S., A.I.Mech.E. Reprinted from "Engineering," January 16, 1920. London: Brownlie & Green, 2 Austin Friars, E.C.2. (Pp. 14.) Price 1*s*. 6*d*. net.

Proposal for a Network of Wireless Communications to serve the Needs of the whole British Empire. Submitted by Marconi's Wireless Telegraph Co., Ltd. February 1920. (Pp. 20.)

Thermodynamics for Engineers. By J. A. Ewing, K.C.B., M.A., LL.D., D.Sc., F.R.S., M.Inst.C.E., M.Inst.Mech.E., Principal and Vice-Chancellor of the University of Edinburgh. Cambridge: at the University Press, 1920. (Pp. xii + 383.) Price 30*s*. net.

The Engines of the Human Body. Being the Substance of Christmas Lectures given at the Royal Institution of Great Britain, Christmas, 1916–17. By Arthur Keith, M.D., LL.D., F.R.S. London: Williams & Norgate, 14 Henrietta Street, Covent Garden, W.C.2, 1919. (Pp. xii + 284, with 2 plates and 47 figures.) Price 10*s*. 6*d*. net.

The subject of this book is well indicated by the title, and the distinguished author has done his task extremely well. The mechanical nature of many, if not of all, the processes of life should be prominently before students and the public.

Sleeping for Health. By Edwin F. Bowers, M.D. London: George Routledge & Sons, Broadway House, 68–74, Carter Lane, E.C. (Pp. vii + 128.) Price 2*s*. 6*d*. net.

A philosophical little book on the fine art of sleeping, containing not only much good advice on health matters for all readers, but also some amusing anecdotes of the experiences of some historical characters on the subject, calculated to keep one awake.

Practical Histology. By J. N. Langley, Sc.D., LL.D., F.R.S., Professor of Physiology, and formerly Lecturer on Histology in the University of Cambridge. Third Edition. Cambridge: W. Heffer & Sons, 1920. (Pp. viii + 320.) Price 10*s*. 6*d*. net.

A number of changes have been made in this edition, including some recent improvements in methods and some alterations made to meet small difficulties which have been experienced in the class-room, as shown to be felt by students. The changes include a large chapter on a course of practical histology for elementary students in Cambridge, and a small chapter for more advanced students and assistants in preparing demonstration specimens.

Aids to the Mathematics of Hygiene. By R. Bruce Ferguson, M.A., M.D., B.C. (Cantab.), D.P.H. (Eng.), etc. Fifth Edition. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1919. (Pp. xii + 186.) Price 3*s*. 6*d*. net.

This new edition contains a valuable chapter on Energy, Exercise, and Diet, which is so lucidly put that it will be of value to everyone, clinicians included. There is also a note on the new method of recording barometrical observations. The whole book is very well done.

Diseases of the Heart. By Sir James Mackenzie, F.R.S., M.D., F.R.C.P., Physician to the London Hospital. Third Edition. Second Impression revised. London: Oxford University Press and Hodder & Stoughton, 1918. (Pp. xxiii + 502, with 264 figures.)

The Blind: Their Condition and the Work being done for Them in the United States. By Harry Best, Ph.D. New York: The Macmillan Company, 1919. (Pp. xvii + 763.) Price $4.

The Physiology of Vision, with special reference to Colour Blindness. By F. W. Edridge-Green, M.D., F.R.C.S., Oculist London Pensions Board. London: G. Bell & Sons, York House, Portugal Street, Kingsway, W.C.2, 1920. (Pp. xii + 280, with 23 figures, and card test for colour blindness in leather case.) Price 12*s*. net; card test in case 25*s*. net.

The Link Between the Practitioner and the Laboratory. A Guide to the Practitioner in his Relations with the Pathological Laboratory. By Cavendish Fletcher, M.B., B.S. (London), M.R.C.S., L.R.C.P., and Hugh McLean, B.A., B.C. (Cantab.), D.P.H. (Camb.), M.R.C.S., L.R.C.P. London: H. K. Lewis & Co., 1920. (Pp. 91, with 7 illustrations.) Price 4*s*. 6*d*. net.

Handbook of Physiology. By W. D. Halliburton, M.D., LL.D., F.R.C.P., F.R.S., Professor of Physiology, King's College, London. Fifteenth Edition (being the 28th edition of Kirke's Physiology), with nearly six hundred illustrations in the text, many of which are coloured, and three coloured plates. (Pp. xix + 936.) Price 18*s*. net.

The Principles of Ante-Natal and Post-Natal Child Physiology, Pure and Applied. By W. M. Feldman, M.B., B.S. (Lond.), Assistant Physician to and Lecturer on Child Physiology at the Infants' Hospital. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. xxvii + 694, with 6 plates and 129 illustrations.) Price 30*s*. net.

Transactions of the Congress of American Physicians and Surgeons. Eleventh Triennial Session held at Atlantic City, New Jersey, June 16 and 17, 1919. New Haven, Conn., U.S.A. Published by the Congress, 1919.

Microscopy: The Construction, Theory, and Use of the Microscope. By Edmund J. Spitta, L.R.C.P. (Lond.), M.R.C.S. (Eng.), F.R.A.S., F.R.M.S. London: John Murray, Albemarle Street, W., 1920. (Pp. xxviii + 534, with 83 half-tone reproductions from original negatives and 255 text illustrations.) Price 25*s*. net.

The Road to En-dor. Being an Account of how two Prisoners of War at Jozgad, in Turkey, won their way to Freedom. By E. H. Jones, Lieut. R.A.F. London: John Lane, The Bodley Head, W.; New York: John Lane Company, 1920. (Pp. xiii + 351.) Price 8*s*. 6*d*. net.

Lieut. Jones and Lieut. Hill escaped from a prisoners' camp in Turkey by practising the arts of spiritualists and conjurors on certain officials. The author not only admits the *leger de main*, but also shows the manner in which it was done, much to the amusement of the reader; and one purpose of the work is really to expose this form of imposition, the prevalence of which in Anglo-Saxon countries tends to lead foreigners to believe that we are as superstitious to-day as we were in the Middle Ages.

The Realities of Modern Science: An Introduction for the General Reader. By John Mills Research Laboratories, Western Electric Company, Inc. Member, American Physical Society. New York: the Macmillan Company, 1919. (Pp. xxi + 327.) Price $2.50.

Intended for the general reader, and contains chapters on the Beginnings of Experimentation, the Molecular Composition of Matter, Some Uses of Mathematics, and a number of chapters on Electrons and Molecular States.

General Science: First Course. By Lewis Elhuff, A.M. (Yale), Instructor in Science in the George Westinghouse High School, Pittsburgh, Pa. Boston: D. C. Heath & Co. (Pp. viii + 433, with illustrations.) Price 5*s*. net.

Intended to offer scientific explanations of many ordinary experiences and to create a desire for further knowledge among pupils of high-school age. The subjects include Health, Chemistry of Common Things, such as Baking, Preservatives and Disinfectants, Heating Buildings, Simple Machines, Gas Pumps, Simple Technical Appliances, how Plants grow, and other lessons, numbering forty-two altogether. Excellently designed and well carried out.

The Historical Method in Ethics, and other Essays. By John Handyside, M.A. (Edin.), B.A. (Oxon), Late Lecturer in Philosophy in the University of Liverpool. Liverpool: The University Press; London: Constable Company. (Pp. xvi + 97.) Price 5*s*. net.

Causeries Philosophiques. By A. Badoureau, Ingénieur en chef des Mines en retraite. Paris: Gauthier-Villars et Cie, Éditeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, Quai des Grands-Augustins, 55, 1920. (Pp. xx + 227.) Price 8 frs.

Sex, the Key to the Bible. By Sidney C. Tapp, Ph.B. Published by the Author, Kansas City, Mo., 1918. (Pp. 172.)

Annuaire pour l'An 1920. Publié par le Bureau des Longitudes, avec des Notices Scientifiques. Paris: Gauthier-Villars et Cie, Quai des Grands-Augustins, 55. (Pp. vii + 80.) Price 4.50 frs.

Introduction to World History. By Ernest H. Short. London: Hodder & Stoughton, 1920. (Pp. 13 + 248.) Price 4*s*. 6*d*. net.

The Field of Philosophy: An Introduction to the Study of Philosophy. By Joseph Alexander Leighton, Professor of Philosophy in the Ohio State University. Second Revised and Enlarged Edition. Columbus, Ohio: R. G. Adams & Co., 1919. (Pp. xii + 485.) Price $2 net.

Psycho-Analysis: A Brief Account of the Freudian Theory. By Barbara Low, B.A., Member of the British Psycho-Analytical Society. Introduction by Ernest Jones, M.D., M.R.C.P. London: George Allen & Unwin, 40 Museum Street, W.C.1. (Pp. 191.) Price 5*s*. net.

ERRATA

In the April issue, section "Books Received," Military Psychiatry in Peace and War, by C. Standard Read, price 12*s*. 6*d*., should read, C. Stanford Read, price 10*s*. 6*d*.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By HUGH ELLIOT.

THE most interesting event of the last six months, from the purely personal point of view, is the centenary of Herbert Spencer, who was born at Derby on April 27, 1820. Of all philosophers who have ever lived, he is perhaps the one who has most appealed to men of science: and perhaps also the one who has least appealed to the professional metaphysician. The latter class did not admit him as a philosopher at all, until public appreciation compelled his recognition; and it is characteristic that his centenary has been passed over in silence by most of the philosophic journals, while a public newspaper such as the *Times* devoted a leading article to the celebration of the event. The neglect of Spencer by philosophic specialists is not at all surprising. He took every opportunity of condemning the methods of metaphysics: he ridiculed their results, and ignored their writings: he would not even read Kant. He insisted that the conclusions of science were the subject-matter of philosophy, which could be effectively studied only by the methods of science: all his interests, all his serious reading, was in science, and on the basis of science alone he started to construct a fresh system of philosophy totally independent of anything that had gone before. It is true that he did not wholly escape metaphysical infection. His doctrine of the Unknowable, and his attempted reconciliation of science and religion, were very weak, and no serious student now attaches the least importance to them. The metaphysical portions of his *Principles of Psychology* have similarly passed into total oblivion. He adopted, as the final test of truth of any proposition, "the inconceivability of its negation": and if he had lived into the days of relativity, he would probably have denied that doctrine on the grounds of *a priori* impossibility. Criticism of Spencer is easy but unprofitable: the real value of his work is apt to be lost sight of, for the simple reason that it has been thoroughly incorporated into all our modes of thought, and no longer strikes us as novel or unusual. He is the founder of scientific psychology. Every psychologist

now assumes evolution as his starting-point. Spencer was the first to point out the fact of mental evolution: and he did so several years before the publication of the *Origin of Species*. His work at that time appeared so novel and revolutionary that it was almost universally ignored. And it is now nearly as much ignored, because the particular kind of evolution which he suggested is not that favoured by the present generation of biologists. We forget that the whole basis of modern psychology was provided by Spencer; we remember only the comparatively trivial points in which we now think he was wrong. In short, Spencer is not read now, because his scientific work is done: his most revolutionary doctrines have become the tamest of platitudes. But his influence has been enormous. He was one of the three or four Victorians who stood out as the great protagonists of science, and induced a degree of public respect for science which before had never been dreamt of. He only did one piece of actual experimental work: it dealt with the "Circulation and the Formation of Wood in Plants," and formed the subject of a paper read before the Linnean Society on March 1, 1866. With this exception, the whole of his work was in the sphere of thought and literature. He was offered the Fellowship of the Royal Society in 1874, but refused it, as he did nearly every other honour proposed to him. His reputation by that time was made, and he undoubtedly felt hurt that recognition did not come to him from learned bodies until he had established himself with the public, and no longer suffered from the absence of academic distinctions. His will provides that, after the execution of certain literary work (still in preparation), his property shall pass absolutely to several of the leading scientific societies. The time for carrying out this provision should now be drawing near.

The past six months have witnessed the publication of a number of works on Relativity, the interest in which continues unabated. The most important of these, from the popular standpoint, are *Relativity: the Special and the General Theory*, by Albert Einstein, translated by Robert W. Lawson (Methuen & Co., Ltd.); and *Space, Time, and Gravitation*, by Prof. Eddington, published by the Cambridge University Press. The latter book branches into Philosophy, with a chapter "On the Nature of Things," from which we infer with regret that the Nature of Things is only destined to be understood by mathematicians. Another important work from the Cambridge University Press is *The Concept of Nature*, by Prof. A. N. Whitehead, containing his Tarner Lectures delivered at Trinity College in November 1919. It forms a companion book to his *Enquiry Concerning the Principles of Natural Know-*

ledge. Prof. Whitehead, as is well known, favours a different form of the Principle of Relativity from that propounded by Einstein, who, according to Prof. Whitehead, " has cramped the development of his brilliant mathematical method in the narrow bounds of a very doubtful philosophy." Notwithstanding Prof. Whitehead's extremely lucid exposition, it is hardly possible for anyone but a mathematician to form an opinion of the relative merits of the rival theories.

Prof. W. R. Sorley has published *A History of English Philosophy* (Cambridge University Press), which should take its place at once as a standard textbook. He deals with the History of Philosophy in Great Britain from the time when it began to be written in the English language (*i.e.* with Francis Bacon) down to the end of the Victorian Era. He has abandoned the usual method of writing histories of philosophy, to illustrate and emphasise the author's own opinions, and has adopted the comparatively impersonal method of taking up the point of view of each philosopher in turn. The result is an eminently readable and informing work, though it is true that his own view is usually visible in the background. It is curious that, in his Retrospect on the *General Characteristics of English Philosophy*, he makes no reference to the marked tendency towards Materialism, which has characterised English philosophy as against all others. This fact, originally pointed out by Lange, has so far as we know never been denied : and it is too abundantly illustrated in Prof. Sorley's History for us to suppose that he would wish to repudiate it.

Another historical work on Philosophy is Prof. J. A. Leighton's *The Field of Philosophy* (Columbus, Ohio : R. G. Adams & Co.), which has now reached a second edition. It introduces the reader to the underlying ideas of the main systems of philosophy of all periods, but is more of the schoolbook type than Prof. Sorley's History, and less generally interesting.

In the sphere of Psychology, several books of importance have been issued. The complete revolution of ideas caused by the writings of Freud, Janet, and others has given rise almost to a new science, most admirably described in *The New Psychology*, by A. G. Tansley (George Allen & Unwin, Ltd.). Mr. Tansley avoids the chief paradoxes of the Freudian theory, and presents a bird's-eye view of the present position, which is certainly the best that we have yet seen. In Social Psychology, Dr. McDougall has published a sequel to his earlier work, entitled *The Group Mind* (Cambridge University Press), carrying a step farther the investigations for which he is already well known.

An interesting philosophical side-line is opened up by Prof. J. B. Bury in his *Idea of Progress* (Macmillan & Co., Ltd.).

We now all look upon progress as an object naturally to be aimed at, and it is difficult to conceive a time when society was regarded as fixed permanently in structure, and progress was not an ideal aimed at by rational people. Their guiding principle was rather the welfare of their souls in a future world, than advancement of mankind in the present. The introduction of the new idea is very ably described by Prof. Bury; and we learn with surprise how very recent are some conceptions which we might have supposed fundamental to the human mind. Relativity is at least as true in Psychology as in Physics.

We may perhaps note the publication of *Causeries Philosophiques*, by A. Badoureau (Paris, Gauthier-Villars et Cie), a sequel to *Les Sciences Expérimentales*. Though not published till the present year, it is entirely pre-relativity in conception: and far more literary than scientific. The author holds that three-dimensional space is an objective fact, that force is also objective, and that atoms are ether vortices. There are no conceptions of novelty in the book, which for the most part is based on a shallow philosophy.

As regards Ethics, Prof. Mair has edited and published *The Historical Method in Ethics*, by John Handyside (Liverpool University Press), consisting of three essays by a young author who was killed in the war. A biographical note is provided by Prof. A. S. Pringle-Pattison. The essays are of considerable interest, though naturally somewhat fragmentary. Sir Charles Walston has published a lecture delivered at Cambridge in 1919 on *Eugenics, Civics, and Ethics* (Cambridge University Press), in which he advocates more attention on the part of Eugenists to the ethical standpoint, desiring that they should form a clear idea as to what type of mankind they desire to cultivate. Sir Charles usually adopts the ethical outlook on philosophical problems: a view natural enough to an ex-professor of Fine Art: for, as we have often insisted, Ethics is not a science, but an art. Sir Charles well insists, however, that it should, like other arts, be based upon science—the science of human character, otherwise called Ethology.

The *British Journal of Psychology* has published its No. VI Monograph Supplement on "Pleasure—Unpleasure," by A. Wohlgemuth (Cambridge University Press): an important investigation into the nature of "feeling," the results of which are summed up in eighty-eight separate propositions. Probably the most important of these conclusions is, that "there are only two qualities of feeling-elements, viz. Pleasure and Unpleasure." All other apparent differences belong in reality to sensation, or other cognitive or conative processes.

The *Revue Philosophique* has had no articles of outstanding

importance, but continues to maintain its position as one of the most readable of the philosophic reviews.

The philosophical congress at Oxford in the latter half of September comes, unfortunately, too late to be referred to in the present notice.

ASTRONOMY. By H. SPENCER-JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Brown's "Tables of the Motion of the Moon."[1]—No record of the progress of astronomy would be complete which omitted a reference to the publication of the new *Tables of the Motion of the Moon*, with the preparation of which Prof. Brown has been occupied during the past thirty years, and the value of which is attested by the honours conferred upon him by the Royal Society, the Royal Astronomical Society, Cambridge University, the Paris Academy of Sciences, and the Astronomical Society of the Pacific.

To a first approximation the " theory " of the motion of the Moon is a particular case of the problem of three bodies, complicated by the fact that neither the Earth nor the Moon is spherical; but, further, the attractions of the planets Mercury, Venus, Mars, Jupiter, and Saturn have to be taken into account. Differential equations expressing the laws of motion and the law of gravitation are formed and solved for the longitude, latitude, and parallax in terms of a single variable quantity—the time. The results are expressed as the sum of many hundreds of periodic terms, each of which has portions which come from several different parts of the calculations. Prof. Brown has stated that in this work " the number of figures written mounted to four or five millions, not counting algebraic symbols or the figures which passed through the mind while doing the calculations. . . . In the final results, about 1,500 terms were left which seemed large enough to be recorded as having an effect to be included when obtaining the position of the Moon at any time."

To calculate the values of these 1,500 terms every time the position of the Moon was required would prove an enormous task: hence the need for the construction of tables which perform the separate calculations once for all time. The num-

[1] *Tables of the Motion of the Moon.* By E. W. Brown, Professor of Mathematics in Yale University, with the assistance of H. B. Hedrick, Chief Computer. [Vol. i, Sections I and II, pp. xiv + 140 + 39. Vol. ii, Section III, pp. 223. Vol. iii, Sections IV, V, and VI, pp. 99 + 56 + 102. Section I, Explanation of the Tables; Section II, Tables of the Arguments and Mean Longitudes; Section III, Tables of the True Longitude; Section IV, Tables of the Latitude; Section V, Tables of the Parallax; Section VI, Tables of the Planetary and other Perturbations and Auxiliary Tables.] (New Haven: Yale University Press, 1919.)

ber of tables must be reduced to the smallest possible, and the interpolation between successive entries made to involve as little labour as possible. With such skill and careful organisation have the new tables been prepared that, although they include nearly five times as many terms as are contained in Hansen's tables (from which the positions of the Moon given in the *Nautical Almanac* are at present computed), the time taken to obtain the annual ephemeris of the Moon will not be greater than is at present involved in the use of Hansen's tables.

There is one feature of the work which deserves mention: there are certain constants involved in the theory, such as the average time of revolution and the mean distance of the Moon, which can only be determined from observation. The basis of these determinations is the long series of observations of the Moon made at Greenwich during the past 150 years, and it is entirely due to this long-continued series of observations that these constants can be determined with such accuracy.

Every possible gravitational term which can influence the position of the Moon has been taken into account. It is known, however, that the Newtonian theory of gravitation will not fully explain the motion. (It is, perhaps, desirable to add that neither will Einstein's theory.) It has been customary, in the preparation of lunar tables, to introduce several empirical terms, so chosen as to represent past observations with sufficient accuracy. But such terms failed, after a few years, to represent subsequent observations. Prof. Brown decided to exclude all such empirical terms except Newcomb's long period term. A comparison of observation with pure gravitational theory therefore becomes very simple with the aid of the tables. It follows, however, that the tables will fail from the beginning accurately to represent the Moon's motion: for purposes for which very accurate positions are required, as in computations of eclipses of the Sun, a correction will need to be applied which must be based upon the latest observations. In course of time, from the comparison between Brown's theory and observation, it may be possible to determine the empirical terms with greater accuracy than has hitherto been possible. In order that the work may not be invalidated by any subsequent change which may be found necessary in any of the adopted constants, data are given by means of which the resulting changes in the Moon's place may be calculated.

The new tables will be used in the computations for the national ephemerides from 1923 onwards. Prof. Brown is to be congratulated on the successful completion of his thirty years' work on the Moon's motion.

The Secular Accelerations of the Sun and Moon.—The most

probable values of the secular accelerations of the Sun and Moon have been discussed by Dr. J. K. Fotheringham in a recent paper (*M.N.*, *R.A.S.*, **80**, 578, 1920). The various lines of evidence have been brought under review, and the previous work of Newcomb, Cowell, and Fotheringham has been critically examined and supplemented by some fresh material. Five sources of evidence are available for such a discussion. (1) The times of ancient lunar eclipses give the difference between the lunar and solar accelerations. The difficulty in utilising the material is due to the inaccuracy or indefiniteness of the recorded times and places at which the eclipses were visible. For various reasons (*M.N.*, **75**, 395) Fotheringham rejects the Babylonian eclipses. Several of the Greek eclipses are also rejected owing to unsatisfactory material. From the remaining eclipses a value of $+ 7''{\cdot}9 \pm 1''{\cdot}3$ is found for the difference between the secular accelerations of the Moon and Sun. (2) The magnitudes of lunar eclipses give a value for the secular acceleration of the Sun. The data have been discussed by Fotheringham (*M.N.*, **69**, 666 ; **78**, 422), and give a mean solar acceleration of $+ 1''{\cdot}78 \pm 0''{\cdot}45$. (3) Records of ancient solar eclipses provide material for determining the secular accelerations of both Sun and Moon. Fotheringham finds that the eclipses of Hipparchus and Plutarch are critical eclipses, and that the records of these cannot be satisfied if the acceleration of the Sun is greater than about $1''$. With this value for the Sun and a value of $+ 10''{\cdot}5$ for the Moon, the most reliable of the ancient solar eclipses are satisfied, and it is concluded that these are the most probable values to be deduced from the whole series. (4) From the records of occultations of stars by the Moon, the lunar accelerations can be found. Dr. Fotheringham and Miss Longbottom have previously discussed these (*M.N.*, **75**, 393), and found a value of $+ 10''{\cdot}8 \pm 0''{\cdot}70$. (5) A value for the solar acceleration can also be found from equinox observations if it is assumed that Hipparchus used a fixed equator in his earlier series of equinox observations, but that in his later series he used the same equator as in his observations of the declinations of stars. In this way a value of $+ 1''{\cdot}93 \pm 0''{\cdot}27$ is found for the solar acceleration.

Summarising, Fotheringham finds that the most probable values furnished by the various lines of evidence are :

	Secular acceleration of Moon.	Sun.	Difference.
1. Lunar Eclipse Times			$+ 7''{\cdot}9 \pm 1''{\cdot}3$
2. Lunar Eclipse Magnitudes . . .		$+ 1''{\cdot}78 \pm 0''{\cdot}45$	
3. Solar Eclipses . .	$+ 10''{\cdot}5$	$+ 1''{\cdot}0$	$+ 9''{\cdot}5$
4. Occultations . .	$+ 10''{\cdot}8 \pm 0''{\cdot}70$		
5. Equinoxes . .		$+ 1''{\cdot}93 \pm 0''{\cdot}27$	

The evidence seems to point very definitely to a value of about $+10''\cdot5$ for the lunar acceleration, and to a value for the solar acceleration lying between $+1''$ and $+2''$, but which is probably closer to the latter figure.

Interference Methods in Astronomy.—The application of interference methods to observations with the 100-inch Hooker telescope of the Mount Wilson Observatory has recently directed attention to the advantages of this method, which, although not new, has not received much attention from astronomers. The method, however, has for some purposes such marked advantages that it seems desirable to summarise here the principles which underlie it, and the results which have already been obtained at Mount Wilson. It may be recalled that the image of a distant star (point-source) formed in the focal plane of a telescope is not a point of light, as would be inferred from the geometrical theory, but consists, in the normal case, of a circular aperture of a bright central disc surrounded by a series of circular diffraction rings whose angular diameters can be calculated. The angular diameter of the first ring is, for instance, given by $1\cdot22\ \lambda/D$, D being the diameter of the aperture and λ the wave-length of the light. If two close distant stars are observed, their diffraction patterns will be superposed. If the central image of one falls on the first diffraction ring of the other, a diminution in the intensity between the two nuclei will be observed, and the star will be recognised as double: if the nuclei are closer still together, then a variation in intensity cannot be definitely asserted, though the star-image may be perceived to be elongated. Lord Rayleigh proposed that this limiting case should be considered as the limit of resolution of the telescope, *i.e.* stars at an angular distance of less than $1\cdot22\ \lambda/D$ cannot be considered as separated by the telescope. This gives a theoretical resolving power of a telescope of aperture D (expressed in inches) of about $5''/D$. Thus, properly to resolve a double star of separation $0''\cdot25$ would require a 20-inch telescope.

In the case of the image of a planet or planetary satellite, geometrical theory would indicate an image with a perfectly sharp edge. Diffraction, however, produces a more or less general falling off of intensity at the theoretical circumference instead of the abrupt transition. For a given magnification, the greater the aperture of the telescope the more rapid is the falling off in intensity. As a result, the determination of the angular diameters of small satellites, etc., is a very delicate matter, and the values obtained in general depend upon the size of the aperture, the magnification employed, and also on the brightness of the object; the smaller the telescope, the larger will the measured diameter be.

If, now, a narrow slit be placed over the objective, the diffraction phenomena are entirely modified and a series of diffraction fringes parallel to the edges of the slit are formed. By using two parallel slits, the diffraction pattern is modified according to the distance apart of the slits, and it is this fact which proves to be of value in astronomical observation. The theory of the phenomena which then result has been discussed by Michelson and also by Hamy. We need consider only the two most important cases:

(1) If a circular object of uniform brightness with a finite, but small, angular diameter is observed, a series of fringes is obtained; but if the separation of the slits is gradually increased, the contrast between the fringes becomes less until a certain stage is reached at which their visibility becomes a minimum. The angular diameter of the object (a) is then connected with the distance apart, d, of the slits by the relationship $a = 1 \cdot 22\ \lambda/d$. The distance apart of the slits corresponding to minimum visibility can be determined with great accuracy, the phenomenon being very sharply marked. A method of determining the angular diameters of satellites, etc., is thus obtained which does not suffer from the causes of error incidental to the method of direct observation. It has been utilised to measure the diameters of the larger satellites of Jupiter by Hamy, using a 12-inch equatorial at the Paris Observatory, and by Michelson, using an equatorial at the Lick Observatory, in each case in the year 1891 and with very concordant results. It is not necessary that the slits should be placed over the object glass: they can be anywhere in the cone of rays, the corresponding separation being proportional to their distance from the focal plane.

Michelson has shown how, by using an interferometer to increase by a known amount the relative retardation between the rays from the two slits, the effective aperture of the telescope can be considerably increased. It does not seem beyond the bounds of probability that by this means, with the 100-inch telescope at Mount Wilson, it will be possible to measure the angular diameters of the nearer stars. It is not improbable that some of these stars have angular diameter of $0''\cdot 01$, which quantity should be measurable.

The disadvantage of this method is that the amount of light utilised is only a small fraction of that transmitted by the object glass. Hamy has considered how this defect may be avoided. The method is still valid if, instead of narrow slits, openings having two perpendicular axes of symmetry are used, one of which coincides with the line joining their centres, and provided that the width of the openings is small compared with their distance apart. Hamy proved that the

latter restriction may be removed, and widths up to one-third of the distance between the centres utilised provided that the formula $a = 1{\cdot}22\ \lambda/d$ is modified thus :

$$a = 1{\cdot}22 \frac{\lambda}{d}\left\{1 + 0{\cdot}765\left(\frac{a}{d}\right)^2\right\}$$

where a is the width of each opening and d the distance between their centres.

It is possible that the results obtained may be inaccurate owing to the supposition having been made that the object is of uniform brightness. It is more probable that the brightness falls off towards the limb as in the case of the Sun. The correction, corresponding to any proposed law of illumination, is easily deduced and is not, in general, very large.

(2) If the object consists of two circular objects whose distance apart is at least several times the diameter of either, then, if the slits be placed at right angles to the line joining their centres, the fringes will disappear when the angle (α) subtended by the two stars is given by $\alpha = \lambda/2d$, it being supposed that the two stars are of equal brightness. If not, and provided the disparity in brightness is not too great, a minimum visibility will be found to correspond to the separation of the slits given by this equation. If, then, for a given double star the position and distance apart of the slits for which the fringes disappear be determined, the position angle and angular separation of the components can be at once obtained. Since the greatest separation of the slits is the full aperture of the telescope (D), the angular separations of all double stars which are not less than $\frac{1}{2}\ \lambda/D$ can be determined. It was shown above that, by the ordinary method of observation, the least angular separation measurable was $1{\cdot}22\ \lambda/D$, so that the interference method at least doubles the theoretical resolving power of the telescope, a matter of supreme importance which, for this type of observation, is equivalent to increasing the aperture of the 100-inch to over 200 inches. Using this method, with the 100-inch telescope, the separation of double stars which are as close as $0''{\cdot}02$ can be determined. The method also possesses several very important advantages when compared with the ordinary methods of double-star observation—viz., both distance and position angle are determined with the same order of accuracy, whatever the separation, and for the successful application of the method, very good observing conditions are not necessary.

The results obtained in the case of Capella (Hale, *Nature*, **105**, 268, 1920) may be mentioned. Capella was known, from spectroscopic observations, to be a binary, but had never been visually separated, though examined with the largest refractors.

Slits 1 inch in length and $\frac{1}{8}$ inch wide, placed 47 inches from the focus, were used. The cone of light in that position had a section of 2·9 inches. The focal length of the telescope is 1,600 inches. The fringes were found to disappear for a certain separation and position angle of the slits, showing that the two components are of approximately equal brightness. The following values of distance and position angle were obtained :

	Position Angle.	Distance.
1919, December 30	148°·0	0″·0418
1920, February 13	5·0	·0458
,, 14	1·0	·0451
,, 15	356·4	·0443
March 15	242·0	·0505

showing rapid orbital motion, in agreement with spectroscopic evidence.

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CROMMELIN, A. C. D., Comets with Small Perihelion Distance and the Resisting Medium, *M.N., R.A.S.*, **80**, 475, 1920.

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HARPER, W. E., Orbit of the Spectroscopic Binary, Boss 4507, *ibid.*, **1**, No. 5. Orbit of the Spectroscopic Binary, ι Delphini, *ibid.*, **1**, No. 8.

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PHYSICS. By D. ORSON WOOD, M.Sc., University College, London.

THE Bakerian Lecture this year was delivered by Sir Ernest Rutherford, who, speaking on "The Nuclear Constitution of Atoms," gave an account of the latest developments of his work on the intense bombardment of light atoms by swift α-rays (*Proc. Roy. Soc.*, 1920, **A97**, 374–401; an account of the earlier work appeared in the *Phil. Mag.*, 1919, **37**, 538, and was dealt with in SCIENCE PROGRESS, 1919, **54**, 206–11). It will be remembered that, when α-rays from radium C are allowed to pass through nitrogen, swift particles are obtained which,

from their range of penetration and from the brilliancy of the scintillations they produce on a zinc sulphide screen, seem to be hydrogen atoms. The proof of this fact was not complete in 1919, for the crucial test provided by the deflection which the particles undergo in a strong magnetic field had failed to give definite results. However, during the past year the very serious difficulties presented by this experiment have been overcome, and the identity of these particles with hydrogen atoms has been definitely established.

Success has been attained by viewing the zinc sulphide screen with objectives of wide aperture (whereby the brilliancy of the scintillations is increased and counting made less difficult), and by passing the stream of α-particles through wide slits, so that the number of scintillations due to the gas, as compared with the number due to particles originating at the source, is much increased. By comparing the deflexion of the particles derived from nitrogen with that of hydrogen atoms set in motion by the impact of α-rays travelling through a mixture of hydrogen and carbon dioxide having the same stopping power as nitrogen, it has been shown that the mass of the particles is certainly less than 2 and, within the limits of experimental error, equal to 1. Thus it is to be inferred that the positively charged atom of hydrogen is one of the components of which the nitrogen nucleus is composed.

This method of preparing hydrogen is not, of course, likely to form the basis of a manufacturing process, for Rutherford estimates that, if the whole of the α-radiation from 1 gram of radium was absorbed in nitrogen gas, only about a two-millionth of a cubic centimetre of hydrogen would be produced per year.

Since the liberation of these particles from nitrogen is a purely atomic phenomenon, similar particles should be emitted from nitrogen compounds in number proportional to the amount of nitrogen present. This has been verified by bombarding a number of compounds rich in nitrogen which had been carefully prepared so as to exclude the presence of hydrogen in any form. The nitrides of boron, sodium, and titanium were used for the experiments, and also para-cyanogen. Of these sodium and titanium nitrides gave the estimated results; but boron nitride and para-cyanogen gave 1·5–2 times the number of long-range (hydrogen) particles expected. This, of course, might have been due to the presence of hydrogen in the specimens employed, in spite of the care taken to exclude it; but there is also the possibility that boron itself may emit hydrogen atoms.

In addition to the long-range hydrogen atoms liberated from nitrogen, the passage of α-particles through oxygen as well as

through nitrogen gives rise to other and much more numerous swift atoms which have a range in air of about 9 cm.—*i.e.*, much smaller than that of the hydrogen atoms we have hitherto been considering, but larger than that of the original α-particles (7 cm.). It was at first assumed that these were atoms of oxygen or nitrogen carrying a single charge which were set into rapid motion by close collisions with the α-particles; but if this assumption was correct, it was difficult to explain why the range (9 cm.) should be the same for both gases. There remained the alternative and more fascinating possibility that the particles might be fragments of disintegrated atoms. Further experiments, using the magnetic deflexion apparatus, have shown this to be the case, for the mass of the particles is greater than 1 (thus excluding the possibility of their being hydrogen) and less than 4 (thus likewise excluding helium). In all probability their mass lies between 3 and $3 \times 1{\cdot}008$. Thus we have obtained a third constituent of the atomic nucleus, and this time one common both to oxygen and nitrogen.

It would seem that the nucleus of a nitrogen atom does not break down into its constituents of mass 1 and mass 3 simultaneously, for the latter component is produced 5 to 10 times more frequently than the former. Considering, too, the relative infrequency of collisions between α-particles and atoms, it is very improbable that a single atom suffers both types of disintegration. The mass 3 atom carries a double charge, and, when associated with the two electrons required to neutralise this charge, should have properties and spectrum, very much resembling helium. It is in fact, in all probability, an isotope of that element.

It is most natural to assume that the nuclei of all atoms are built up of hydrogen nuclei and electrons; *e.g.*, that the helium nucleus is composed of four hydrogen nuclei and two electrons, giving it a resultant charge $+2$ which is neutralised by the two external (or ring) electrons associated with it. The fact that the atomic weight of helium ($3{\cdot}997$ in terms of $O=16$) is less than that of four hydrogen atoms ($4{\cdot}032$) may be explained as being due to the close interaction of the fields in the nucleus resulting in a smaller electro-magnetic mass than the sum of the masses of the individual components. As Sommerfeld has pointed out, this would make helium (and also its isotope) extremely stable, and here, perhaps, is the explanation of its appearance as a secondary unit in the nuclear structure of the heavy atoms (*e.g.* those emitting α-particles).

If the nuclei are built up on this plan, then it is possible that hydrogen has an isotope of nuclear mass 2 and charge 1.

There may also be an atom whose nucleus has mass 1 and zero charge, *i.e.* one formed by the close union of a hydrogen nucleus and an electron. Such an atom would have very novel properties; it would, for example, have no external field except at very short distances, and would therefore pass freely through ordinary matter.

Since the number and arrangement of the external electrons which determine most of the chemical and physical properties of an atom are fixed by the nuclear charge, it is to be expected that, as long as this charge remains the same, the properties also would be the same. This explains the existence of isotopes having different atomic weights but the same nuclear charge. It will thus be seen that a positive knowledge of the nuclear charge is a matter of fundamental importance. It is probably equal to the atomic number. Further information can be obtained by means of experiments on the scattering of swift α- and β-rays. Such experiments are at present in progress, and preliminary results obtained by Mr. Chadwick, who is working with α-rays, indicate that the number of free unit positive charges in the nucleus does not differ from the atomic number of the corresponding atom by more than 1 %, *i.e.* by more than the experimental error.

In the course of the lecture Sir Ernest indicated some other lines along which the problem of atomic structure is being attacked at the Cavendish laboratory. It is possible that the impact of rapidly moving electrons may suffice to cause the disintegration of oxygen, nitrogen, or other elements. This can be tested by observing the spectrum given by one of these gases in a vacuum tube after an intense bombardment of a suitable substance by cathode rays, and experiments on these lines are being carried out by Dr. Ishida.

Dr. Shimizu has devised a modification of C. T. R. Wilson's expansion apparatus whereby several expansions can be produced in one second. By studying photographic records of the tracks of the α-particles in these expansions, it is hoped to obtain information as to the conditions which determine the disintegration of the atoms, and also something about the relative energies in the α-particle, the escaping atom, and the residual nucleus.

Prof. D. C. Miller, of Cleveland, U.S.A., has published several papers containing the results of experiments on explosive sounds carried out during the war. Using the standard apparatus employed for sound-ranging, he finds (*Science*, June 18, 1920, pp. 619–20) that the velocity of sound at a distance of 100 ft. in front of a 10 in. gun is about 1,240 ft. per sec., or 22 % above the normal value; at a distance of 200 ft. the ex-

cess is only 5 %, and for all distances above 500 ft. the velocity of the explosive sound from the largest gun is practically normal. Careful observations of temperature, barometric height, humidity, and wind velocity were taken over a maximum range of 21,000 ft. in order to obtain a value for the velocity of sound under standard conditions. The final calculations of the corrections for this determination are not complete, but a preliminary value of 1,089 ft. per sec. has been worked out. Prof. Miller has also obtained photographs of the wave form of sounds from large guns (*Phys. Rev.*, 1920, **15**, 230). It is found that in all cases the characteristics are the same. There is no true vibratory form, but a single compression pulse rising abruptly to a maximum, and then falling to a rarefaction of much smaller amplitude but longer duration; the whole disturbance passing any fixed point in something of the order of a fiftieth of a second. Photographs of air waves round a rifle bullet (*Phys. Rev.*, 1920, **15**, 518) show that the bow and stern waves behind the bullet are quite straight and parallel, and are propagated in the direction corresponding to the relative velocities of the projectile (2,720 ft. per sec.) and of sound (1,123 ft. per sec. at the temperature of the experiment). Near the projectile the bow wave increases in velocity, probably owing to the heat developed by the projectile and by compression, until its velocity equals that of the projectile itself. The stern wave, immediately behind the projectile, has a diminished velocity, probably due to the cooling produced by the rarefaction of the air; this velocity increases rapidly up to the normal velocity of sound when the stern wave becomes parallel to the bow wave. The wake is a very strongly developed and well defined turbulence which, ten bullet-lengths behind the bullet, is about twice the diameter of the bullet in cross-section. In the same number of the *Phys. Rev.* (p. 516), A. T. Jones discusses the production of the characteristic whine due to a shell. It is attributed to vortices in the wake of the shell, and the author attempts to apply Strouhal's laws for the æolian tones produced by wires to calculate the variation in the pitch of the sound heard by an observer standing in the plane of the trajectory. The results obtained show no indication of any possibility of detecting by ear whether the observer is nearly underneath the vertex of the path, but in the case of an elevation of 45° indicate a rise of pitch followed by a fall.

PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

The Reaction Limits of Mixed Crystals.—The investigation of the chemical and galvanic properties of mixed crystals by

Tammann (*Zeit. Anor. Chem.*, 1919, **107**, 1–239) marks a considerable advance in the study of isomorphism. Tammann has examined the resistance to chemical reagents of a large number of mixed crystals, and finds that, whereas their physical properties vary continuously with the composition, this is not the case for the galvanic and chemical properties. Those reagents which attack only one of the components, such as nitric acid on the Au-Cu alloys, are shown to be without any action on the mixed crystal when the unattackable component exceeds a definite concentration. A series of mixed crystals can be divided into (1) the series containing 100 to g_1 per cent. of attackable component, which is completely disintegrated by the action of the reagent; (2) the series g_1 to g_2, which is partly attacked; and (3) the series g_2 to 0 per cent., which, except for the molecules on the surface layer, is unattackable. The resistance limits of normal mixed crystals, *i.e.* those which have been well tempered, lie at molecular fractions which are multiples of $\frac{1}{8}$. Thus boiling nitric acid removes the whole of the copper from a gold alloy if the gold content is less than $\frac{2}{8}$ mol; if the composition lies between $\frac{4}{8}$ and $\frac{8}{8}$ mol of gold, the material is unattackable.

These reaction limits can occur only at temperatures where there is no appreciable diffusion of the atoms in the crystal (*i.e.* in the Au-Cu series of mixed crystals the copper and gold atoms are not interchangeable); if the temperature be raised and the diffusion of the atoms becomes appreciable, then the protective action of the noble metal disappears, and the chemical properties vary continuously with the composition. The sharpness of the limits of protective action depends on the previous history of the alloy; crystals which are prepared at ordinary temperatures by electrical deposition or by precipitation from solution are more reactive and give less sharp reaction limits than those prepared by fusion of the components and tempered at temperatures not far below the melting-point. Thus alloys with the same molar composition, prepared in different ways, may behave differently to chemical reagents. Tammann ascribes these differences to the manner in which the atoms are distributed in the space lattice. He concludes that the arrangement of the atoms in those mixed crystals which are prepared at ordinary temperatures, though fulfilling the requirements of symmetry, is irregular, whereas in the well-tempered material there is a uniform distribution of the atoms. During the process of tempering the "unsymmetrical" mixed crystals are converted into the "symmetrical" variety.

The work has been extended to non-metallic mixed crystals, and provided that these have been well tempered, they yield

similar reaction limits to those of the alloys. Sodium chloride and silver chloride are completely miscible; crystals with 1 to 0·75 mol of AgCl scarcely yield any sodium chloride to water, while from 0·75 to 0·625 the series yield part of the sodium chloride, and from 0·625 to 0 the whole of the salt is removed.

Galvanic resistance limits are also observed; thus mixed crystals between g_2 and 0 per cent. give the same polarisation potential and possess the same precipitating ability as the more noble metal. The thermodynamical theory of the dependence of polarisation potential on the composition of an alloy evidently stands in opposition to the existence of galvanic resistance limits. This theory can only be employed when the interchange of atoms in the crystals proceeds sufficiently rapidly to enable the metallic phase to be in equilibrium with the liquid. These conditions are not fulfilled at ordinary temperatures in the case of the Au-Cu and the Au-Ag alloys, so that galvanic resistance limits corresponding to the chemical limits could have been predicted. The polarisation potential of the Au-Cu series of alloys in the element

$$\text{Cu-Au Alloy}\left\{\begin{matrix}0{\cdot}5\text{ mol}\\ CuSO_4\text{ or}\\ CuNO_3\end{matrix}\right\}\text{Cu}$$

has a constant value of 1·40 volt for mixed crystals containing 1·00 to 0·50 mol of gold, while below this concentration of gold the voltage falls off rapidly, and at 0·25 mol the voltage has fallen almost to zero. Measurements of the polarisation potentials at higher temperatures show no discontinuity at 0·50 mol gold, but vary continuously for the whole series of alloys. At these temperatures the atoms in the metallic phase are in equilibrium with the liquid.

Working the metal either by rolling or hammering imparts increased chemical activity to the metal; the uniform arrangement of the atoms in the space lattice is destroyed, and the crystals more readily admit the attack of the reagent.

Tammann develops a theory of the distribution of the atoms in the mixed crystals from the results of this work.

Isotopes of Chlorine.—According to the work of Aston (*Nature*, 1919, **104**, 393; *Phil. Mag.*, 1920, **39**, 620), who applies the positive-ray spectrograph to the analysis of the elements, the mass spectra obtained with chlorine appear to prove that this element consists of two isotopes of atomic weight 35 and 37. This is confirmed by W. D. Harkins (*Science*, 1920, **51**, 289–91), who has carried out preliminary experiments on the separation of hydrochloric acid by diffusion. His analyses

indicate that the density of the fraction which remains in the diffusion tubes is increasing at the rate which is predicted by the Raleigh theory of diffusion, if the two atomic weights of chlorine are 35 and 37.

Triatomic Hydrogen.—Previous reference has been made to the preparation of triatomic hydrogen (SCIENCE PROGRESS, 1919, **53**, 34). Wendt and Landauer (*J.A.C.S.*, 1920, **42**, 930–46) have published a further paper, in which several methods of preparation are given. The production of triatomic hydrogen is possible by three methods, the actions of α-rays, the alternating discharge at low pressure, and passage through an ozoniser at atmospheric pressure. Schumann light is without action. The activated hydrogen does not resemble the atomic hydrogen of Langmuir (*J.A.C.S.*, 1915, **37**, 417) in its general, physical, and chemical properties.

Its activity is not due to ions or atoms, and the contraction in volume under the action of an electric discharge indicates a triatomic form. It is not yet clear that the atom of atomic weight 3, observed by J. J. Thompson (*Proc. Roy. Soc.*, 1913, A, **89**, 1) and by Rutherford (*Proc. Roy. Soc.*, 1920, A, **97**, 374), is the same substance, for the form prepared by Wendt appears to be more reactive.

The chief theoretical interest in this work lies in the fact that no hydrogen molecule larger than H_2 is possible on the older conception of valency. One suggestion put forward by the authors is that of a triatomic ring of monovalent atoms with the alternation of the atom and the electron in a six-membered ring. Bohr, however, regards the structure of this molecule as a system of three electrons rotating at equal angular distances in a circular orbit, the three nuclei being placed respectively at the centre of the orbit and at two points on its axis equidistant from the centre.

The Specific Heats of Gases by the Explosion Method.—The radiation from the explosion of gases reaches very high values in the case of coal-gas and air and of hydrogen and air (David, *Phil. Mag.*, 1920, **39**, 66–95, 551–3); 26·1 and 16·1 per cent. of the total chemical energy respectively being emitted as radiation in the two cases. It is concluded that part of the radiation is due to chemical action, and part due to a purely thermal emission from the hot gases during the period of heating and cooling. It is suggested that the specific heat of a gas is the greater, the greater the mean free path of the molecules, and that the specific heats of the gases at 2,000° C. and over depend to a large extent on the density and the volume. If this suggestion be correct, the values of the specific heats of gases determined by Pier, Bjerrum, and others would require serious modification.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., University College, London.

UNDER the general heading of "Catalytic Actions at Solid Surfaces," Armstrong & Hilditch (*Proc. Roy. Soc.*, 1919, A, **96**, 137 and 322) have been studying the hydrogenation of unsaturated fatty oils by finely divided nickel. By measuring the volume of hydrogen entering and leaving the system by means of water-meters and plotting time-absorption curves, it was found that these were not of the logarithmic type required for a monomolecular reaction, but were composed of two linear portions, representing changes in which the active mass is constant, and it is concluded that this is evidence of the interaction between the catalyst and the unsaturated compound, similar to the combination between enzyme and substrate observed in enzyme hydrolysis. In the second of the two papers the question is raised whether this catalytic action, like that of enzymes, is not reversible; evidence in support of such reversibility is offered by the observation that, when equimolecular proportions of cyclohexanol and methyl cinnamate are heated with nickel at 180° C., 10 per cent. of the latter compound is reduced to methyl-phenylpropionate, while the former is oxidised to cyclohexanone, which implies a transference of hydrogen from a saturated to an unsaturated compound. While this change may be due to the production of hydrides of nickel, the authors prefer to regard it as a case of catalytic equilibria represented by the following scheme:

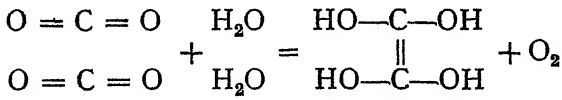
$$\text{Saturated compound} + \text{Ni} \rightleftarrows [\text{Saturated compound, Ni}] \rightleftarrows$$
$$[\text{Unsaturated compound, Ni, } H_2] \rightleftarrows \text{Unsaturated compound} + \text{Ni} + H_2.$$

The reversibility of this catalytic hydrogenation is called upon to explain the observation of Moore (*J. Soc. Chem. Ind.*, 1919, **38**, 320, T), that during the hydrogenation of ethyl oleate to ethyl stearate some isomerisation to oleate is produced; the latter compound is, according to Armstrong and Hilditch, formed by dehydrogenation of some of the freshly produced ethyl stearate.

In an earlier review (SCIENCE PROGRESS, 1918, No. 48, 564), mention was made of a paper by Woker on the diastatic properties of formaldehyde; since that time a considerable amount of controversy has arisen over this question. According to Jacoby, von Kaufmann, Lewite and Sallinger (*Berichte*, 1920, **53**, 681), the supposed diastatic property of formaldehyde may be explained by the fact that this compound forms a loose addition compound with starch, whereby the physical properties of the latter are much altered, and it no longer gives a blue colour with iodine; the starch does not thereby undergo

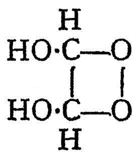
degradation, since it may be precipitated from the solution unchanged by the addition of alcohol or electrolytes.

In a paper entitled "New Theory of Carbon Dioxide Assimilation," Kogel (*Zeitschr. wiss. Photochem.*, 1920, **19**, 215) suggests that the action of light is to cause the carbon dioxide to combine with the hydrogen of water to form tetrahydroxyethylene (I) as under:



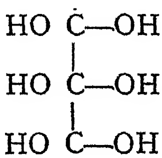
(I)

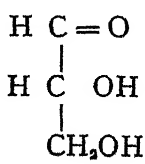
the tetrahydroxyethylene undergoes tautomeric change into the ketonic modification (II),



(II)

from which the common plant acids formic and oxalic could be easily produced by simple fission, or the removal of two atoms of hydrogen respectively. On the other hand, three molecules of carbon dioxide would by this method produce the compound (III), which by losing three oxygen atoms would, after rearrangement, yield a molecule of glyceric aldehyde (IV):





(III) (IV)

The obvious criticism of this theory is the readiness with which oxygen or hydrogen atoms are dispensed with at will in order to produce any compound desired by the propounder of the theory.

Langdon and Gailey (*J. Amer. Chem. Soc.*, 1920, **42**, 641) reaffirm the observation previously made (*loc. cit.*, 1917, **39**, 149) to the effect that the gas contained in the floaters of the giant Pacific coast kelp *Nercocystis leutkeana* contains carbon

monoxide ; the gas is not produced by enzymatic decomposition, since it is only formed in the living plant, and, moreover, finely divided kelp in contact with sea-water gives only carbon dioxide and hydrogen. The gas is considered to be a respiration product of the plant, and not an intermediate product of photosynthesis.

In spite of the considerable time that has elapsed since " Salvarsan " was first put on the market, conflicting views have, from time to time, been expressed concerning the composition of the commercial product ; as the result of a re-investigation of this question by Fargher and Pyman (*J. Chem. Soc.*, 1920, **117**, 370), there appears to be no doubt that the substance contains two molecules of water of crystallisation, but is free from methyl alcohol, which had been stated by some authors to be present ; its composition is therefore represented by the formula $C_{12}H_{12}O_2N_2As_2$, 2HCl, $2H_2O$. Both the British and German products contain from 1 to 2 per cent. of sulphur, as an impurity, combined, most probably, in the form of a sulphaminic acid or possibly merely physically absorbed.

The view expressed by Blount (*J. Chem. Soc.*, 1919, **115**, 705), that the colour of " Blue John " fluorite is of organic origin, is confirmed by Garnett (*ibid.*, 1920, **117**, 620), who has shown that the finely powdered substance on combustion gave a carbon content of 0·27 per cent., while on subjecting the material to distillation, a yellowish-brown oil collected in the cool parts of the vessel, together with a mirror-like coating of carbon, while a residue of colourless fluorite was left behind.

In a paper dealing with the constitution of Cholesterol, Windaus (*Chem. Zentr.*, 1920, [1], 82) gives a possible formula for this substance ; considerations of space preclude the printing of this formula, but attention is drawn to it here as it is more complete than any previously published formula for this compound.

CRYSTALLOGRAPHY. By ALEXANDER SCOTT, M.A., D.Sc.

Crystal Structure.—In the section on Crystallography in the Reports on the Progress of Chemistry, T. V. Barker (*Ann. Rep. Chem. Soc.*, **16**, 197, 1920) gives a critical summary of recent work on the elucidation of crystal structure by means of X-rays. The various modifications of the original methods are discussed, and the results obtained up to the present are summarised. The view that the chemical molecule persists in the solid state is upheld, but the argument that the fusion of crystals of, say, an ortho-compound should result in the formation of a mixture of ortho- and meta-molecules in the liquid state is scarcely convincing. It is conceivable that, even if " solid " molecules

analogous to chemical ones do not exist, the configuration of the atoms in the crystals of an ortho-compound may be such that, on fusion, the rearrangement involving the minimum energy change may be such as to give the ortho-compound in the liquid state as well. A general account of the X-ray methods of exploring crystal structure is given by R. Gross (*Jahrb. Radioakt. Electr.*, **15**, 305, 1918), while papers on the utility of the Laue radiogram, especially as a supplement to the results obtained by Bragg's method, by H. Seeman (*Phys. Zeitsch.*, **20**, 169, 1919) and P. Niggli (*ibid.*, **19**, 225, 1918), have also appeared.

The investigations in this direction are now tending to go beyond the mere determination of the relative location of the atoms, and in several recent papers the question of the structure of the atoms is taken into account. P. Debye and P. Scherrer have discussed, both from the mathematical and the physical points of view, the possibility of the electron rings acting as bonds in such a case as the diamond (*ibid.*, **19**, 474, 1918). If such a possibility exists, the presence of these bonds ought to be detectable by means of X-radiograms, but the fact that the results were negative is in favour of Bragg's original structure. On the basis of the theory that the diffracting power of atoms should vary as the Moseley number, the same authors have endeavoured to determine whether there is ionisation in crystals or not, by calculating the number of electrons connected with each atom. In the case of sodium fluoride, the derivation of the number of electrons from measurements of the intensity of reflection from planes composed of a single type of atom suggests that such ionisation exists (cf. D. Coster, *Proc. Akad. Amsterdam*, **22**, 536, 1920; H. Thirring, *Phys. Zeit.*, **21**, 281, 1920). The suggestion that the influence of the electrons must be taken into account in considering the ultimate structure of crystals has already been made, on theoretical grounds, by F. Rinne (*Neues Jahrb. Min.*, ii, 47, 1916).

The influence of the electrons is also discussed in a series of papers by M. Born and A. Landé. In the first paper (M. Born, *Verh. deut. phys. Ges.*, **20**, 224, 1918), the absolute crystal dimensions are calculated mathematically on the assumption that the lattice is composed of electronic ring systems such as those postulated by Bohr. In this way the lattice constants of cubic crystals of the sodium chloride type are derived (cf. M. Born and A. Landé, *Ber. preuss. Akad. Wiss.*, **45**, 1048, 1918). From these constants the same authors (*Verh. deut. phys. Ges.*, **20**, 202, 210, 1918) calculate the compressibility of the crystal, and find the theoretical value to be twice the observed value. Hence they conclude that atomic structures are three-dimensional, and that the plane electronic system is not satisfactory.

To get over the difficulty, A. Landé (*ibid.*, **20**, 217, 1918; **21**, 2, 644, 1919; *Ber. preuss. Akad. Wiss.*, 101, 1919; cf. M. Born, *Verh. deut. phys. Ges.*, **20**, 230, 1918; **21**, 13, 1919) assumes that the electronic orbits are symmetrically placed, and that, in the examples considered, the symmetry of the orbits is cubic. Some criticisms of the theory by L. Vegard (*ibid.*, **21**, 383, 1919) are replied to by Born and Landé (*ibid.*, **21**, 385, 1919). W. Voigt (*Ann. Phys.*, [4], **60**, 638, 1919) gives an exhaustive discussion of the same work, especially with reference to Landé's view that the forces of cohesion are due to ionic electrical attractions. In connection with the relation between force and deformation, he points out that the difficulty in anticipating the rupture-point may possibly be explained by the symmetrical arrangement of the orbits. The theory has been further tested by M. Born and E. Bormann (*Verh. deut. phys. Ges.*, **21**, 733, 1919), using zinc blende as an example; and although the calculated and observed results do not agree very closely, yet they are of the same order of magnitude (cf. M. Born, *Ann. Phys.*, [4], **61**, 87, 1920).

In a paper on a "Kineto-electro-magnetic Theory of Crystals," J. Beckenkamp (*Verh. phys. Ges. Wurzburg*, **45**, 135, 1918, *Abs.* in *Journ. Chem. Soc.*, **116**, 273, 1919) extends Bohr's theory and applies it to cubic crystals. In a lecture to the Royal Institution, W. L. Bragg (*Nature*, **105**, 646, 1920) summarises the work which has been done on the relation between the structure of the atoms and that of the crystal. On the basis of X-ray work and glide-plane experiments, A. Johnsen (*Cent. Min.*, 385, 1916) discusses the deformation of crystals of bismuth, in which the molecules are considered to be diatomic and to have the same symmetry as the crystals. In a further paper, A. Johnsen and A. Gruhn (*ibid.*, 366, 433, 1917) conclude that, in deformation, the atoms tend to move in groups or "complexes" (cf. A. Gruhn, *ibid.*, 85, 1918). F. Rinne (*ibid.*, 161, 172, 1919), on the basis of the absolute atomic volumes, calculated from the crystal structural units of such metals as aluminium, silver and gold, of the rhombohedral carbonates and the alkali haloids, comes to the conclusion that the chemical nature and the valency of the atoms are more important than the atomic volume. For example, the atomic volumes of the three metals named are approximately equal, but while silver and gold form solid solutions, aluminium does not do so with the others. Mixed crystals are supposed to be intermediate between compounds and physical mixtures. According to A. Johnsen (*ibid.*, 97, 1919), zircon is probably hemimorphous, and hence not in the same symmetry class as rutile, xenotime, etc., which are holohedral (cf. L. Vegard, *Phil. Mag.*, [6], **32**, 65, 505, 1916).

In a discussion with A. E. H. Tutton (*Journ. Wash. Acad. Sci.*, **9**, 94, 1919), E. T. Wherry (*ibid.*, **8**, 840, 1918; **9**, 94, 1919) maintains that the symmetry of the "crystal-molecule" (the unit of the lattice) must be considered as well as the symmetry of the lattice as a whole in determining the crystal class, and that crystals may belong to different classes according to the basis which is adopted. Tutton controverts the statement that on the basis of one property a crystal may belong to one class, and on the basis of another, to another class.

In the past year the structure of a number of crystals has been determined. A. J. Bijl and N. H. Kolkmeijer (*Chem. Weekbl.*, **15**, 1264, 1918; *Proc. Akad. Amsterdam*, **21**, 405, 494, 501, 1919) have investigated the structure of white and grey tin. The former is regarded as divalent and tetragonal, and the latter as tetravalent and octahedral. C. L. Burdick and E. A. Owen (*Journ. Amer. Chem. Soc.*, **40**, 1749, 1918) find carborundum to be composed of two interpenetrating face-centred rhombohedral lattices, the angle being almost 90 degrees, and each being displaced along the hexagonal axis a distance equal to 0·36 of the basal edge (cf. T. V. Barker, *loc. cit.*). This structure is similar to that of the diamond if half of the carbon atoms be assumed to be replaced by silicon, which, however, are not in the centre of the carbon tetrahedra. A. W. Hull (*Phys. Rev.*, **13**, 292, 1919), on the other hand, maintains the exact equivalence of the diamond and carborundum structures.

By a modification of Debye's method, H. Bohlen (*Ann. Phys.* [4], **61**, 421, 1920) finds thorium and nickel to be composed of face-centred cube lattices, and magnesium of two interpenetrating simple hexagonal lattices. According to P. Scherrer (*Phys. Zeit.*, **19**, 23, 1918), aluminium is similar to copper, etc., and has a face-centred lattice whose edge measures $4{\cdot}07 \times 10^{-8}$ cm. F. Rinne (*Cent. Min.*, 129, 1919) has determined nepheline to belong to the hexagonal pyramidal class, and ice (*Ber. Sachs. Ges. Wiss.*, **69**, 57, 1917; *Neues Jahrb. Min.*, 25, 1919) to the dihexagonal bipyramidal class, the structure resembling that of magnesium (cf. *Cent. Min.*, 137, 1918).

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Phylogeny and Morphology.—Several of the papers, read at the joint session of American Botanists at St. Louis in December last, dealing with the Phylogeny of Seed Plants, have recently appeared in the *Amer. Jour. Bot.* Bucholz reviews the details of the embryology of the Coniferæ and emphasises their phylogenetic importance. *Pinus* is regarded as very primitive in this respect, stress being laid upon its cleavage polyembryony, with eight potential embryos derived from the four initials and the four rosette cells. The development of a cap

as in *Agathis* and *Podocarpus* and simple polyembryony resulting from the fertilisation of several eggs, as in Cycads, are considered to be advanced features. Grouped in respect to the pro-embryo structure, the assumed line of progressive specialisation and advance is also that of numerical decrease of the Cotyledons, whilst the presumably primitive genera are those with dwarf shoots.

Chamberlain, in considering the living Cycads, upholds the view that the Cycadophyta have been derived from the Ferns by way of Cycadofilices, directly, or as a branch from the Bennettitales, and that this line of evolution is distinct from that of the Coniferophyta, to which probably the Gnetales and Angiosperms are connected by way of supposed extinct herbaceous ancestors.

In a third paper by Wieland, the distribution of the Cycadeoids is considered and their affinity to other groups of seed plants. Of these latter the Gnetales, Angiosperms, and Conifers are regarded as the most remote, whilst the Cycads, Cycadofilices, and Cordaitales are held to be the most nearly allied in the order named.

In this connection a recent paper by Scott is of interest (*Brit. Ass. Rep.*). This author suggests that the Pteridosperms and Angiosperms have always been distinct from any known Phyla of Vascular Cryptogams, and regards the fern-like features of the Cycadofilices as no evidence of affinity.

Yampolsky (*Amer. Jour. Bot.*) describes and figures various types of hermaphrodite flowers of *Mercurialis annua*. In place of the two carpels or 8–20 stamens of the normal flowers, these showed in some cases only one carpel with a group of stamens replacing the second. In other flowers three carpels were present accompanied by 1–6 stamens, or structures which were intermediate between stamens and carpels.

Anatomy.—Experimental work carried out by Snow (*Bot. Gaz.*) indicated that the intervals, between the successive diaphragms of a water plant (*Scirpus validus*) increased with a decrease in the rate of growth, and the increased intervals observed under low pressures was attributed to this cause. Diminished pressure appeared to have no effect either on the number or size of the air-chambers, and the conclusion is drawn that the occurrence of air-spaces bears no relation to low oxygen-pressure.

Barrat and Browne both contribute papers on the anatomy of *Equisetum* (*Ann. Bot.*). The former finds that the sporeling of *E. arvense* is at first protostelic, later becoming siphonostelic. The existence of secondary thickening at the nodes of *E. arvense* and *E. maximum* is denied, whilst the sporangiophores are regarded as organs *sui generis*, and not as fertile leaves.

Browne finds that the cones show progressive reduction, with respect to the xylem, which is least in *E. arvense* and most pronounced in *E. limosum*.

Ecology.—Several interesting papers dealing with the relation of plant growth to acidity have recently appeared. Latter and McIlvaine find that the hydroxyl ion is more harmful than the hydrogen ion in equivalent concentration, and that for germination a slightly acid reaction appears to be most favourable. The plants studied were Wheat, *Glycine*, *Zea*, *Medicago sativa*, and *Trifolium*. Of these the maximum growth was obtained at P_H 5·94, a reaction of approximate neutrality being slightly less favourable for *Medicago*, and markedly so for Wheat, *Zea*, and *Glycine* (*J. Agric. Res.*, 1920). Hoagland had previously noted that the absorption of NO_3, Ca, and Po_4, was greater under slightly acid conditions (P_H 5–5·5) than when the solution was nearly neutral. The Lime factor in permanent soil improvement is the subject of two papers by Lipman and Blair (*Soil Science*, 1920). These authors found that, on arable land without Leguminous plants, in rotation, there was a loss of Nitrogen from the limed plots as compared with the unlimed. They suggest a slightly acid reaction on light and medium soils may be desirable to prevent too rapid oxidation of organic matter. In the second paper, treating of rotations with Leguminous crops, it is suggested that the marked value of lime is to render the conditions favourable for nitrogen-fixing organisms, since when Leguminous plants are present, the gain from liming is pronounced.

Brenchley (*Ann. Applied Biol.*) finds that the effect of crowding on Barley plants, grown separately in water-culture solutions, is at first beneficial, but subsequently deleterious, tending to lower the amount of dry matter formed, and to encourage shoot development at the expense of the growth of the root. Moreover, the consumption of nitrogen per unit of dry matter is greater than in widely spaced plants.

Cryptogams.—A useful summary of the recent literature dealing with the sexuality of the Mucorineæ is contributed by Namyslowski to the *Revue générale de Botanique*. The conclusion is arrived at that external conditions greatly influence the mode of reproduction, carbohydrates favouring the production of zygospores, whilst organic nitrogenous materials favour vegetative reproduction. Although normally monœcious or diœcious, the latter is not a constant character, but monœcious (homothallic) forms have been observed to arise from diœcious (heterothallic), and in addition partially monœcious or partially diœcious types have been recognised.

The Mycoplasm Theory of Eriksson as applied to *Puccinia malvacearum* has been the subject of experimental investiga-

tion by Bailey (*Ann. Bot.*). The hypothesis brought forward by Eriksson for the persistence of this disease as naked protoplasm within the cells of the host was largely based on the recurrence of the disease year after year, although he found no trace of mycelium either in the seeds or young plants. Bailey finds that the two types of germination of the teleutospore are dependent on external conditions, and not on dimorphism of the spores. Two experiments were carried out: in the first sterilised seeds from diseased plants were sown in conditions which prevented all infection except from internal mycoplasm, whilst the controls were exposed to infection. No rust appeared on any of the protected plants, whilst the controls were infected. The second series of experiments, lasting over ten months, likewise supported the view that infection was not due to persistent mycoplasm.

Bristol contributes a review of the Genus *Chlorochytrium* to the *Jour. Linn. Soc.* in which thirteen species are recognised, including those formerly placed in the genera *Chlorocystis, Stomatochytrium, Endosphæra, Scotinosphæra,* and *Centrosphæra.*

Taxonomy.—In the *Journal of Botany,* Baker and Salmon deal with some of the segregates of *Erodium cicutarium* and describe *E. neglectum.* A revised arrangement of British Roses is compiled by Wooley-Dod. In the same journal, T. and T. A. Stevenson describe a British Orchis allied to *O. incarnata* under the name of *O. purpurella,* and Ladbrook describes a new species of *Couponi.*

The Hawaiian species of *Plantago* are dealt with by Rock (*Amer. Jour. Bot.*), of which two are endemic, viz. the shrubby *P. princeps* and *P. pachyphylla.* Of both these a number of varieties and formæ are recognised.

An extensive paper on *Mesembryanthemum* by Brown is contributed to *Jour. Linn. Soc.,* and contains diagnoses of more than fifty new species.

PLANT PHYSIOLOGY. By R. C. Knight, D.Sc., Imperial College of Science and Technology, London. (Plant Physiology Committee.)

Storage and Translocation.—The storage of reserve food materials in plants presents a diffuse problem, which by virtue of its widespread ramifications can hardly be considered alone. At the outset one is confronted with the fact that storage involves two processes of translocation, the first by which the nutritive materials are conveyed from the manufacturing tissues to the storage region, and the second, following the rest period, by which the store is transferred to the growing organs. Thus, in addition to the actual process of accumulation of the products of assimilation in stem, root, or fruit, we have to con-

sider the passage through the stem of the materials which constitute the food reserve. Moreover, throughout the growing season the stem is also a channel for the continual upward flow of the "transpiration stream," and the importance of this in relation to storage and translocation, essentially processes of solution and deposition from solution, is self-evident. Nor is the work of investigation simplified by the recurrence of annual, as well as diurnal, rhythmic changes in transpiration rate, food manufacture and utilisation, superposed one upon another. Consequently it is to be expected that progress towards an understanding of these processes will be slow and laborious.

The significance of the rôle played by osmotic pressure in the movement and storage of water and nutritive substances compels attention to the osmotic properties of cell sap. N. A. Maximov and I. A. Krasnoselskaia-Maximova (*Publ. Tiflis Bot. Gard.*, 1916, **19**, 10), in the course of extensive researches on transpiration, made determinations also of fluctuations of the osmotic pressure and of the sugar content of the cell sap of evergreen leaves. They found that the osmotic concentration of the leaf cell-sap was greater in winter than in summer, and this difference they were able to correlate with changes in the quantities of sugars and glucosides present. The winter accumulation of these substances, with the resulting concentration of the sap, is regarded as a protection against freezing and consequent injury. Another aspect of the question of changes of osmotic pressure of the sap at different stages of growth has been discussed by B. F. Lutman (*Amer. Journ. Bot.*, 1919, **6**, 181–202) in the course of investigations of potatoes. He arrived at some interesting conclusions with regard to translocation of stored food reserves, but in common with most workers in this field, he is confronted with cases of movements of nutritive materials which at present cannot be reconciled with known osmotic phenomena. Young shoots growing from a potato tuber yielded a sap which exhibited a slightly higher osmotic pressure than that obtained from the tissue of the parent tuber, and further, the leaves of the young plant gave a sap more concentrated than the stem sap. Later in the growing season this relation was found to be reversed, the stems then developing the higher pressure. As in the investigation first quoted, these changes were correlated with the local accumulation of sugars, more especially sucrose. The rather remarkable conclusion arrived at is that nutritive substances reach the growing parts of the plant from storage regions by passing along a simple concentration gradient, but the problem of the passage of assimilation products to storage organs and fruits is not amenable to such a simple explanation ;

in consequence of which the author is compelled to consider Haberlandt's "pumping sieve-tube" theory. The conclusion recorded in the paper previously referred to prompts the suggestion that the seasonal changes in the nature and quantity of the cell-sap solutes may be related to the need for retention of water in drought or rest periods. In the same connection may be noted the work of W. G. Craib (*Notes from Bot. Gard., Edinburgh,* 1918, **11**, 1–18) on the winter water storage in deciduous trees. He found that during the period immediately following leaf-fall the water in the trunk concentrates towards the centre, beginning at the base of the tree. Later in the winter, however, the reverse process takes place and the water content of the peripheral layers increases at the expense of the central portion, which eventually becomes the driest region. In consideration, however, of the rather variable relationship existing between heart-wood and sap-wood in different trees, it appears unlikely that the course of water movement here outlined can be a general phenomenon. It would nevertheless be instructive to trace any changes in the distribution of reserve substances in the trunk which might accompany the translocation of water during the rest period.

W. H. Chandler (*Univ. Missouri Agr. Exp. Station Research Bull.,* **14**, 1914, 491–552) obtained some noteworthy results in his work concerning the competition for water by different parts of the plant, and the part played therein by osmotic phenomena, under arid conditions. He found that the osmotic concentration of the sap of leaves and stem cortex was due chiefly to non-electrolytes, except in the rather particular case of succulents. The work of Lutman referred to earlier also finds confirmation in Chandler's observation that there exists a gradient of sap concentration in the cells of the cortex, increasing from the roots to the topmost twigs. The question of fruits is dealt with also in this paper, and it is noted that the sap concentration in the leaves of common fruit-trees is higher than that of the fruits themselves. This observation explains the fact that fruits wither before the leaves during a dry period, when the higher osmotic pressure of the leaf sap constitutes a distinct advantage in the competition for water. Moreover, it is shown that water will actually pass from fruits to leaves under excessively arid conditions. In this connection the writer urges the necessity of adequate summer pruning, which, whilst limiting root formation, nevertheless reduces the risk of loss of fruit through withering. Ringing experiments were also undertaken, and, in accordance with expectation, it was found that the removal of a ring of phloem resulted in a lowering of the concentration of the sap in the region below the ring, especially in the sap of the roots.

Atkins (*Some Recent Researches in Plant Physiology*, 1916, chap. xi) advanced a coherent theory of winter storage and translocation of carbohydrates, in which he suggested that starch stored in the wood parenchyma is transformed into sugars—sucrose being the chief sugar of translocation—in the spring, and passes into the vessels where a high osmotic concentration is developed. With the opening of the leaves and the rapid increase in transpiration rate, the quantity of water traversing the vessels is greatly increased, and this results in a corresponding decrease in the osmotic concentration of the carbohydrate solution, which, in fact, is found to be the case. Atkins considers that the carbohydrates manufactured in the leaves reach the vessels by way of the bark, medullary rays, and wood parenchyma.

Extensive ringing experiments have been carried out by O. F. Curtis (*Amer. Journ. Bot.*, 1920, **7**, 101–24), who dealt with a variety of plants, chiefly cut shoots growing in water culture. He set out to investigate the fundamental problem of the path taken by organic food materials in their upward passage through the stem during the spring season of active growth. Atkins considers that the carbohydrates pass through the xylem, an opinion he shares with earlier workers; but, as this conclusion is based mainly on the continued presence of these substances in the wood, Curtis is inclined to require more direct evidence of actual translocation through the xylem tissue. He lays emphasis on the possibility that these soluble substances may quite easily remain stationary in a tissue in spite of a current of water flowing through the cells containing them, and argues accordingly that the mere presence of sugars in the vessels is not evidence of their habitual conduction through those elements. Curtis found that growth practically ceases in a shoot if a ring of phloem is removed below it, unless the leaves are allowed to remain. On the other hand, if the leaves are detached and no phloem removed, growth, although retarded, is not completely stopped. Incidentally it is interesting to note that in some of the "ringed" shoots a strip of phloem was accidentally left intact. These shoots showed considerable growth, thus providing a valuable control showing that the check in growth observed in other cases could not be due to injury. Similar experiments were carried out on dormant shoots just previous to the period of bud-opening. Adequate control experiments were introduced, such as injuring the xylem whilst allowing some phloem to remain, and the results obtained were exactly comparable with those in the previous experiment. Experiments were also conducted with fruiting stems, in which a ring of phloem was removed from the stem below the fruit, and no leaves were

allowed to remain on the section of stem above the ring. The results obtained were identical with those recorded by Hanstein in 1860—namely, cessation of growth in the fruits above the ring. Moreover, Curtis was able to show, by dry weight and volume measurements, that the fruit gained nothing but water from the date of ringing. The author concludes that either the xylem carried no essential food material, or alternatively, whilst the xylem may carry some, the phloem carries the important constituents, *e.g.* possibly nitrogenous compounds.

In the cases where leaves are allowed to remain on a ringed shoot, their photosynthetic activity in some degree compensates for the loss of food material normally carried by the phloem ; as is shown by the considerable growth of the shoot under these conditions. In addition, the osmotic concentration above the ring is found to be greater in the presence than in the absence of these leaves, additional evidence of the part they play. Curtis completed his case by employing several different methods to extract and estimate the carbohydrates of his ringed shoots, and was able to supplement the results of his growth measurements by showing that carbohydrates could not pass a gap in the phloem of the stem, either upwards or downwards. Perhaps the most striking result obtained is in the case of the sugar maple, *Acer saccharum*, which bleeds sugar solution when it is tapped, yet does not remove its carbohydrates from the xylem of a length of stem when the path through the phloem has been interrupted.

Winter storage of nutrient materials has been studied by E. W. Sinnott (*Botanical Gazette*, 1918, **16**, 162–75) from the anatomical standpoint. Previous investigations on the relation of anatomical structure to the nature of the stored food materials have resulted in the broad classification of trees into starch-storing and fat-storing, the former retaining starch in wood and pith throughout the winter, and the latter replacing starch by fat in all its tissues in the autumn. From examination of a large number of species, Sinnott is able to confirm in general the results obtained by other workers, but finds that the line of distinction between " starch trees " and " fat trees " is not well defined, many types storing both starch and fat in large quantities. A study of these cases convinced the author that the character associated with starch storage was a thick or unpitted cell wall, whilst fat was accumulated in cells with thin or much pitted walls. This fact, together with the position of " fat cells " in close proximity to water channels, led to the suggestion that it is a question of the conversion of starch into fatty materials, a process which appears to be dependent upon easy access to the water, or possibly to enzymes in the water of the vessels. Some co-ordination of

this work with the observations of Craib appears desirable, although, in view of the relatively small seasonal changes of water content of sap wood, and the high permeability of the wood to water, it may be doubted whether there is anything of the nature of competition for water by the tissues.

Some experiments with reference to the winter storage of starch and fat have been carried out upon herbaceous plants growing under the severe conditions of Western Canada. Gwynethe M. Tuttle (*Annals of Botany*, 1919, **33**, 201–10) has found it possible to influence the starch fat ratio in evergreens. Normally, starch disappears in autumn and oil takes its place as a food reserve, starch reappearing with the advent of the growing season. It was found that these changes could be induced at will in the case of *Linnæa* by exposure to suitable temperature conditions. Starch reappeared in oil-containing cells within two days of their removal from the outside cold to the laboratory temperature of 20° C. Reappearance of oil and loss of starch can then be induced by exposure to moderately low temperatures (about freezing-point), but subjecting the plants at once to the winter temperature of 15° to 30° C. below zero results in death. This work emphasises the point previously noted—namely, the protection against freezing derived by the plant from this capacity to vary the nature of its food reserves. The question of the formation of starch from oil, and *vice versa*, is also discussed in this paper, and on the basis of the localisation of starch at its first appearance (as if surrounding an oil globule), and of the occurrence of lipase and oxidase in the leaf, the author inclines to the view that direct transformation takes place. The records of this investigation are continued by F. J. Lewis and Gwynethe M. Tuttle (*Annals of Botany*, 1920, **34**, 405–16), who carried out serial determinations of osmotic pressure, electrical conductivity, and amounts of sucrose, maltose, and glucose in leaf tissues of both woody and herbaceous evergreens. They hesitate to record any correlation between these values and the temperature records on the results of the work of one season only, but they note the occurrences of maximum osmotic concentration either in December or in March. They found, as Chandler did, that the osmotic pressure was due chiefly to non-electrolytes, the concentration of electrolytes changing but little throughout the season. Sugars disappear progressively from winter to summer, glucose being the last to be removed. An important fact in relation to resistance to frost-injury is revealed by supercooling experiments. Whereas the sap extracted from leaves of *Pyrola* froze at about –3° C., the leaves themselves could be cooled to –32° C. before freezing. A significant observation is also recorded regarding the chloroplast, which

was seen to become disorganised during the winter months, the chlorophyll distributing itself around the nucleus, coalescing to form plastids again in April.

PALÆONTOLOGY. By W. P. Pycraft, F.Z.S., A.L.S., F.R.I.B.A., British Museum (Natural History), London.

But little freshly garnered material seems to have come into the hands of the Palæontologists during the last few months, since most of the published work which has been achieved is concerned with the harvest of pre-war days. This, however, is not a matter for surprise.

Dr. Jacob L. Wortman contributes an extremely important paper on some hitherto unrecognised reptilian characters in the skull of the Insectivora and other mammals, to the *Proc. U.S. Nat. Mus.*, vol. lvii, 1920, which must be very carefully studied by palæontologists and students of recent mammals alike, for his discoveries will have far-reaching importance. Among other things, he suggests that we should regard the "malar foramen" of Tupaia of certain primates, and possibly of the fruit-eating bats, as the remains of the lateral temporal vacuity of the reptilian skull; and he insists that the mammalian auditory chain originally arose from a chain of elements similar, in all respects, to that now found in the Anourous Batrachia. The long-cherished hypothesis of the intercalation of the quadrate and articular into the mammalian auditory chain he regards as "monstrously improbable if not altogether impossible." Finally, he contends that the Cynodonts can no longer be regarded as the long-sought ancestors of the mammalia. They belong, on the contrary, to the Sauropsida.

Messrs. Gerrit Miller and James Gidley describe a new rodent from the Upper Oligocene of France (*Bull. Am. Mus. Nat. Hist.*, vol. xli). The fragment of a skull on which this description is based was found by Porrier, nearly sixty years ago, in a calcareous butte at Peu-Blanc, Commune of Sorbier, and soon after seems to have passed into the Cope Collection. Till now it has never been carefully examined. The authors have named it *Rhizospalax porrieri*. While its exact position cannot be determined, owing to its fragmentary character, it seems clear that it shows a combination of peculiarities found in the living *Spalax*, *Myospalax*, and *Tachyoryctes*, though it cannot be regarded as ancestral to any of its living relatives.

Renewed exploration of the Huerfano Basin of Colorado, under the auspices of the American Museum of Natural History, has resulted in very important additions to our knowledge of the Titanotheres of the Lower Eocene, and of the relations thereto to the little-known fauna of the base of the Bridger Formation, Wyoming, known as Bridger A. Com-

plete reports of this survey are in course of preparation. Meanwhile Prof. H. F. Osborn describes (*Bull. Am. Mus. Nat. Hist.*, vol. xli, 1919), three new species of Titanotheres, and makes some important additions to our knowledge of the Huerfano beds, which are now shown to attain a thickness of 3,500 feet.

Palæontologists have, till now, failed to recognise the marked differences between the peccaries and the true pigs. Mr. James Gidley, in an important paper on Pleistocene peccaries from the Cumberland Cave deposit, Maryland, sets the matter finally at rest (*Proc. U.S. Nat. Mus.*, vol. lvii). In a series of carefully compiled tables he compares the several fossil species which have already been described, and supplements these with descriptions of four new species. A number of excellent illustrations adds much to the value of his work.

That the Great Auk (*Alca impennis*) ranged much farther south than has hitherto been supposed has been demonstrated by Dr. C. W. Andrews, who describes (*Ann. & Mag. Nat. Hist.*, vol. v, July 1920) the upper end of a humerus from a cave in St. Brelade's Bay, Jersey : and further adds that the figure of a bird, supposed to represent the Great Auk, has recently been found in a recess of the Cavern of Gargas (Hautes Pyrénées).

Dr. C. W. Andrews also describes (*Ann. & Mag. Nat. Hist.*, vol. v, Jan. 1920) two new species of fossil Tortoises which are of considerable interest. The first of these was derived some years ago from the upper Greensand of Melbury Down, near Shaftesbury, Dorset ; and for years was used for blocking a gate open. As a consequence the carapace has been much damaged, but the plastron is beautifully preserved. This specimen, which belongs apparently to the Pleurodira, has been named *Trachydermochelys rutteri*, in honour of Mr. Clarence Rutter, who has presented it to the British Museum. It measures 18½ inches in length, and represents the third specimen of this genus found in Great Britain. The second specimen is represented by part of a carapace from the Barton Clay at the foot of Highcliff, near Christchurch, Hants. When complete it must have measured about 28 inches in length. It apparently belongs to the family Emydæ, and represents a new genus and species—*Patanemys bartonensis*—and is now in the collection of the Museum of Practical Geology, Jermyn Street, London.

Many years ago, Dr. John Ryder, under the direction of Prof. Cope, attempted a restoration of the Sauropod dinosaur *Camerosaurus*. It was a far from convincing piece of work, and contained one or two rather curious errors of judgment. Prof. H. F. Osborn, assisted by Mr. C. C. Mook, has made a fresh attempt to restore this skeleton and the appearance of the living animal (*Proc. Am. Phil. Soc.*, vol. lviii, 1919).

Though he points out that the result, in some particulars, is based on conjecture, a careful study of the facts submitted will show that in all essentials this restoration will stand. It is certainly a vast improvement on that which it has replaced.

Our knowledge of *Lysorophus*, the most remarkable land vertebrate which has been discovered for many years, has been extensively enlarged by Prof. W. J. Sollas (*Phil. Trans. Roy. Soc.*, Ser. B, vol. ccix). The nodule containing the remains was cut into a series of sections, at intervals of 0·2 mm.; each of which was photographed under an enlargement of five diameters. The photographs were then traced on to glass plates, and then reconstructed in plaster. This method enabled him to build up the skull and other parts of the skeleton, so that the restoration can be examined as easily as if it were a recently macerated and articulated skeleton. This is without doubt a laborious process, but, in the hands of Prof. Sollas, it yields most wonderful results.

Till now *Lysorophus* has been regarded by some as a reptile, by others as an amphibian. Prof. Sollas is now able to show definitely that it is a veritable but primitive Amphibian, and a member of the primitive ancestral Urodeles. The relationship of the Gymnophiona to the Urodeles is but one of many new facts which has been brought to light, and recorded here as a result of this investigation.

We do not, as yet, know how the Portage Sea of the Mackenzie-Yukon region was connected with the synchronous marine basin which occupied the Upper Mississippi, Wabash, and New York area. But a considerable advance in this direction has resulted from the Canadian Geological Survey expedition to the Mackenzie River in 1917, when a Portage fauna was discovered in the Devonian shales of the Upper Mackenzie Valley. Mr. E. M. Kindle, who directed this expedition, gives the results thereof (*Canadian Geol. Survey, Mus. Bull.*, No. 29, *Geol. Series*, No. 36) in a brief but well illustrated memoir, which is likely to lead to very important results.

Some extremely valuable data have been derived by Mr. R. Bullen Newton (*Ann. & Mag. Nat. Hist.*, vol. v, No. 27) from a study of some obscure fossils from Matabeleland. These remains, representing freshwater mollusca resembling *Viviparus* and *Paludestrina*, and plants of the genus *Chara*, were embedded in a chalcedonised rock, occurring in a peneplain of Upper Karoo Beds, and at the base of the Pleistocene deposits known as the Kalahari Sands, which, in this region of Africa, mostly cover the basalts and the other underlying formations. This formation, it would seem, extending from the Zambesi country to Cape Colony, may be older than Eocene, and from the assemblage of the contained organisms, may be associated

in time with the land platform which united Africa with India during Cretaceous times.

British Eocene insects, in the collections of the British Museum of Natural History, are described by Dr. T. D. A. Cockerell, of the University of Colorado. "The most remarkable find," we are told (*Ann. & Mag. Nat. Hist.*, vol. v, No. 27), "is a large wing belonging to the Mesozoic family Pseudosiricidæ. Its discovery is almost as startling as that of a Tertiary dinosaur.

Finally, mention must be made of a small volume on *Invertebrate Palæontology*, by Mr. Herbert Leader Hawkins, (Methuen & Co.). In the first part of the book the author endeavours to "show something of what is meant by 'Palæontology.'" The second part is designed to give an epitome of the sequence of evolution as it has been unfolded in geological time. The result is admirable.

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S., Zoological Laboratory, Cambridge.

The *Journal of the Royal Anthropological Institute* for the second half of 1919 (vol. xlix, pt. 2) is now to hand. The journal contains an extraordinary number of articles dealing with Indonesia, but the first article is concerned with a problem nearer home. This is by Harold Peake, and is entitled "The Finnic Question and Some Baltic Problems." As all readers will be aware, it was universally held, until quite recent years, that the true Finns were rightly classed as an Asiatic and Mongoloid race, comparable to the Lapps and Samoyeds. Recently, however, an entirely different view has been put forward, and has gained some acceptance. Ripley, Giuffrida-Ruggeri, and others contend that the true Finns are represented by the typical Nordic inhabitants of the country; and that the language, and the very special Finnish folk-lore and culture, have been only secondarily imposed upon the Mongoloid people of the district. It will be remembered that, on the old theory, the Nordic element (which undoubtedly exists in Finland) was merely considered to be due to a Scandinavian, particularly, of course, a Swedish, infusion. Peake argues the whole question at length. He shows that the great bulk of the population of Finland is short and brachycephalic, and that in districts where the tall dolichocephalic element exists in considerable numbers, the Swedish language is also significantly prevalent. And the author continues: "Now the tall, blonde, long-headed Finns seem, as a rule, to be indistinguishable from a similar type in Courland, from the Letts, Lithuanians, Swedes, and the tall fair type of Englishman. All those west of Finland and south of the Livs speak Aryan languages, nearly all of them Teutonic dialects. On the other hand, we find the Finnic tongue spoken in the

east, from Nijni Novgorod to the Urals, and from Samara nearly to the Arctic Ocean. The Finnic dialects belong to a group of languages known as Ugrian, spoken over large tracts of Western Siberia, and the Ugrian languages are thought by some to be part of a greater group, formerly called Turanian, but now termed Ural-Altaic, which stretch in an almost complete belt from Lapland to Korea—some would say to Japan." Peake comes to the conclusion that the old view is correct—that the Finn properly so called is a Mongolian. It is difficult to doubt that he is right. The alternative is to suppose that a part of the Nordic race, speaking an utterly different language from its kindred of the West, imposed its language and culture on Mongolian peoples, spread all over Eastern Europe and Western Asia, without leaving any serious trace, physical or otherwise, of this extensive conquest. It is, I think, much more natural to infer that the few Nordics of Finland are Teutons who have lost their language and culture. In the later part of the article Peake discusses other Baltic questions, including the important problem as to the earliest inhabitants of Scandinavia. The author quotes Prof. Montelius as saying that the first race gaining entry to Scandinavia after the last ice age was dolichocephalic, and was directly ancestral to the present inhabitants of the country. Peake deals with the early migrations very fully, and puts forward an hypothesis that, before the Nordic incursion into Scandinavia, the country had already been occupied by a Lapponic people.

In the new issue of the *American Journal of Physical Anthropology* (vol. iii, No. 1, January–March, 1920), the first article is by Philip Newton, and is entitled, " Observations on the Negritos of the Philippine Islands." The paper embodies the results of an expedition to the chief subdivisions of the Negritos which the author carried out in the summer of 1912, the research being directed by Dr. A. Hrdlicka, and being supported by the Smithsonian Institution and by the Pan-American Exposition of San Diego. The observations were limited as far as possible to pure-blooded Negritos. According to the latest Government census, there were estimated to be 25,000 Negritos in the islands, but Dr. Newton considers that the great majority of these were of mixed blood. He estimates that, aside from the population of an unexplored district in Luzon, the number of pure Negritos falls short of 5,000. In a letter transmitting his report to Dr. Hrdlicka, Dr. Newton says : " The Negritos answered freely all questions they could understand. No information as to their number, births, and deaths, etc., could be obtained from the Philippine Government, for the simple reason that they had none. The Negrito has no unit of time, so knew nothing of his age ; all ages, therefore, are approximate

only." The Negritos live in the hill districts, whither they have evidently taken refuge from the more powerful native tribes who arrived more recently in the Philippines. In regard to their occupations, the author says that the Negrito, in his truly natural state, " has no occupation other than hunting game and gathering wild fruit and roots for his subsistence. The men construct shelters and make their bows and arrows." A curious physiological character is the slow rate of reproduction observed. In ninety-three families from which particulars were collected, the average number of children was only 2·27.

Turning to a different subject, I may mention that in *Man* for July 1920, E. G. Fenton returns to the subject of the alleged ancient " cart-ruts " of Malta, about which there was a considerable controversy in 1918, and to which reference was made in SCIENCE PROGRESS. Two photographs of these ruts are published in *Man*.

The now familiar eugenic argument relating to the racial desirability of the upper classes has been reiterated recently by more than one writer, notably by William McDougall in *Anthropology and History* (H. Milford), which was the Twenty-Second Robert Boyle Lecture, delivered at Oxford in June; and also by Warren S. Thompson, in " Race Suicide in the United States," an article in the above-mentioned number of the *American Journal of Physical Anthropology*. The former article is written with force and originality, but the latter is, for the most part, singularly trite.

The following articles on physical anthropology may be mentioned:

In the *American Journal of Physical Anthropology*, vol. iii, No. 1: " The Indian Brain " (*i.e.* American Indian), by J. J. Keegan; " A New Craniometric Method, including a Description of a Specially Designed Indexometer for Estimating it," by John Cameron; " Aspects of the Skull; how shall they be Represented? " by G. G. Maccurdy; and " Multiple Births among the Chinese," by B. Laufer. And in the *Journal of the Royal Anthropological Institute*, vol. xlix (July to December 1919): " Some Observations on the Physical Characters of the Mendo Nation " (Sierra Leone), by F. W. H. Migeod.

And the following articles on social anthropology may be mentioned:

In the *Journal of the Royal Anthropological Institute*, vol. xlix (July to December 1919): " Some Personal Experiences in British New Guinea," by Dr. W. M. Strong; " Stone-work and Gold-fields in British New Guinea," by E. W. P. Chinnery; " String Figures from New Caledonia and the Loyalty Islands," by R. H. Compton; and " The Languages of Northern Papua," by S. H. Ray. And in *Man*: " The Stoney Indians," by A. C. Breton (May); and " The Mackie Ethnological Expedition to Central Africa," by Sir James G. Frazer.

ARTICLES

MASS-SPECTRA AND THE ATOMIC WEIGHTS OF THE ELEMENTS

By F. W. Aston, M.A., D.Sc., A.I.C., Trinity College, Cambridge.

Some ten years ago two widely different sets of experiments were in progress the results of which were to shake, and in the end destroy, one of the best established articles of scientific faith: Dalton's postulate that the atoms of an element were equal to each other in weight. One of these was the work of Sir Ernest Rutherford and his colleagues on radio-active disintegration, the other was Sir J. J. Thomson's analysis of Positive Rays.

The results of the first of these researches led inevitably to the conclusion that it was possible to obtain quite a number of elements whose chemical properties were identical with those of lead, but whose atomic weights not only differed from each other in a measurable degree, but also from the accepted atomic weight of lead. Other branches of radio-active transformation proved the same remarkable result in the case of thorium. The theory that elements of different atomic weight could still be chemically identical and occupy the same position in the periodic table was largely developed by Prof. Soddy, who, in view of the latter property, called them "isotopes." As far as lead is concerned, the existence of its isotopes has recently been triumphantly vindicated by the production, in quantities ample for chemical determination, of varieties chemically indistinguishable but yielding atomic weights differing by quite unmistakable amounts. As the present article is concerned with the constitution of the lighter and non-radio-active elements, it will be sufficient to state that the possibility of the existence of isotopes among the group of elements studied in radio-active change is now no longer in question.

The first suggestion of the existence of isotopes among the lighter elements was afforded by the anomalous behaviour of the monatomic gas neon when subjected to positive-ray analysis by Sir J. J. Thomson. The method of analysis then used has

already been the subject of an article in SCIENCE PROGRESS of July 1912; it consists briefly in subjecting the rays to electric and magnetic fields applied simultaneously and in such a manner that their deflections are at right angles to each other. Under these conditions particles having the same mass but variable velocity will appear on the photographic record obtained as a parabolic streak. The atoms of the ordinary elements tried all gave single, or at least apparently single, parabolas, but whenever neon was present in the discharge tube, *two parabolas* appeared instead of the one expected.

The position of these two parabolas showed that the atomic weights of the atoms causing them were, roughly, 20 and 22 respectively. The latter was always about one-tenth the brightness of the former, but otherwise similar in all respects. The details of this similarity and other lines of reasoning made it appear highly probable that the parabolas were caused by two elements of exceedingly similar properties, and that it was very unlikely that the 22 parabola could be explained away by a hypothetical NeH_2 or, *e.g.*, CO_2 with a double charge. The idea that neon was a mixture of two isotopes, and that the parabolas were due to these, naturally arose, and the author undertook a systematic investigation to throw light on this important point.

Attempts to Separate the Isotopes of Neon.—At the time (1913) when this was started, one had no idea of the amazing degree of similarity between the isotopes of an element. The constituents of neon were regarded as two distinct monatomic elements occurring in the atmosphere, and for convenience the hypothetical constituent was called "metaneon." The accuracy of positive-ray analysis then available was not sufficient to distinguish between the atomic weight of the element causing the lighter parabola and the accepted atomic weight (20·20) of atmospheric neon, so that the most hopeful line of attack seemed that of separation. The first method tried was fractional distillation. The vapour pressure of neon over charcoal cooled in liquid air is of a convenient order, and a pure specimen of the gas was carefully fractionated over this substance in a specially designed apparatus. At the same time a quartz microbalance was devised to determine the densities of the fractions with rapidity and with an accuracy of $\frac{1}{10}$ per cent. The result of a laborious series of several thousand operations was completely negative within the error of experiment; it is, however, of some satisfaction to the operator to know now that this result was probably inevitable, as has been recently shown by Prof. Lindemann from theoretical considerations.

The second attempt at separation was made by means of

fractional diffusion through pipeclay. If molecules of different masses exist in a gas, they *must* be separable, at least partially, by this method; but unless the masses are very different, it is extremely slow. The two neons only differ by 10 per cent., and it was only after many months of tedious work that a positive result was obtained, and a difference of 0·7 per cent. measured between the two extreme fractions. Further diffusion experiments were in progress when the war interrupted the work, for although evidence as to the existence of the isotopes of neon was then available from several different lines of reasoning, and a positive result had been obtained of their partial separation, the latter was considered too small to settle such an important point in a satisfactory manner.

By the time work was recommenced in 1919, the "isotope" theory was generally accepted as regards the radio-active elements, and a good deal of interesting theoretical speculation had been made as to the possibility of it applying to the elements in general; it was therefore very desirable that the case of neon should be put beyond dispute. As diffusion experiments were at the best extremely slow and laborious, attention was once more turned to the possibilities of positive rays. Thomson's method of analysis, as previously used in the work, was not capable of very great resolution; it could separate masses such as the two isotopes of neon, but, as has been already stated, was incapable of distinguishing between 20·0 and 20·2 with certainty. If a method could be found of sufficient precision, and it could be demonstrated that the accepted atomic weight of neon lay between the two values given by positive rays, the question would be settled; this it was found possible to do as follows:

The "Mass-Spectrograph."—The principle of this instrument is indicated in Fig. 1. The positive rays, which are charged atoms and molecules of matter moving with high velocities, are generated in the usual manner in a large discharge tube similar to an X-ray bulb, and are allowed to pass through an aperture in the cathode. In Thomson's apparatus the aperture consisted of a long fine tube giving a narrow circular beam or string of rays; in the new arrangement, two very fine slits s_1, s_2 are employed, giving a thin ribbon of rays. These slits are made of aluminium by a special method. In the present apparatus they are 1 mm. long by ·05 mm. wide, and mounted about 100 mm. apart, so that the beam produced is exceedingly narrow and would, if undeflected, make a fine sharp line on the recording plate. The space between the slits is exhausted as highly as possible, as also is the "camera" in which the analysis takes place.

The beam of rays produced then passes between the two

charged plates P_1, P_2. As the charged particles pass through the electric field, they are each deflected and so spread out into an "electric spectrum," the deflections being inversely proportional to the *energy* of the particles. The rays now behave as though radiating from a virtual source, Z, and a group of them is selected by the diaphragm D, and allowed to pass between the poles of a powerful electromagnet. These poles are circular, and as a magnetic field deflects the particles inversely as their *momentum*, it can be shown that if the angle through which they are bent in this case is more than double the foregoing electric deflection, and in the opposite direction,

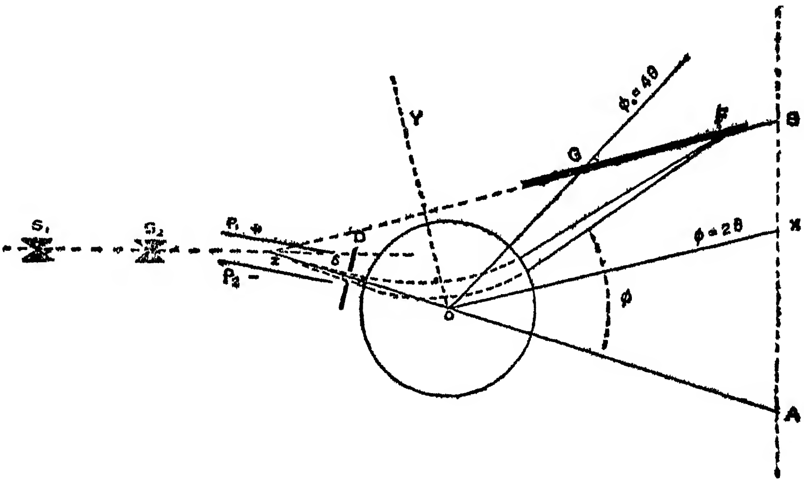


Fig. 1.—Positive-ray spectrograph.

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rays of *constant mass* will come to a focus F as indicated in the diagram. Also, if a photographic plate is placed at GF in the line passing through Z, it will be approximately in focus for all rays whatever their masses. The position of the focal points along such a plate depends only upon the masses—or, to be precise, the ratios of mass to charge—of the particles causing them, so the result obtained is called a "mass-spectrum." Just as in the ordinary spectrum of a gas the image of the slit is deflected according to the wave-lengths of the light, giving the familiar appearance of bright lines, so in the mass-spectrum the image caused by one of the slits collimated by the other will appear at different points, each point representing a definite mass.

The exact mathematical relation between the positions of the focal points or lines on the mass-spectrum and the masses causing them is by no means simple. But, fortunately, it is not

necessary to evaluate it, as there are so many lines whose masses are known which can be therefore taken as standards. What is actually done is to take a fixed point called the register spot, which is photographed as a circular dot on each spectrum (see Fig. 2), and measure the relative distances of known and unknown lines from this. By altering the magnetic field, a chosen group of known lines can be photographed in various positions. In this way the plate can be fully calibrated, a process which is assisted materially by the fact that at the

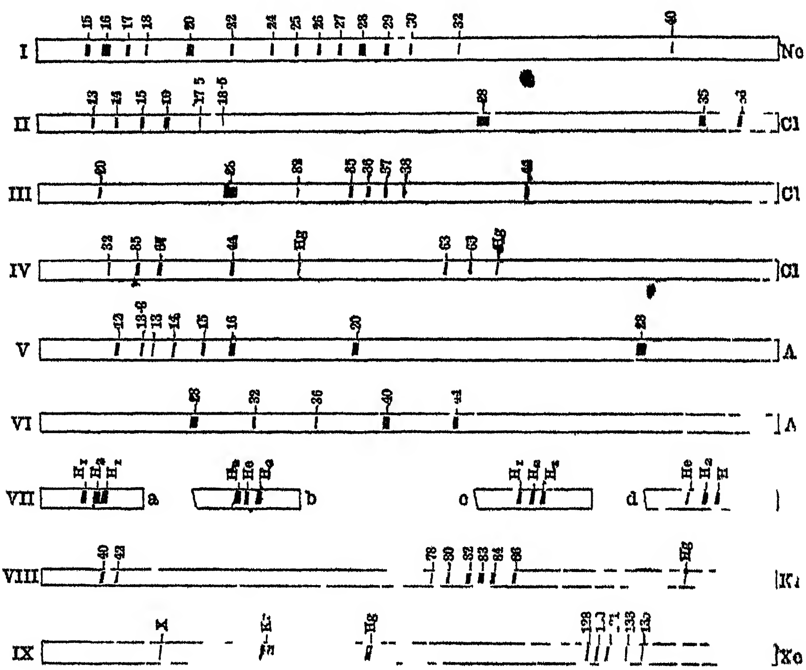

FIG. 2.—Typical mass-spectra.
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end corresponding to greatest deflection—the left-hand end in the illustrations,—the relation between position and mass happens to be nearly linear.

An excellent gas for calibration is a mixture of carbon dioxide and methane. Taking the brighter lines given by this mixture in order (omitting the hydrogen atom and molecule), we have first a very well marked and easily recognised group called the C_1 group: this consists of $12 - C$, $13 - CH$, $14 - CH_2$, $15 - CH_3$, $16 - CH_4$ or O. After a gap this is followed by the C_2 group 24, 25, 26, 27, 28, 29, 30, starting with what

appears to be a diatomic carbon molecule, and going on with successive additions of hydrogen atoms. This group contains the very strong reference line 28 due to C_2H_4 or CO. The next line is 32, the oxygen molecule, usually faint; and finally we have 44, due to CO_2. The C_1 group is well illustrated in Spectrum V, and the C_2 group in Spectra II and III. In calculating masses, the "Oxygen" scale is used—that is to say, the oxygen atom is taken as of mass 16 units. We have no evidence whatever of the presence of isotopes in oxygen or carbon; indeed, from the extremely close whole-number relation of their chemical atomic weights, none was expected. Hence carbon may safely be taken as 12·00 to the accuracy of experiment, giving us at once a direct scale of reference for masses up to 44.

Lines due to Multiply Charged Particles.—As has been pointed out, what one really measures is not mass, but the ratio of mass to charge. When the latter is the unit *e* itself (the fundamental unit of negative electricity: 4·77 10^{-10} C.G.S.), the lines give a measure of the masses themselves, and are called lines of the *first order.* If, however, a particle carries a charge 2*e*, it will behave exactly as if it had half its normal mass; with a charge 3*e*, as if it had one-third its normal mass, and so on. Lines formed in this way are called lines of the *second*, *third*, and *higher orders*. Positive rays carrying multiple charges have been fully investigated by Sir J. J. Thomson. The atoms of most of the elements employed, with the exception of hydrogen, seem capable of carrying two charges; some, such as argon, krypton, and xenon, appear with three or more; while mercury can carry no less than eight. First order lines are always brighter, generally much brighter, than the corresponding ones of the second order; the second than the third, and so on in decreasing strength.

The mechanical details of the actual apparatus used to obtain the following results have been given elsewhere (*Phil. Mag.*, vol. xxxix, May 1920, p. 611). It was somewhat difficult to construct and adjust, but even before the most advantageous position of the plate had been fixed, it was realised that the resolution was ample to settle the question of neon. The results obtained with this and a few of the other more important elements will now be considered in order.

Neon (*At. Wt.* 20·20).—On introducing a little of this gas into the mixture of CO_2 and CH_4 used for calibration, the new lines expected were at once recognised. These were first order lines at 20 and 22, and second order lines at 10 and 11. All four of these were well placed for comparison, the first two between the C_1 and C_2 groups, the second two between the C_1 group and the second order oxygen line 8. Several sets of

careful measurements showed the masses of the two isotopic constituents of neon to be 20·00 and 22·00 to an accuracy of $\frac{1}{10}$ per cent. on the oxygen scale. In order to give the accepted density, the relative quantities required are 90 per cent. and 10 per cent., which is in good agreement with the estimated intensity of the lines.

It is as well to state here that the photographic intensity of the line is not, in general, to be regarded as a measure of the quantity of the substance present. With two isotopes of the same element, however, the chemical identity makes it practically certain that the intensity is a reliable criterion—this, of course, only refers to the intensity of lines on the original negative, not to reproductions.

The two first order lines of neon can be seen on Spectrum I between the two carbon groups. The above results obtained for their masses prove the complex nature of atmospheric neon beyond dispute. Faint indications of a third constituent of mass 21 were obtained, but this can only be regarded as doubtful, and in any case only present in the minutest proportions.

Chlorine (*At. Wt.* 35·46).—The very marked fractional atomic weight of chlorine has been the subject of much interest and discussion in the past. It was naturally the next element to be investigated. The question whether its accepted atomic weight represented the true mass of its individual atoms or was merely a statistical mean possessed a far deeper chemical significance than that at issue in the case of neon, which is inactive chemically. It was not long in doubt, for the photographs obtained showed that the introduction of chlorine into the discharge tube gave rise to a group of strong lines corresponding to masses 35, 36, 37, and 38, all of which were whole numbers to the accuracy of experiment. *There was no indication whatever of a line corresponding to a mass* 35·46. Spectra II, III, IV were obtained with chlorine; on the first, which was taken with the smallest magnetic field, two faint lines can be seen at 17·5 and 18·5. These are the second order lines of 35 and 37, and are strong testimony that the latter are elementary, for it is extremely rare for compounds to give second order lines at all. Further evidence on the same point is given by the two lines 63, 65, Spectrum IV, which are doubtless due to two compounds, COCl.

These figures leave no escape from the conclusion that the element chlorine is complex, and two of its constituents have atomic weights 35 and 36. It might be argued that 36 and 38 are elementary lines, and at present there is no evidence to deny this; it is much more likely that they are due to the two hydrochloric acids. In all spectra taken with chlorine present,

a faint line is distinguishable corresponding to 39. It is just possible that this is a third isotope.

At first sight it may seem incredible that chlorine, whose chemical combining weight has been determined more often and with greater accuracy than almost any other element, should not have given evidence of its isotopic nature in the past. But it must be remembered that, in all probability, every one of these determinations has been performed with chlorine originally derived from the sea in which the isotopes, if ever separate, must have been perfectly mixed from the most remote ages. Chlorine from some other source, if such can be found, may well give a different result, as did radio-lead when examined.

Argon (*At. Wt.* 39·88).—The spectra obtained from this element show its main constituent to be of mass 40, with second order line 20 and third order line 13·33 (Spectra V and VI). The last is very well placed for measurement, and from it the mass of the singly charged atom is found to be 40·00 ± ·02. At first this was thought to be the only constituent, but later a faint companion was seen at 36, which further spectra showed to bear a very definite intensity relation to the 40 line. This is probably an isotope the presence of which to the extent of about 3 per cent. would account for the fractional atomic weight.

Hydrogen (*At. Wt.* 1·008) *and Helium* (*At. Wt.* 3·99).—The determination of masses so far removed from the reference lines as these offered peculiar difficulties; but as the lines were expected to approximate to the terms of the progression 1, 2, 4, 8, etc., the higher terms of which are known, a special method was adopted by which a 2 to 1 relation could be tested. From the dynamics of the instrument it can be shown that, if an exposure is made and then the potential applied to the electric plates exactly doubled, and another exposure given, the magnetic field being kept constant, all masses having a 2 to 1 relation will be brought into coincidence on the plate. Such coincidences cannot be detected on the same spectrum photographically; but if we first add and then subtract a small potential from one of the above potentials, two lines will be obtained which will closely bracket the third. Thus the hydrogen molecule line was found symmetrically bracketed by a pair of atomic lines (Spectrum VII, *a* and *c*), showing that the mass of the atom is exactly half the mass of the molecule. When the same procedure was applied to the helium line and that of the hydrogen molecule, the bracket was no longer symmetrical (Spectrum VII, *b*), nor was it when the hydrogen molecule was bracketed by two helium lines (*d*). Both results show in an unmistakable manner that

the mass of He is less than twice that of H_2. On carrying on the ratio in the same way to the second order line of oxygen, it is found that helium is exactly 4, but that hydrogen gives a value the same as that obtained by chemists, namely 1·008.

Krypton (*At. Wt.* 82·92) *and Xenon* (*At. Wt.* 130·2).—The results with these elements were particularly interesting, as they proved to have a quite unexpected complexity. Also, as they are inactive gases, there is no question of confusion of the evidence due to possible hydrogen compounds. Krypton gives a remarkable group of five strong lines at 80, 82, 83, 84, 86, and a faint sixth at 78. This group or cluster of isotopes is beautifully reproduced, with the same relative intensities in the second, and fainter still in the third order. These multiply-charged clusters give most reliable values of mass, as the second order can be compared with A (40) and the third with CO (28) with the highest accuracy. The singly and doubly charged krypton clusters can be seen to the right and left of Spectrum VIII.

Only the minutest trace of Xenon was available to determine its constitution. The five lines to the right of Spectrum IX indicate that provisionally it may be said to be a mixture of five isotopes of masses 128, 130, 131, 133, 135.

Mercury (*At. Wt.* 200·6).—Owing to the presence of mercury vapour (which is generally beneficial to the smooth running of the discharge), the lines of this element appear on nearly all the plates taken. They do so as a series of blurred clusters of decreasing intensity around points corresponding to 200, 100, 66·6, 50, etc., some of which are indicated in the spectra reproduced. It may be stated provisionally that they indicate a strong component 202, a weak one 204, and a strong band from 197 to 200 containing three or four more, unresolvable at present.

It is not proposed to discuss in detail all the other elements analysed, particularly as the evidence in many cases is by no means simple; a table of the results so far obtained is given at the end of the article. The case of the element bromine is of some special interest, for, in spite of the fact that its chemical combining weight is almost exactly a whole number (80), it actually consists of approximately equal parts of 79 and 81. Boron is the lightest element found to consist of isotopes.

The Whole-Number Rule.—The most important generalisation yielded by these experiments is the remarkable fact that *all* masses—atomic or molecular, element or compound—so far measured are whole numbers on the oxygen scale within about 0·05 or less. It is perhaps premature to state that this relation is true of all elements, but the number and variety of

those already exhibiting it makes the probability of this extremely high. The idea put forward by Prout, that all elements were built of atoms of a common substance which he called " Protyle," is therefore to a certain extent substantiated. The atom of protyle regarded according to modern views is a neutral pair of oppositely charged atoms of electricity closely combined, as in the nucleus of helium or that of oxygen, not comparatively widely separated as in the atom of hydrogen. The fact that the positively charged atom of hydrogen—which must be the positive atom of electricity itself—differs from unity to a measurable extent is probably the result of its not being associated with a negative charge, but free to exercise its maximum mass effect.

General Considerations and Conclusions.—The Rutherford-Bohr atom model consists of a central nucleus around which rotate planetary electrons. The mass of an electron is 0·00054 on our scale, so that the whole mass of the atom can be regarded as residing in the nucleus. The number of planetary electrons on a neutral atom must, of course, be equal to the number of excess positive charges on the nucleus, which is most probably Moseley's atomic " number." All the chemical and spectroscopic properties of the atom seem to depend on these planetary electrons, and therefore on the *charge* on the nucleus, and only to an insignificant extent on its *mass*. Isotopes are elements having the same nuclear charge but different nuclear mass. Thus one may now suppose that an elementary atom of mass m may be changed to one of mass $m + 1$ by the addition of a positive particle and an electron. If both enter the nucleus an isotope results, for the nuclear charge is unaltered. It the positive particle only enters the nucleus, an element of next higher atomic number is formed. In cases where both forms of addition give a stable configuration, the two elements will be " isobares "—that is, elements of equal atomic weight but different chemical properties.

The only fact which appears to limit the number of isotopes of one element is that, given any number of positive and negative charges with which to build a nucleus having the necessary excess positive charge, only a few of the possible configurations will be stable enough to exist at all. Partially stable arrangements would correspond to radio-active elements. These considerations lead one to expect that the greater the total number of positive and negative charges, the greater the number of stable or partially stable configurations which can be built with them. So that the higher the atomic weight of an element, the more isotopes it may have, and the greater the chance of some of these being radio-active. Both these conclusions are in accordance with experience.

The fact that many, probably the majority, of the elements are mixtures of isotopes renders the chemistry of their compounds, theoretically, excessively complicated. On the other hand, it must be remembered that, so far as one can tell, the separation of the isotopes is so extremely difficult that practically this matter is unimportant, while the whole theoretical aspect of mass is most advantageously simplified by the whole number relation.

Table of Elements and Isotopes

Element.	Atomic Number.	Atomic Weight.	Minimum Number of Isotopes.	Masses of Isotopes in Order of Intensity.
H . . .	1	1·008	1	1·008
He . . .	2	3·99	1	4
B . . .	5	10·90	2	11, 10
C . . .	6	12·00	1	12
N . . .	7	14·01	1	14
O . . .	8	16·00	1	16
F . . .	9	19·00	1	19
Ne . . .	10	20·20	2	20, 22, [21]
Si . . .	14	28·30	2	28, 29, [30]
P . . .	15	31·04	1	31
S . . .	16	32·06	1	32
Cl . . .	17	35·46	2	35, 37, [39]
A . . .	18	39·88	[2]	40, [36]
As . . .	33	74·96	1	75
Br . . .	35	79·92	2	79, 81
Kr . .	36	82·92	6	84, 86, 82, 83, 80, 78
X . . .	54	130·20	5	[128, 131, 130, 133, 135]
Hg . . .	80	200·60	[6]	[197–200], 202, 204

(Numbers in brackets provisional only.)

THE MEASUREMENT OF SURFACE TENSION

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OF the many methods which have been devised for the measurement of surface tension, two only have been used to any extent in physico-chemical work. This is largely due to the fact that most of the methods which are otherwise satisfactory do not lend themselves to accurate temperature control (*e.g.* the method of the wave length of ripples), and since the temperature effect is of the order of 0·2 per cent. per degree, accurate temperature control is necessary. The two which have been most largely used are the capillary rise method and the falling drop method. The former depends upon the fact that, when a capillary tube is dipped into a liquid, the liquid rises to a height (h) in the tube given by the relation

$$2T \cos \omega = rg(\rho - \sigma)(h - x)$$

where T is the surface tension in dynes per sq. cm.
ω is the angle of contact of the liquid with the walls of the tube.
r is the radius of the capillary tube in cm.
g is the acceleration due to gravity in cm. per sec. per second.
ρ is the density of the liquid, and σ that of the vapour or gas with which it is in contact.
x is a correction factor for the liquid above the level of the bottom of the meniscus of the liquid in the tube.

The data given in Landolt-Bornstein's tables show that the values obtained by different experimenters for the surface tension of water at 20° C. by the capillary rise method vary between 70·6 and 72·7, while the values by all methods vary between 70·6 and 78. Thus, although a single experimenter gets

values which agree very well among themselves, they may differ by 3 per cent. from those of other workers. It is also noteworthy that the capillary rise results are generally lower than those obtained by other methods.

Before describing the method of measurement, it will be well to consider the causes of the discrepancies pointed out above. Willard Gibbs has shown that, in order to keep the potential energy of the system at a minimum, a solute which diminishes the surface tension will be concentrated in the surface layer, while a solute which raises the surface tension will have a lower concentration in the surface layer than in the main bulk of the liquid. Now the surface layer in a capillary tube has very small dimensions, and thus a very minute trace of impurity, which lowers the surface tension, is capable of giving a high concentration in this very small surface, and in consequence may produce a lowering of the surface tension altogether out of proportion to its mass.

The angle of contact has been generally assumed to be zero in the case of liquids which wet glass, and cos ω is then equal to 1; since cos $2°30'$ is 0·999 and cos $8°$ is 0·990, the errors which would be introduced by taking these angles as zero would be 0·1 and 1 per cent. respectively. In the case of a liquid such as water, there seems to be an element of doubt as to whether we are justified in taking the angle of contact to be zero. Some writers, *e.g.* Ferguson (SCIENCE PROGRESS, No. 35, 1915) and Matthews (*J. Phys. Chem.*, vol. xx, p. 554) think not; while others, *e.g.* Lord Rayleigh (*Science Papers*, vol. iii, p. 393), Bancroft (*J. Phys. Chem.*, vol. xxi, p. 407), and Richards and Coombs (*J. Amer. Chem. Soc.*, vol. xxxvii, p. 1656) either definitely consider the angle to be zero, or to be too small to be of any practical importance. While the weight of the evidence seems to be in favour of the latter view, further experimental investigation of the point is undoubtedly necessary.

The height h given in the formula represents the distance of the meniscus in the tube above the flat surface in the outer vessel. It is probable that one of the chief reasons for the variations between the results of different observers lies in the fact that they have assumed the surface in the outer tube to be flat, which is not the case. The question has been considered by Richards and Coombs (*loc. cit.*); they showed that in a tube 2 cm. in diameter, with a thin rod in the centre, water rose 0·31 mm. higher than in tubes on either side without the central rod; they found experimentally that a tube 3·8 cm. in diameter gave a flat surface at its centre, all the capillary rise taking place at the edges. Lord Rayleigh (*Proc. Roy. Soc.*, 1915) calculated that the minimum diameter for a flat surface would

be 4·7 cm. It should be noted that these values refer to tubes without a capillary tube in the centre, such as there is in the ordinary arrangement of, say, Ramsay and Shields' apparatus; with this arrangement a still wider tube would be required.

In calculating the height to which the liquid is raised, a correction has to be employed for the liquid contained in the meniscus above the lowest point (where the measurement is made). In very narrow tubes the surface of the liquid is practically hemispherical, and the volume of the liquid is then the difference between the volume of a cylinder of height r and that of a hemisphere of radius r (*i.e.*, $\pi r^3 - \frac{2}{3}\pi r^3 = \frac{1}{3}\pi r^3$), so that the correction is made by adding one-third of the radius of the tube to the observed height h. A more exact relation has been worked out by Poisson for tubes of small radius, who found

$$h = h_o + r/3 - 0{\cdot}1288r^2/h_o.$$

This result has been confirmed and extended by Lord Rayleigh, who gives

$$2T/g\rho r = h + r/3 - 0{\cdot}1288r^2/h + 0{\cdot}1312r^3/h^2.$$

Richards and Coombs measure the height h_m of the meniscus and use the expression $h = h_o + n.h_m$, where n varies from $\frac{1}{3}$ in tubes 2 mm. in diameter to $\frac{2}{3}$ in tubes 30 mm. across.

The first step in setting up an apparatus for the measurement of surface tension by the capillary rise method consists in the preparation of a capillary tube of uniform bore. A piece of glass tubing about 1 cm. in diameter and fairly thick walled is chosen: this is heated to redness at the centre until it softens. An assistant holding one end then walks rapidly away until the tube is pulled out to a length of ten or twelve feet. The central part of the capillary is cut into lengths of about 10 cm., and these are examined for uniformity. The capillary tubes can be cleaned by making a pin-hole in a piece of rubber tubing, one end of which is closed by a piece of glass rod and the other is attached to the water pump; one end of the capillary is pushed into the pin-hole, and the other is placed in a beaker containing the cleaning liquid, which is thus sucked through the tube, the usual liquids—chromic acid, distilled water, alcohol, and ether—being used. A thread of clean mercury is then drawn into the tube, and its length is measured at various points in the tube by laying it alongside a standard scale on a measuring microscope giving good magnification. A tube should be rejected which shows a variation in the length of the thread of more than 1 in 1,000.

When a satisfactory tube has been found, the length of the thread is recorded for the position nearest to which the meniscus will be formed, and the mercury is run out and weighed. The radius of the tube can then be calculated from the equation $r = \sqrt{m/\pi\rho l}$, where m is the mass of the mercury, ρ its density at the room temperature, and l the length of the thread corrected for the meniscus at each end by a quantity which can be obtained from a table. By means of a very fine sharp file, a small ring is then cut off the end of the capillary and is placed on the stage of a microscope with a micrometer eyepiece, and the diameter is measured in four directions at 45 degrees to find if it is truly circular, the same standard as before being required. If the cross-section is found to be satisfactory, a divided millimetre is placed on the stage, and the micrometer readings are reduced to millimetres. The diameter thus found can be compared with that found by weighing the mercury, the preference being given to the direct measurement in case there is any slight difference. Ramsay and Shields (*J.C.S.*, 1893, T. 1089) then proceeded as follows: A short tube of thin-walled glass containing a spiral of iron wire was sealed on to a short piece of glass rod 1 mm. in diameter. The capillary was dipped in ether, inclined so as to fill it with the liquid, and one end was sealed; the open end was then held so as to touch the glass rod, and a very small pointed flame was directed on to the junction; the glass was then sealed on, and the vapour pressure of the ether blew a small bulb at the bottom of the capillary and a minute hole in the side of the bulb. The sealed end was then cut off, and a thin ring of glass cut off for the determination of the radius. The capillary was then placed in the wider tube and the liquid was added. The wider tube was constricted at the top, and attached to the pump in order to boil out all the air; it was then sealed off at the narrow portion. Thus Ramsay and Shields' determinations were all made with the liquid in contact with its own vapour. Renault and Guye (*J. Chim. Phys.*, 1907, vol. v, p. 81), used the same method, but carried out their determinations in the presence of air as being much simpler—the difference in the values obtained was about 0·5 per cent. In carrying out a determination, a scale was attached to the front of the wider tube and this was placed in a vapour-bath. The capillary could now be lowered by means of the attraction of the electromagnet on the iron wire spiral attached to its lower end. It was lowered so as to allow the liquid to flow over the top, thus renewing the surface before each reading and ensuring the wetting of the glass above the meniscus; the capillary tube was allowed to rise until the meniscus came to a point 2 mm. below the end of the tube, and the height was then read by means of a cathetometer and the scale in front of the glass. As an example

of their results, with carbon disulphide as the liquid and $r = 0{\cdot}0129$ cm., they obtained :

At	t	19·4° C.	at	t	= 46·1° C.
,,	h	4·21 cm.	,,	h	= 3·80 cm.
,,	h	4·20 cm.	,,	h	= 3·795 cm.
,,	h	4·20 cm.	,,	h	= 3·80 cm.
mean	h	4·203 cm.	mean	h	= 3·798 cm.
,,	d	1·264 cm.	,,	d	= 1·223 cm.

$$T = \tfrac{1}{2}\, grdh.$$

$T = 33{\cdot}6$ dynes per sq. cm. $T_2 = 29{\cdot}4$ dynes per sq. cm.

They then used these values to calculate $\dfrac{dT\,(Mv)^{2/3}}{dt,}$

$$T_1\,(M/d_1)^{2/3} = 33{\cdot}6\,(76/1{\cdot}264)^{2/3} = 515 \text{ ergs.}$$

$$T_2\,(M/d_2)^{2/3} = 29{\cdot}4\,(76/1{\cdot}223)^{2/3} = 461 \text{ ergs.}$$

$$\text{then}\,\frac{dt\,(Mv)^{2/3}}{dt} = \frac{515 - 461}{46{\cdot}1 - 19{\cdot}4} = \frac{54}{26{\cdot}7} = 2{\cdot}02.$$

The defect of the apparatus consists in the fact that the outer tube is not sufficiently wide to ensure that there is no capillary rise there, thus making the result too low. More theoretically perfect is the apparatus used by Richards and Coombs. The capillaries were carefully selected according to the method already described : in one form the diameters of the narrow and wide tubes were 0·20198 and 3·8 cm. respectively, and in the second 0·01936 and 3·8 cm. ; the volume of liquid required for the first tube was 36 c.c., but this was reduced to 12 c.c. in another form by means of the sinker. A reference mark was etched near the centre of the capillary. Particular care should be taken, when using apparatus containing stopcocks, to make certain that these are thoroughly clean ; they can only be lubricated with the liquid in the tube, and, so as to prevent the entrance of water, the outside of the tap is covered with a film of paraffin before the apparatus is placed in the bath. The method used to observe the position of the meniscus with accuracy was to set the apparatus vertical with a plumb-line in the thermostat (which had parallel plate-glass sides). A black metal screen having a rectangular hole in it, with the horizontal edge quite straight, was lowered into the thermostat so as to lie just behind the apparatus, and was then adjusted so that the straight edge was just horizontal, and when viewed through the cathetometer, appeared to make a tangent with the lowest point of the meniscus. The upper edge of the meniscus could also be determined when desired by gradually raising the plate, whereby the line of the straight edge at first appears broken,

but when it reaches the top of the meniscus again appears to be straight. Before using the apparatus, it was carefully calibrated with water in the following way: The apparatus was first thoroughly cleaned, and was then filled with water so that it reached a position below the reference mark. The water was then run up and down the capillary several times, and the apparatus was set vertical with the plumb-line and the height read; the tube was again wetted, turned through 180 degrees, and the height again read (always allowing for the liquid to drain and reading with a falling thread); next the tube was inverted and the two readings were repeated. More water was added and the readings were taken, and so on. To show the kind of values obtained, the following figures represent the mean of five readings in each position: 1·4378, 1·4422, 1·4384, 1·4391 cm. The mean of these is 1·4394, with a probable error of 0·03 per cent. By proceeding in this way, it was found, for example, with one tube, that the most constant results were obtained within the region 0·4 to 1·3 mm. of the reference mark, and this part of the capillary was therefore used in the actual determinations. The surface tension of benzene was obtained at 20° C. in both tubes, with the following results: tube 1, $T = 28{\cdot}94$; tube 2, $T = 28{\cdot}88$; and the value for the surface tension of water at the same temperature was found to be 72·62. The results generally are higher than those obtained by other observers using the capillary tube method, as might be expected owing to the use of the wider tubes.

Tate made experiments upon the weight of a drop of water falling from the end of a glass tube. He used tubes from 0·1 to 0·7 inch in diameter, and made of thin glass ground to a sharp edge. He came to the conclusion that the weight of the drop is proportional to (1) the diameter of the tube; (2) the weight of the liquid which would be raised up in the tube owing to the capillary; and he found that the weight decreased with rise of temperature. His first two conclusions may be stated in the form $mg = k.rT$.

Many textbooks and research workers describe methods using capillary tubes without any references to the sharp edges, and using the equation $mg = 2\pi rT$, *i.e.* making Tate's constant k equal to 2π. Poynting and Thomson (*Properties of Matter*) point out that the forces acting on the drop include the excess pressure over the external pressure which is developed in the drop owing to surface tension; if the drop is cylindrical at the top, the value of this excess pressure is T/r, and we then have $2\pi rT = mg + \pi rT$—*i.e.*, $mg = \pi rT$, or exactly half the previous value. Rayleigh finds (*Phil. Mag.*, vol. xlviii, p. 321) that the equation $mg = 3{\cdot}8rT$ is sufficiently accurate for most purposes, while J. L. R. Morgan (see below) finds $mg = 3{\cdot}94rT$. There is

thus some doubt as to the exact value of the constant to be used, but this difficulty is overcome by Morgan by using the falling drop method to give values of the surface tension relative to that of benzene as determined by the capillary rise method. As a general method, Morgan's has the advantage over the capillary rise method that the surface of the drop is a large one, and, moreover, is constantly renewed, so that the effect of small quantities of impurities is much reduced. Morgan's apparatus can be used for all temperatures up to 90° C., and errors due to evaporation are avoided. He has established, by a very careful series of experiments, that for a given dropping tip the ratio of the surface tension to the weight of the falling drop at the same temperature is equal to a constant, *i.e.*,

$$\frac{T_{C_6H_6.t}}{mg_{C_6H_6.t}} \quad \frac{T_{H_2O.t'}}{mg_{H_2O.t'}} \quad \frac{T_{1.t''}}{mg_{1.t''}} = C.$$

wherefore $T_{1.t''} = Cmg_{1.t''}$ for any given tip. Each tip has to be standardised by means of benzene or water using the values

$$T_{C_6H_6.t} = 30{\cdot}514 - 0{\cdot}1321\ t + 0{\cdot}000082\ t^2$$
$$T_{H_2O.t} = 75{\cdot}872 - 0{\cdot}154\ \ t + 0{\cdot}00022\ t^2$$

The equation for benzene is derived from the average of all the results of all accurate workers using the capillary rise method: that for water was obtained by comparison with benzene using the drop method. Another equation which Morgan finds to hold rigidly is

$$T_{C_6H_6.t} = 2{\cdot}115\ \frac{(288{\cdot}5 - t - 6)}{(78/d_t)^{\frac{2}{3}}}$$

where the density of the liquid is given by $d_t = 0{\cdot}9002 - 0{\cdot}001066\,t$. In using the method, there is one condition which is essential, *i.e.* the drop must have a normal outline; this is a bag-like form with the sides parallel for a part of their length. If the tip is too small, the drop bulges at the bottom, so that the maximum diameter of the drop is greater than that of the tip, the results being too high. If the tip is too large, there is a loss of control over the formation of the drop, and the latter converges at the bottom and breaks away at a different point to the "normal" drop, and again gives too high results. It is found that the conditions are satisfactory with tips having diameters from 4·5 mm., giving drops with volumes greater than 0·0196 c.c. to 5·5 mm., giving drops with volumes greater than 0·0239 c.c. for all but one out of two hundred liquids studied; the exception is carbon tetrachloride, having $T = 24{\cdot}9$ and $d = 1{\cdot}576$, so that with a 4·5 mm. tip the volume of the drop is

0·0143 c.c. The accuracy of Morgan's method is exemplified by his results with six different liquids: using the equations

$$T_t(M/d)^{\frac{2}{3}} = K(t_c - t - 6)$$

and $$mg_t = T_t/C$$

we get $$mg_t(M/d)^{\frac{2}{3}} = K/C\,(t_c - t - 6)$$

where mg_t is the weight of a drop at the temperature t.
T_t is the surface tension at the same temperature,
M is the molecular weight and d the density of the liquid.
and t_c its critical temperature.

The value of K should be constant in the surface tension equation, and that of K/C in the weight of a drop equation if the same tip be used with all the liquids: for the six liquids Morgan found $K/C = 2{\cdot}5694 \pm 0{\cdot}0013$, *i.e.* 0·05 per cent. variation from the average; while Renault and Guye, using the capillary rise method, found for the same six liquids $K - 2{\cdot}116 \pm 0{\cdot}0965$, *i.e.* a variation of 4·6 per cent. from the mean. The laboratory form of Morgan's apparatus is made entirely of glass, the various parts fitting together by means of ground-glass joints. It consists essentially of two stoppered straight-sided weighing bottles 2 cm. diameter connected by an inverted capillary U-tube, one limb being slightly shorter (1 cm.) than the other. The weighing bottles, by means of their stoppers, fit into a perforated disc, which in its turn fits into a larger weighing bottle, the latter serving as an air-bath. The two weighing bottles and the outer air-bath are all supplied with a ventilation tube, and a thermometer is attached having its bulb close to the U-tube. The inverted tube is the "capillary" drop tube, and has the shorter end ground accurately cylindrical for a length of 1 cm. to take the tip: the bore of this tube is 0·2 mm. Tips of various sizes can be fitted; the bottom of the tips is made plane and perpendicular to the sides, a clean sharp edge being very important. When the apparatus is fitted with a fresh tip, great care should be taken to get the bottom of the tip exactly parallel to the cover of the apparatus, so that the tip can afterwards be set exactly level by levelling the cover. Before using, the apparatus is very thoroughly cleaned with the usual liquids. A rubber bulb is connected to the ventilation tube, which fits into the weighing bottle on the tip side; the compression of the bulb can be accurately controlled by means of a screw. The empty weighing bottle on the tip side is put on first, and then that containing the liquid is fitted in position on the supply side, and the outer vessel is attached: the whole apparatus is placed in the thermostat and allowed to remain for twenty to forty minutes to attain thermal equilibrium, the capillary being

kept free from liquid the whole time by means of the compression bulb. The tip is then carefully levelled by getting the cover of the apparatus level (parallelism between the two having been carefully attended to in the original assembling of the parts). The liquid is now sucked slowly through the capillary, and a drop of nearly the maximum size is allowed to form; this drop is left hanging for five minutes, so that any evaporation effects which are going to occur may be completed: the drop is then allowed to fall and twenty-nine other drops are collected, each being allowed about one minute to form, and its shape being observed during its formation by means of a lens to ensure that the drops are "normal," great care being taken that the drops fall of their own weight, by making the rate of formation very slow towards the end of the formation of each drop. The liquid in the capillary is then forced back, and the weighing bottle on the supply side is removed; any liquid adhering to the capillary is taken up with filter-paper. The weighing bottle on the tip side is dipped in cold water to condense any vapour, and the outside having been dried, the bottle is disconnected, stoppered, and weighed. The weight so obtained is that of a bottle together with thirty drops of liquid and a certain amount of vapour. The weighing bottle on the tip side is dried, and the parts of the apparatus are assembled in the same order as before and the whole is placed in the thermostat and left for the same time as in the first part of the experiment; a single drop is formed and allowed to hang for five minutes; five drops are then allowed to fall, and the sixth is kept hanging at the end of the tip until the total time is the same as in the first experiment; the sixth drop is then forced back into the supply vessel, and the weighing bottle containing the five drops is removed with the same precautions as at first and is weighed. This gives the weight of the bottle together with five drops and a certain amount of vapour which, owing to the conditions of the experiment, cannot be very different from that in the first experiment. Thus, if the second weight is subtracted from the first, the weight of twenty-five drops is obtained. The example now given shows results obtained with benzene, M.W. 78 and t_c 288·5° C.

Temp.	Weight of B + 25 drops.	Weight of B + 5 drops.	Weight of 20 drops.	Weight of 1 drop.	D.	$mg(M/d.)^{\frac{2}{3}}$	K/C.
10° C.	7·6515	7·0063	0·6452	0·03226	0·88956	629·0	2·291
40·7° C.	12·0194	11·4611	0·5583	0·027915	0·85683	553·9	2·291

The mean value of K from surface tension results being 2·115, that of C becomes 2·115/2·291, and since $T = Cmg$, the surface tension can be calculated; in this way we get $T_{10} = 29·20$

and $T_{40 \cdot 7} = 25 \cdot 26$, the values calculated from the formula given on p. 229 being 29·20 and 25·27. Once the drop weight of benzene has been obtained at a given temperature with a tip, the calculation of the surface tension of a second liquid whose drop weight has been determined with the same tip at the same temperature is simple, *e.g.* with a tip 4·51 mm. in diameter, the weight of a drop of benzene was found to be 0·024269 gram, and that of a drop of quinoline 0·038487, both determined at 27·8° C.; since the surface tension of benzene at this temperature is 26·75, the surface tension of quinoline is

$$T_{Q,\,27 \cdot 8} = \frac{0 \cdot 038487 \times 26 \cdot 75}{0 \cdot 024269} = 42 \cdot 4.$$

Morgan's drop weight apparatus is made by Eimer and Amend, of New York.

A method depending on the formation of bubbles of air in the liquid is that described by Jager (*Wein. Akad. Berichte*, 100, 245). The pressure inside a spherical air-bubble in a liquid exceeds that outside by an amount p given by the equation $p = 2T/r$; at constant temperature the product pr is constant. Now, if a glass tube is placed vertically in the liquid and is attached to an air vessel fitted with a manometer, on gradually increasing the pressure, the air will pass down the tube and a bubble will form at the end. If the pressure is then slightly increased, since pr is to remain constant, the radius of curvature of the drop must diminish as the volume of the drop increases; the drop thus continues to grow until it becomes hemispherical. The excess pressure p above that of the atmosphere shown by the manometer is then equal to the hydrostatic pressure plus the pressure due to the curvature of the drop:

$$i.e.,\ p = dgh + 2T/r$$

where d is the density of the liquid.
h is the depth of the end of the tube below the surface.
T is the surface tension.
and r is the radius of the tube.

If the pressure is again slightly raised, since the radius of curvature of the drop cannot be less than the radius of the tube, the radius of the drop must increase as the volume increases and the capillary counterpressure is reduced (pr being fixed); thus a very slight increase in the pressure above the value given in the equation causes the drop to continue to increase in size until it detaches itself. The experiment therefore consists in finding the minimum pressure at which bubbles will continue to form and detach themselves from the tube.

Capillary tubes having very sharp edges are used, and two tubes of different radii are attached to vertical rods so that they can be moved up and down a vertical scale. The two tubes are attached to the same air reservoir, and the heights are adjusted until bubbles slowly detach themselves from both tubes simultaneously; then

$$T = \frac{rr'g}{r' - r} \cdot dh$$

and the actual value of the pressure is not required. Since

$$T'/T'' = d'h'/d''h''$$

the method is very convenient for determining relative surface tensions.

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A SHORT REVIEW OF COLLOID THEORY

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To Thomas Graham is due the credit of first studying, in 1862, the properties of the colloid state. Although he drew a sharp distinction between solutions of colloids and crystalloids, it has, since that time, become increasingly more difficult to do so. Possibly the simplest definition of colloidal solutions, which we can at present give, is that they differ from true solutions in not being homogeneous, and they contain, therefore, more than one phase. This is due to the aggregation of the "dissolved" substance into masses which are large compared with molecular magnitudes. Of the physical properties which indicate such heterogeneity, that which gives rise to what is known as the Tyndall effect is probably most reliable. If a lateral beam of intense light is thrown across any solution, when viewed under a microscope, the presence of particles above a certain size is shown by moving specks of light. These are due to diffraction of the light by the particles which, by transmitted light, would be invisible. With such an arrangement the transition between true and colloidal solutions may readily be seen, as, for example, with cane sugar. Dilute solutions of this substance show no turbidity, but, on gradually increasing the concentration, specks begin to appear, until eventually they fill the field of view.

Graham, as we have said, drew a sharp distinction between colloids and crystalloids, based upon their rate of diffusion in solution through membranes. It now seems certain that such a distinction is purely artificial, and that we ought rather to speak of the colloid state than of colloid substances. This recognition of the universality of the colloid state is one of the great advances of recent years which has made possible the formulation of a general theory of colloids; but such a theory has still to be brought forward. Wo. Ostwald[1] and Von Weimarn,[2] amongst others, have done much to emphasise the fact that we must now admit the possibility of preparing

[1] *Grundriss der Kolloidchemie.*
[2] *Grundzuge der Dispersoidchemie*, 1910.

any substance in the colloid state. This being so, we are justified in comparing it with other states of matter. Traditionally it has been contrasted with the crystalline form, but Von Weimarn [1] has recently shown the probability of some colloidal salts having a crystalline structure. It would, however, be unjustifiable to apply his results generally, even to suspensoids—that is, to those systems in which the disperse phase before dispersion is solid. The question also arises as to what we are to understand by " crystalline," when the surface energy is as large as that certainly present in colloid systems. One of the most essential properties of the latter is, in fact, the relative magnitude of surface energy as compared with internal energy. As an immediate effect of this large surface energy, we might expect adsorption phenomena to play a large part in the chemistry of colloids, and recent work has tended largely to confirm this view. Adsorption effects are, however, modified to a certain extent by other forces acting upon the colloid particle, which we will shortly consider.

Dispersion.—Before proceeding, it is of interest to consider the methods of dispersion, or actual passage from the homogeneous, or coarsely heterogeneous, state of matter to the more finely divided state which exists in all colloid systems. It is convenient to consider the disperse phase in such systems as being potentially a transitional stage between the homogeneous substance and a true solution. Viewed in this light, colloidal solutions might theoretically be prepared in two ways. We might either suspend the disperse phase in a suitable medium, and, by some means, increase the degree of dispersion to the required extent ; or, starting with a true solution, we might bring about aggregation of the molecules, causing it to cease when it had reached a certain stage. The actual methods of dispersion are, however, not so simple as this, and vary in different cases. Some substances are met with ordinarily in the colloid state, as, for example, gelatin and albumin ; but with most substances we have to adopt methods—chemical, electrical, or mechanical—suited to their conversion to this state. In any case, it is necessary not only to produce a system in which dispersion is of a certain order, but also to prevent aggregation of the particles by some means. Our knowledge of this second factor is very far from complete, but stabilisation is usually ascribed to molecular forces at the interface. We can recognise three classes of colloid solutions, in which stability is due to adsorbed liquid, to adsorbed non-electrolyte, or to adsorbed ion.[2] Of these three, more is known of the last case than of the other two.

[1] *Loc. cit.*, and *Zur Lehre der Zuständen der Materie*, 1914.
[2] Bancroft, Second Brit. Ass. Rep. on Coll. Chem., 1919.

The work of Beans and Eastlake,[1] on the preparation of colloidal metals in pure water by sparking, tends to show that they owe their stability to certain ions formed at the same time. The presence of ionic complexes was deduced from the conductivity of the resulting solutions. Beans and Eastlake found, moreover, that stability in such solutions was increased, in some cases, by addition of very small quantities of electrolytes. These results also confirm the current hypothesis that the charge upon the colloid particle is due to adsorption of ions. Hardy,[2] as long ago as 1899, had shown that albumin in acid solution was positively, and in alkaline solution negatively, charged. Recently, Powis [3] has demonstrated that ferric hydroxide, which is usually obtained positively charged (as, for instance, by the dialysis of ferric chloride solution in the presence of positively charged ferric ions), may be obtained negatively charged by running ferric chloride solution into an alkaline hydroxide solution. It seems possible that this may be due in part, if not entirely, to the great mobility of the $H^{\cdot}$ and OH' ions, which would lead to more numerous encounters between the colloid particles and these ions than any others.

Brownian Movement.—The kinetic forces of the solvent molecules, the effects of which are visible in the Brownian movement, seem largely responsible for maintaining the dispersoid particles in suspension. Perrin's estimation of the Avrogadro constant makes use of the Brownian movement in suspensions of gamboge and mastic. By extending the principle of the equipartition of energy, deduced from the gaseous state, to solutions, and finally to colloidal solutions, he concludes that[4] "we are led to render the theory precise by saying, not only that each particle owes its movement to the impacts of the molecules of the liquid, but, further, that the energy maintained by the impacts is on the average equal to that of any one of the molecules. . . ." Bancroft[5] considers that, when the size of the colloid particles is such that they are sensibly affected, this energy tends to cause stability. "Any substance may be brought into colloidal solution, provided we make the particles of that phase so small that the Brownian movement will keep the particles suspended, and provided that we prevent coagulation by a suitable surface film." This is also another argument in favour of the universality of the colloid state. There seems to be an opinion

[1] *Journ. Amer. Chem. Soc.*, vol. xxxvii, p. 2667, 1915.
[2] *Journ. Physiol.*, vol. xxiv, p. 288, 1899.
[3] *Journ. Chem. Soc.*, vol. cvii, p. 818, 1915.
[4] *Brownian Movement and Molecular Reality*, trans. Soddy, 1910.
[5] *Journ. Amer. Electrochem. Soc.*, vol. xxvii, p. 175, 1915.

that the Brownian movement ceases immediately before coagulation. The Svedberg [1] has shown that the Brownian movement is unaffected by the presence of electrolytes in solution, and there seems no reason why it should so stop. We have, however, to distinguish between absolute cessation and the rapid falling off of the movement owing to increasing size of the particles, so soon as coagulation has started. It has been stated that the Brownian movement is the manifestation of forces tending to keep the colloid particles in suspension, but it may be pointed out that they may also assist coagulation, other conditions being suitable, by bringing the particles into close contact with one another.

Electrostatic Forces.—In the case of charged particles, electrostatic forces will tend to keep them apart, thus aiding the kinetic forces of the solvent molecules. We have seen that the charge is probably due to specific adsorption of ions. A charge on a particle will set up a definite potential difference at its surface, and, assuming the existence of a Helmholtz double layer, we may calculate this in terms of the charge and the size of the particle. If we consider the charge as extending to a depth d on the surface of a sphere, where d is small compared with r, the radius of the sphere, we may write for the capacity of the particle,

$$C = \frac{s}{4\pi d} K$$

Where s is the surface area of the particle, and K is the di-electric constant of the medium between the layers. This may possibly be regarded as that of the disperson medium itself.

Then, if the potential difference be V volts, we can write,

$$\begin{aligned} e &= CV \\ &= V \frac{s}{4\pi d} K \\ &= V \frac{r^2}{d} K \end{aligned}$$

Burton [2] has used this formula to calculate V by combining it with the expression deduced by Lamb [3] for the velocity v of a charged particle through a liquid under an electric force,

$$Xe = 4\pi r^2 \cdot \eta \cdot \frac{v}{l}$$

[1] *Die Existenz der Molekule*, 1915.
[2] *Phil. Mag.*, vol. xi, p. 425, 1906.
[3] *Ibid.*, p. 60, 1888.

where X is the gradient of electric potential in liquid.

l is a linear magnitude measuring the "facility of slipping" of the particle against the molecules of the medium.

η is the coefficient of viscosity of the liquid.

e, r, and d as above.

Whence we may write,

$$V\left\{\frac{l}{d}\right\} = \frac{4\pi}{K}.\frac{\eta v}{X}$$

Lamb gives reasons for considering that l and d are of the same order of magnitude, and they do not probably differ by much. Burton found that the numerical value of V was of the same order of magnitude for each substance investigated, and in different media. For platinum in water he found

$$V = -0{\cdot}031 \text{ volt.}$$

It was generally assumed that, the colloid particles being charged either by friction or adsorption, the opposite charge was borne by the molecules of the solvent. Recently, McBain, Laing, and Titley,[1] working on colloidal soap solutions, have attempted to apply an ionic theory to all charged colloids, which seems to have certain experimental grounds for its adoption. "The current assumption that the other charge is carried by the solvent is replaced by the hypothesis that free ions of charge equal and opposite to that of the charged colloid are present in sol or gel." In this connection Wilson's electrical theory of the colloid state is of interest. Wilson,[2] working on the assumption that "the colloidal state in sols owes its stability to the formation of a complex between the particles of the disperse phase and certain substances present, or formed in the medium during preparation of the colloid,"[3] has derived a relation between the potential difference and the difference in concentration between the ions at the surface layer and in the bulk of the solution. He first deduces, by thermodynamical methods, that "the products of concentration of any pair of diffusible and oppositely charged ions will be equal in surface layer and bulk of solution." He adopts the following notation. Suppose the colloid negatively charged.

[1] *Journ. Chem. Soc.*, vol. cxv, p. 1279, 1919.
[2] *Journ. Amer. Chem. Soc.*, vol. xxxviii, p. 1982, 1916.
[3] *Cf.* above, and Beans and Eastlake, *loc. cit.*

In bulk of solutions, let
x = concentration of positive or negative ions.
In surface layer, let
y = concentration of negatively charged diffusible ions.
z = concentration of positively charged ions bound by electrochemical attractions.
$\therefore\ y + z$ = concentration of positively charged ions.
From the relation just derived,

$$x^2 = y(y + z).$$

The formula given by Donnan [1] for the potential difference due to inequality in concentration of ions in surface layer and bulk of solution is

$$V = \frac{RT}{F} log \frac{1}{\lambda}$$

But in this case $\lambda = \dfrac{y}{x} = \dfrac{-z + \sqrt{(4x^2 + z^2)}}{2x}$

We may therefore write $V = \dfrac{RT}{F} log \dfrac{2x}{-z + \sqrt{(4x^2 + z^2)}}$

where z is either constant or has a limiting maximum value.

$$\text{Hence } \lim_{x = \infty}, \ V = \frac{RT}{F} log \frac{2x}{\sqrt{(4x^2)}} = 0,$$

proving that the potential difference will diminish as the concentration of electrolytes is increased, a result of considerable theoretical importance.

Surface Energies—A. Positive Surface Energy.—Surface tension is due to intermolecular forces within the colloid particle. These are, however, dependent to a certain extent upon the solvent with which the particle is in contact. In support of this, it may be stated that the solution obtained by dissolving soap in water is colloidal, while under similar conditions it forms a true solution in alcohol. Bredig has observed that the surface tension of mercury in contact with dilute solutions of its own salts is lessened by a potential difference across the interface. It is a generally accepted statement that the existence of a potential difference lowers the surface tension of the disperse phase, and experience seems to show that this is true. Surface tension may also be lowered in the presence of certain substances in the dispersion medium, but we will return to this when dealing with coagulation. Donnan [2] has developed an important theory of colloid state founded upon

[1] *Zeit. Elektrochem.*, vol. xvii, p. 572, 1911.
[2] *Phil. Mag.*, vol. i, p. 647, 1901.

capillary effects alone, the basis of his theory being that the range of molecular attraction varies with different substances. It would, however, seem impossible to formulate any theory of colloid state of general application which neglected electrical phenomena. As a result of his theory, particles of the same colloid appear to be acted upon by two effective forces—positive and negative surface energy—tending respectively to increase and decrease the surface area. The particles would then have, within limits, a critical size.

B. Negative Surface Energy.—The idea of an effective negative surface tension has recently been developed by Wo. Ostwald.[1] Reasoning from analogy to liquids and gases, he would suspect a form of surface energy to exist in dispersoid systems, of which the intensity factor might be called expansive or negative surface tension. The increases in surface which occur in strictly diphasic systems can only be explained by assuming the existence of such a surface energy. Such increases in surface have been observed by Traube, Mengarini, and Scala,[2] and Amann,[3] when apparently no electric energy is available. It can be shown that negative surface energy should increase with the existence of a potential difference at the surface. In the homogeneous state of matter, the increase of positive surface energy is equivalent to the decrease of expansive surface energy. Ostwald considers that excessive development of absolute surface is accompanied by liberation of positive surface energy, and this characterises the act of dispersion. This view is by no means new—we have seen that Donnan recognised an effective negative surface tension—but Ostwald has laid upon it considerable stress.

Osmotic Forces.—Osmotic forces are due to a difference in concentration of the ions at the interface of two phases, and in the bulk of solution. Little experimental work is possible on this subject, for we are unable to examine directly the constitution of the dispersoid phase. Work on the swelling of gels would seem to show that the dispersoid was permeable by the solvent molecules, but generally, when we consider the magnitude of the surface tension, this seems improbable to any extent. The subject of osmotic forces and capillary chemical effects in general may best be approached by a study of Gibbs' adsorption equation[4]:

$$\frac{\delta\sigma}{\delta v} = -\frac{\delta P}{\delta S}$$

That is, if the surface tension σ at an interface alters with

[1] *Loc. cit.*
[2] *Koll. Zeit.*, vol. vi, p. 65, 1910.
[3] *Ibid.*, vol. vi, p. 235, 1910.
[4] *Scientific Papers*, vol. ii.

the volume of solution, *i.e.* with the concentration of the solute, then the osmotic pressure alters with the surface area of the interface. This can only be so if the concentration of the solute in the bulk of the solution depends upon the surface area—*i.e.*, if the concentration in the surface area is greater or less than in the bulk of the solution. It can also be shown that, if the difference at the surface layer of the solute per unit area be Γ, then,

$$\Gamma = -\frac{c}{RT} \cdot \frac{\delta\sigma}{\delta c}$$

where c is the concentration of the solution. That is, if $\frac{\delta\sigma}{\delta c}$ is negative (the substance lowers the interfacial tension), then Γ is positive, and positive adsorption occurs. This equation has been verified experimentally by Lewis [1] with fairly good results. We have already seen the expression given by Wilson for the potential difference which results from adsorption in the case of ions.

Coagulation.—The greater part of our knowledge of dispersoids has been derived from a study of their coagulation, though the exact method, and also the rate of coagulation, seem to have been neglected. Linder and Picton have observed microscopically the coagulation of suspensions to be an aggregation of the particles, and the gradual growth in size of the masses so formed. This would result in the increase of internal energy at the expense of surface energy, and at the same time the Brownian movement would cease. Attractive forces between two colloid particles seem to fall off more rapidly than repulsive forces, since close contact of the two particles appears necessary for coagulation. We have seen that the repulsive force should vary directly with some function of the charge on the particles. Hence neutralisation of this charge will permit of closer contact. At the same time we have also seen that a decrease of potential difference, due to the charge on a particle, leads to an increase in the surface tension. This would favour coagulation, by which the absolute surface would be reduced. We may, therefore, expect that the neutralisation of the charge upon the dispersoid phase would be one effective cause of coagulation. Neutralisation may most readily be brought about by adsorption of ions carrying a charge of opposite sign. That this causes coagulation is a well-known experimental observation. An interesting fact, due to the work of Powis [2] on hydrocarbon oil emulsions, has recently been brought to light. Powis showed that

[1] *Phil. Mag.*, vol. xv, p. 499, 1908; vol. xvii, p. 466, 1909.
[2] *Zeit. Phys. Chem.*, vol. lxxxix, pp. 91, 179, 186, 1914.

coagulation does not take place at the iso-electric point, when the potential difference between particle and medium is nil, as might have been expected, but there is a critical value of potential difference, ±0·03 volt, above which the emulsion is stable. He has more recently shown [1] that this is also true of colloidal As_2S_3. If these results are of general application, it is an observation of considerable importance.

It has been found that the adsorption of oppositely charged ions is specific, both for the ions and different colloids. The present writer has, however, obtained an indication that for the same colloid the coagulative power of the ions is proportional to their atomic number, but this observation requires verification. It is possible that the kinetic energy of the ion plays a part in coagulation.

A close contact between particles of the dispersoid phase being effected, a large positive surface tension will cause coagulation. It has, however, been noticed that coagulation may be brought about in some cases by substances which lower the interfacial tension. This would apparently tend to the production of an opposite effect, *i.e.* passage of the dispersoid into the molecular disperse state. This action has not been discussed, and its meaning at present remains obscure. In connection with the type of adsorption effected by coagulative ions, it has been suggested that a loose chemical union takes place. This hypothesis is based on the fact that the coagulating electrolyte is very difficult to wash out of the coagulated substance, and it has some experimental grounds for belief.

Conclusion.—The chemistry of colloids is a comparatively new branch of science, but, owing to its importance, a considerable amount of work has been done upon it. In an article of this length, it is impossible to touch upon all its points of attack—the phenomena of peptisation, of viscosity changes, and so on—but it is hoped that the more important theoretical considerations have been presented as fully as space would permit. As has been mentioned, the phenomena of coagulation have provided us with our deepest insight into dispersoid systems, and it is from this direction that our future knowledge will probably come.

[1] *Journ. Chem. Soc.*, vol. cix, 734, 1916.

THE THYROID GLAND

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The thyroid is one of the few secreting glands which are devoid of ducts, and which are consequently unable to pour their secretion on the surface in the manner of such glands as the salivary. Glands of this type are known as " internally secreting " (Claude Bernard) or " endocrine " (Schäfer), since the secretion elaborated is passed into interior channels or blood-vessels.

Structure.—Arising in the first instance from a pharyngeal outgrowth, the thyroid ultimately loses its original connection and develops into a gland composed of closed rounded vesicles. The vesicles, however (and the cells which line them), vary in shape and size according to functional conditions. They almost always contain a viscid fluid (colloid) which is the product of their secretion. This, as explained above, is absorbed into the numerous blood-vessels supplied to the gland. In addition to blood-vessels, the thyroid receives branches from the autonomic (sympathetic and vagus) nerves. The structure varies slightly in different animals, and accessory thyroids are not uncommon.

Lying behind each lobe of the thyroid, and even embedded within its substance, are two or more small bodies, composed of a compact mass of cells, known as the parathyroids. These were first described by Sandström (1880), although the internal (embedded) parathyroids were unknown until they were recognised by Kohn in 1895.

Early History.—The function of the thyroid remained obscure from the time of Galen (A.D. 131–201) down to about a hundred years ago. Galen taught that its secretion lubricated the larynx! His teaching was perpetuated by his successors until the eighteenth century, when Haller (1776), recognising the ductless nature of the gland, reasoned (after Ruysch) that its secretion was carried away by the venous blood. Simon (1884) made no advance on Haller when he suggested that, as the thyroid was extremely vascular, it served as a reservoir to regulate the flow of blood in the cranial cavity.

In 1848, Claude Bernard demonstrated the possibilities of internal secretion in the case of the liver, but he applied the term "internal secretion" in a wider sense than it is held to-day. The modern conception owes its origin to Brown Séquard (1891), who limited the term to the secretion of specific substances whose influence was necessary to the well-being of the organism. Although nothing new was learnt regarding the thyroid, this doctrine inspired and guided the experiments which were subsequently undertaken to elucidate its function. The earlier experiments, however, were largely prompted by observations on individuals suffering from disease of the gland.

The first serious attempt at experimentation was undertaken by Schiff (1856). He removed the thyroids of various animals, but, as his observations were incomplete, little attention was paid to them. At a later date (1884) he was encouraged by the work of the Reverdins (1883), on the removal of goitres in human beings, to renew his former experiments on a larger scale. He found that dogs and cats died shortly after the operation, but that rabbits and rats survived. These results drew the attention of physiologists to the importance of the thyroid, and stimulated research in this direction.

Since 1884, not only have numerous investigators confirmed and added to Schiff's work, but a vast literature, requiring an encyclopædic compilation, has come into being. A brief sketch of the more important results is all that may be attempted here, and for clearness the subject will be described under the headings given below.

The Effects of Diminished Secretion: Hypothyroidism.—This condition may be brought about either by disease of the gland or by operative interference.

In the condition in children known as Cretinism, the gland is atrophied or perhaps has never properly developed since birth. Symptoms do not appear until the child is weaned, presumably because sufficient thyroid material can be obtained from the mother in suckling. The signs appear slowly, for they are not new phenomena, but the non-appearance of changes which one expects to find in a growing child. Years may pass, and the cretin—for such it is by this time—has still the appearance and bearing of an infant. It is short in stature and stodgy, with podgy hands and feet and a pendulous belly. Ossification remains incomplete, and the fontanelles persist. The skin (likewise mucous membranes) is dry and almost hairless, especially the scalp, the muscles are flabby, and the external genitals are undeveloped. Want of intelligence portrays the non-development of the nervous system, the cretin often being a deaf-mute, imbecile, or idiot. The condition is, in short, one of complete arrested development, and almost the same

picture is presented at whatever age the cretin may live to be. On feeding with thyroid substance, development may be induced to proceed normally, and the quiescence of months or even years rectified in a short space of time. The treatment, however, must be continued throughout life. Some cretins are known to exhibit nervous symptoms collectively described as " tetany." These cases McGarrison (1908) attributes to involvement of the parathyroids. He showed that cretinism may result from deficient thyroid secretion in the mother, the maternal condition being produced by the action of bacterial toxins absorbed from the intestinal tract. He traced the infection to contaminated water-supplies, and in doing so, explained the endemic nature of cretinism.

In the adult, toxins acting on the thyroid may also give rise to hypothyroidism, or the causal condition may be a tumour. The symptoms are not unlike those seen in cretinism, but differ in that they supervene after the body is fully developed. Thus the integument is thickened and pseudo-œdematous, especially that covering the face, hands and feet. Increased deposition of fat adds to the appearance of thickening and stolidity. The surface is dry, and becomes rapidly denuded of hair, baldness being usual, while perspiration is much diminished. The body temperature is lowered on account of diminished metabolic activity, less oxygen being consumed and less nitrogen broken down and excreted. Sugar, instead of being utilised, is largely stored as fat, and greater quantities can be taken without causing glycosuria. Sexual functions are in abeyance. Mentally, similar changes are present. The sufferer is apathetic, and less able to exercise her or his senses. In brief, hypothyroidism in the adult leads to a lowering of all the body functions, with its attendant sequelæ.

This condition was first described by Gull (1873) as a " Cretinoid State supervening in Women." Some years later Ord (1877) labelled it Myxœdema, and correlated it with the thyroid; and in 1886, Hadden pointed out the atrophied state of the gland. As in cretinism, the symptoms vary in severity according to the degree of hypothyroidism present. This in turn largely depends upon the condition of the gland. In many cases there is a complete absence of thyroid tissue, which would indicate an entire absence of secretion (athyroidism).

The treatment of hypothyroidism with thyroid juice was first attempted by Murray (1891) in a case of myxœdema, and with what success may be judged from the notes of the case, which he has recently published. The patient recovered and continued in good health for twenty-eight years, during which

time she was regularly treated with thyroid. She died eventually from heart failure. Murray's results have been amply confirmed.

Complete removal of the thyroid for tumours was performed by Kocher and the Reverdins (1883) with unhappy results, for the patients ultimately developed myxœdema, treatment by thyroid medication being then unknown. Kocher found, however, that, if a portion of the gland was left, symptoms were obviated.

There can be no doubt that cretinism and myxœdema are the results of diminished thyroid secretion, for both conditions are alleviated by thyroid feeding, the differences between the two depending entirely upon the age of onset.

Let us now turn to the results of removal (thyroidectomy) in animals. Schiff's important work in 1884 has already been cited. His results are now of little value as records of thyroidectomy, for he unwittingly removed the parathyroids in the course of his operations. This mistake was repeated by those who confirmed his results, and was not rectified until Gley had preached the separate function of the parathyroids and Kohn had described the internal glandules. The fatality and nervous symptoms recorded were largely attributable to this error. Schiff, however, showed that some animals (rabbits and rats) do not suffer from thyroid deprivation. According to Gley, the immunity was only apparent, as in these animals the accessory glandules had not been removed. Schiff was also the first to point out that symptoms could be allayed by grafting a portion of the thyroid into the abdominal cavity.

A host of workers subsequently carried out thyroidectomies —and all with such results as Schiff's. Horsley (1884), who was the first to investigate the effect on the monkey, described typical myxœdema, accompanied by nervous symptoms referable to parathyroid deprivation. Additional species were discovered to be insusceptible—birds by Allara (1885), and herbivora by Sanguirica and Orrechia (1887). According to Briesacher (1890), the immunity was merely a question of diet, for all the immune animals were vegetarians.

Later and more careful work by Gley, Horsley, Vincent and Jolly, and others showed that the general effect of thyroidectomy alone, in the young, was arrested development, in the adult, a lowered state of function, with or without a myxœdematous condition of the skin. Further, the older the animals, the less were they affected. The following symptoms were observed: Thickening and dryness of the integument, loss of hair, wasting followed by adiposity, general weakness, lowered temperature, sexlessness, nervous depression resulting in dullness and apathy, and lowered metabolism, including

a high sugar tolerance. The condition was essentially chronic, and death usually supervened as the result of intercurrent infection. Exceptions to the above may be found in the records of most observers, *e.g.* cats and dogs have occasionally showed no discomfort after thyroidectomy, or merely transient symptoms. Nevertheless, despite the discrepancies which have been recorded, there is no doubt that the general effects of thyroid deprivation, whether produced by disease or excision, are strikingly similar, and may be observed in all mammals with few exceptions. These exceptions may be explained as the result of anatomical peculiarities such as the presence of remote accessory thyroids (Gley) or the vicarious functioning of other glands, *e.g.* parathyroids (Vincent and Jolly) or the pituitary [Rogowitsch (1886) found this gland to be hypertrophied after thyroidectomy].

The Effects of Increased Secretion : Hyperthyroidism.—Here, as in hypothyroidism, the effects may be produced either experimentally or as the result of disease. Hyperthyroidism may be artificially induced by injecting a liquid extract of the gland substance or by feeding with the gland itself. The former method was first employed by Oliver and Schäfer (1895). They showed that the only immediate result of injecting was a temporary lowering of blood-pressure due to dilatation of the blood-vessels. Whether this is a specific effect is doubtful, since a similar result may be obtained with most tissue extracts. On prolonged administration, a characteristic train of symptoms develops. The skin becomes lax, hot and moist from increased perspiration. Weight is lost, and fat disappears from regions where it is normally abundantly present, e.g. subcutaneous tissues, bone-marrow, etc. [Leichtenstern (1893)]. Metabolism is accelerated, and there is an increased consumption of food [Ord, White, Mendel (1893)], of oxygen [Magnus-Levy (1895)], and hence a greater excretion of nitrogen and carbon dioxide. The liver is emptied of its glycogen [Krause and Crammer (1913)], and the pancreas (rat) exhibits extensive mitoses within a week of feeding [Kojima (1917)]. Sugar tolerance is lowered and glycosuria readily provoked. The heart hypertrophies [Herring (1916)] and beats at a faster rate (tachycardia), and the blood-pressure is persistently raised. Nervous activity is also enhanced, tremors, mental excitement, protrusion of the eyeballs (or exophthalmos), and dilatation of the pupils being commonly observed. Some of the above symptoms, *e.g.* those pertaining to the heart, blood-pressure, and eye, are the same as those produced by stimulation of the sympathetic, and are probably effected by the internal secretion of the suprarenal gland (adrenalin). Evidence of an increased secretion of adrenalin is supplied by Fraenkel

(1909), and more recently by E. R. Hoskins and by Herring (1916).

As might be expected, the symptoms of hyperthyroidism are the opposite of those of hypothyroidism, and result from increased functional activity of the tissues generally.

The same changes occur in man in the disease known as Exophthalmic Goitre, first described by Parry (1825), and later by Graves (1835) and Basedow (1840). Tremors, exophthalmos, tachycardia, high blood-pressure, a high metabolic rate, and nervousness are typically present, and, in addition, there is usually a marked swelling of the thyroid gland in the neck. The similarity with the symptoms produced by excessive thyroid feeding is too close to avoid the conclusion that the mechanism is the same in both cases. Moreover, the enlargement of the gland and the histological picture of hypersecretion adds to the probability, even although no definite proof of an increase of thyroid secretion in the circulating blood is available.

Lastly, it is of interest that both myxœdema and exophthalmic goitre occur more commonly in women, in whom the thyroid normally enlarges during menstruation and especially during pregnancy.

The Function of the Thyroid in the Lower Vertebrates.—Most work has been done on amphibians, and according to Gudernatsch (1912), who initiated these experiments, feeding tadpoles with thyroid caused them " to metamorphose . . . weeks before the control animals did so." Miniature frogs, with well-formed limbs and abdominal and other organs, could be produced in less than three weeks in this way. Thyroidectomy, on the contrary, arrests differentiation [Hoskins and Morris (1916)], the tadpoles remaining larval although the gonads continue to develop [Allen (1917)]. The above results are strictly comparable with those obtained in mammals, and have been repeatedly confirmed. Gudernatsch's discovery is of importance in that it furnishes a reliable method of detecting and estimating thyroid.

The Nature of the Thyroid Secretion.—Although the actual secretion of the thyroid has never been obtained, an extract of the gland is believed to contain it. At any rate, Baumann (1875) was able to prepare an organic compound of iodine from the gland, which acts in the same way as the extracts. Iodine is present in nearly all normal thyroids, and it was shown by Reid Hunt (1904) that the activity of the gland varies directly with its iodine content. Lenhart (1915), employing tadpoles, arrives at the same conclusion. The iodine, however, may be combined either in an active or an inactive form [Kendal (1916), Marine and Rogoff (1916)], hence the iodine content, as

such, is not the true criterion of activity. The active combination (Thyroxin, or $C_{11}H_{10}O_3NI_3$) has recently been isolated by Kendall (1918), who states that it has all the physiological properties of thyroid itself.

The Parathyroids.—It has already been noted that the earliest thyroidectomies invariably included the parathyroids, and hence the results were always accompanied by nervous manifestations (tetany). Further, these cases were rapidly fatal, but, as with the thyroid, exceptions occurred. There has been much speculation as to the function of these small glandules, some holding that the thyroid and parathyroids are mutually antagonistic (Rudinger), others that they are mutually dependent (Vincent). Macallum and Voegtlin (1909) believe that the latter regulates calcium metabolism, for the administration of calcium salts relieves tetany. There seems little doubt, however, that the parathyroid secretion controls the detoxication of guanidine, which is normally produced by the muscles of the body. In parathyroidectomised animals, there is an increase of guanidine in the blood, and after injection of guanidine into a normal animal, the symptom complex of tetany is produced [Noël Paton (1916)]. An increase of guanidine is also found in the blood of children suffering from tetany and rickets.

Conclusions.—The function of the thyroid varies according to age. In the young it hastens development, in the adult it maintains the working of the body processes at a normal rate, and in the aged it probably ceases to function. The active principle of the thyroid is an organic compound of iodine whose constitution may perhaps be represented by the formula $C_{11}H_{10}O_3NI_3$.

POPULAR SCIENCE

THE CYCLES AND SUPER-CYCLES OF NATURE

By W. E. REYNOLDS, M.D.

THE philosophy of the last century regarded Evolution chiefly as relative to Matter. The two Primaries, Force and Matter, though recognised as two different entities, were found to be so associated with each other that Evolution was mostly referred to the more concrete entity, and the endeavours of scientific inquiry were based upon this belief. This was the unavoidable result; for the material is the only aspect that is directly apparent to our senses. The part played by Force, the more important entity, was studied, but its Evolution—or "Assortment," as we shall call the change here to obviate confusion—was only suspected and almost overlooked. Thus it is that Herbert Spencer, in his *Synthetical Philosophy*, tells us that, along with the redistribution of matter composing any material aggregate undergoing evolution or change to complication, there goes on as well a redistribution of the retained motion of its components in relation to one another; and *this redistribution of motion also becomes, step by step, more definitely heterogeneous.* This is the summary of the cardinal principles of his doctrine as Spencer himself prepared in his letter to Prof. Youmans.[1] There was here an attempted segregation of the two conceptions, but it was not seen then that security was only to be found in the more abstract foundation of the two, nor does it seem fully appreciated now that all evolutionary changes are due to this assortment of Force, that the complicating process is directly one in Force, but only referable by our senses to the less abstract entity.

In Spencer's statement, however, is to be found the beginning of the development of Thought that is bringing a truer knowledge of Nature, though in this beginning we naturally find little more than a subconscious recognition of important facts which need amplification.

It has long been known that the impact of solidity—say of a

[1] *Athenæum*, July 22, 1882.

block of ice—is produced by certain assorted forces inherent in the matter; that the other physical manifestations, known as weight, colour, etc., are also caused by inherent forces; that the other properties of this substance in the liquid state are due to certain liberated forms of Energy which now manifest themselves in the "work" characteristic of this state; that the change from the liquid to the gaseous forms of matter, in turn, is due to a further liberation of energy; and that the change from the molecular form to the simpler atomic one is conditioned by a further liberation of energy, such as we bring about by electrolyses. These deductions are only such as concern the physical change in the same atomic structures, but our knowledge of Force has been further enlarged by the study of radio-activity, for here we become acquainted with a change that reveals the construction of the atom itself, now actually resolvable, in part at least into electronic activity, theoretically into nothing but electronic activity. Further, here the disintegration of the radio-active atom has been found to be no pure disintegration, but one occurring *pari passu* with the necessary accompaniment of a change from an unstable substance to a more stable one —no true retrogression in the complexity of the original substance, for the uranium atom or the thorium atom, losing its electrones, becomes also transmuted through a gradual series of substances until some final products, in each case, of greater stability and of high atomic complexity or heterogeny as well, are produced, *i.e.* the change from heterogeny to homogeneity represents only the cycle taken by the emanation and the resultant simplex of the helium atom; the necessary accompaniment—the change from uranium to probably lead, from thorium to probably bismuth—being still one retaining its heterogeny, a change from one complex into another complex and no true simplification. It is a natural truth that simplification does not occur here, without complication there.

Spencer assumed that, with the complication or Evolution of Matter, matter became more integrated, and in this integration stored up a greater quantity of Force than it dissipated; but when Matter underwent simplification or dissolution, it became disintegrated, and in this disintegration it dissipated more Force than it stored in itself. He further assumed that these two changes were going on simultaneously and incessantly in Nature, and that the alternating disequilibrium between the two changes was rhythmical.

This is the truth, but only the partial truth, for as a result of these universal rhythms—to which we are now attributing even all chemical change [1]—there are other concurrent cycles produced in the storing up of Force, represented, only in a simpler form,

[1] Cf. Science Progress, January 1920, p. 376.

by the dual transmutation in radio-activity, and we see now how, in this rhythmical action, Force itself is becoming more and more complicated or "assorted" by the formation of more stable association. The "Ultimate Equilibration" formulated by Spencer, the final rest in Matter, is thus an impossibility, or at least further postponed by a factor unknown in his day, for this dual action must necessarily be regarded as leaving a simplex capable of continuing the complication, not as leaving a complex capable of continuing the simplification.

The rhythms of Nature are manifested in all changes. They have recently been referred to,[1] and considered to be the direct result of Cosmic influences, showing themselves, as well in animal and vegetable cells, as if the protoplasm itself had, as it were, absorbed and retained these rhythmical properties from Cosmic sources. This "assimilation" is not confined to protoplasm alone, but is being gradually discovered to be a property of all Matter. The fundamental rhythms obviously serve the universal purpose of storing up more and more Force in an assorted form within the unit with each cyclic change undergone by the unit. It is in such repetition of these actions that the increasing quantities of Force absorbed and complicated has resolved itself into atomic energy within an atom so created, and that the molecule has been produced by the further assortment of this atomic energy into molecular energy. Similarly, colloidal, enzymic, biotic, and psychic energies can be considered the successive steps in this assortment of Force. This "Evolution of Matter" can only be accounted for and explained by assuming that the assorted rhythms of Force were capable of creating, by interaction among themselves, other rhythms as the result of assorted Forces; that the interaction of these newer rhythms were capable of producing still other rhythms or vibrations, and so on until a state of high complexity is attained. This is what Spencer was conscious of when he says that Force becomes step by step more heterogeneous.

It is the development of this conception that concerns us here, for in the light of modern knowledge we are in a better position to elaborate this idea than Spencer; we are beginning to realise that the basis of Matter itself is the expression of the assortment of Forces. We will apply ourselves to endeavour to show that the formation of these ultra-rhythms is as much a reality as any change we see in the laboratory. A digression into elementary physics is here necessary, for the interaction is more accessible to our understanding in the more primitive and simpler forms of rhythmic motion.

Let us consider the periodic rhythm responsible for sound.

If two simultaneous note-vibrations of the same frequency

[1] Science Progress, January 1920, p. 418.

meet, there is no interference between the two notes, no additional vibration; but if their frequencies are different—say one possessing A vibrations per second, the other A + 2 vibrations—it is obvious that in half a second the second note makes one vibration more than the first; each would therefore be in exactly the same phase of the wave in each half-second. A particle under the influence of the two vibrations will therefore, once every half-second, be subjected to a maximum one, due to the combined waves; it is obvious that this maximum displacement represents an additional vibration or rhythm of two per second. The number of these "beats" or additional vibrations per second corresponds to the numerical difference in the frequencies of the two notes, which as far as we know remain themselves unaltered. Such interference between two waves, if insufficient to produce destruction of the fundamental notes, will unavoidably produce reinforcement: this is what we mean by an assortment of Forces in its simplest aspect. Another example in sound vibration of a superadded rhythm is the overtone, which has probably an origin in analogous interference.

The interaction between wave lengths of all sorts is ubiquitous in nature, and obeys the same laws of interference. In a suitable medium, when the rays of white light pass through a prism, dispersion reveals vibrations of other wave lengths in the colours of the spectrum; wave lengths that in a repassage through another prism produce again white light. White light is thus an "overtone"[1] produced by the suitable adjustment of vibrations of the fundamental colours of the spectrum. That the fundamental vibrations of colours is not seen in white light in the same way that the fundamental notes are audible along with the superadded vibrations is, of course, not opposed to this assumption. That these two types of vibration obey the same laws of interference is shown by the fact that complete interference between two waves will in either case cause destruction. Another example of the creation of an overtone is shown in the magnetic action of an electric current: it is known that, if a small single electric cell be floated in water and a loop of insulated copper wire be connected to its poles, the cell will in every way behave like a magnet. We know certain wave vibrations are emitted from a current conveying wire even though insulated, for these are shown to deviate a magnetic needle. Magnetism can thus be considered as much the overtone of electric vibration as a superadded vibration is of two discordant notes. It must be noted that an induced current is not an overtone of magnetism, but an overtone of an interrupted magnetic induction only.

[1] This term, for the sake of simplicity, will be used to signify a supercycle of vibration, irrespective of the nature of the fundamental waves, unless otherwise specified.

That the assortment of Force itself is in cycles or periods, and brought about primarily by Cosmic influences, is shown by the periodic changes in Terrestrial Magnetism synchronous with the periodic activity of the Sun as seen by the presence of spots on the solar disc; an activity which itself is due to the periodic concentration of gravitational forces, from the fact that the planets are placed at certain periods in such a position that they act together on one part of the solar disc. Incidentally we may mention how, as the result of this solar activity, the quantity of Terrestrial Magnetism is increasing in definite periods, and how its distribution and intensity vary geographically in definite waves.

In the chemical laboratory, any coloured precipitate shows that, besides the chemical reaction, there are also other inseparable physical changes—thermo-chromic or electro-magnetic—which are due to the assortment of certain forces in action, and which are necessarily "overtones" of the operating forces.

It is in the more abstract and more highly developed forms of natural activity that these changes are more pronounced though more complex. The study of Ethics affords a good example. According to Fichte,[1] the basis of morality is reduced to a "moral Fatalism," presumably worked by Fear —a something which we prefer to call an "instinct," in the generic meaning of the term, inasmuch as it comprises all those psychic powers which lead to a conscious performance of actions that are adaptive in character according to the economic requirements of Nature relative to the unit; actions that are pursued by the unit at the command of Nature's pre-knowledge, but without the necessary knowledge, on the part of the unit, of the means employed or the ends attained. Such an instinct is created and developed by the interaction, like any other overtone, between certain forces: the activity of the human mind, or psychic energy, on the one hand, and contact with the dangers threatening the Human Herd on the other in the case of the Human Race. Thus did this instinct arise, and its active manifestation is represented in the protective Law of the Herd. The controversy raised between Fichte and Schopenhauer will be recalled. Schopenhauer realised that Fear was only capable of generating an order analogous to that of an animal community: the fuller development of Civilisation, as we know it to-day, being only possible by the quickening of Compassion,[2] what we would call another Instinct or assorted form of psychic energy, rising directly by the interaction between demonstrating the natural importance of the well-being of the individual on the one hand, and the Law of the State or of the Human Herd on the

[1] *System der Sittenlehre* and *Die Wissenschaftslehre in ihrem Allgem. Umrisse Dargestellt.*

[2] *The Basis of Morality*, part iii, chap. v, and part iv.

other—a Law which was made for the protection of the community as a whole only, and not developed enough to consider any of the units individually comprising that community. This is a point that Fichte, in his myopia, overlooked to explain the genesis of morality, for the fact remains that the Instinct of Compassion, as developed by the Doctrine of Christianity, interacted with the Law of the Human Herd and created the overtone of Civilisation as we see it developed to-day in a Christian nation.

These deductions, though in the realms of abstract thought, are indisputable, the interactions concerned being fundamentally the same as the more primitive ones between physical forces, for rhythmical action is the method of all physiological and psychological motion. The age periods, sleep, and other diurnal cycles, the monthly cycles and the periods in vegetable life, in innumerable variety, are well known, to mention but a few physiological periods. Of the pathological periods, we have certain periodic diseases and the periodic insanities: Folie Circulaire, Psychorhythm, Folie à Double Forme, Circular Insanity, Periodic Mania, Katatonia, etc. The impossibility is to find any true exception in both normal and abnormal life.

Thus it is that rhythmical vibrations, inherent in the Sun on the one hand, and in our planet on the other, have been able to produce overtones, which have ever complicated themselves into the various forms of energy responsible for these organised changes. It is the assortment of these forces that takes us from the more primitive to the more complex forces, until we get to the level of the Psyche. The physiology of the living body and the psychology of the mind are rich in examples of this periodic action and its superimposed overtones. Let us consider this assortment of Force from (1) the Cosmic, (2) the Physical, and (3) the Biological aspects.

The Cosmic aspect of the Assortment of Force naturally takes us to its very foundations. Bode's Law of the relative distances of the planets is only explicable as the result of rhythmical action conditioned by the antagonism between gravitational and electro-magnetic forces; for it is here a general law that all opposing forces react against each other in rhythms. While the gravitational forces are dependent only upon the masses of, and the distances between, the bodies, and vary rhythmically according to the periodic alteration in distance: the electro-magnetic forces are dependent upon radiation, which is in itself rhythmical in nature. If we accept the Nebular Hypothesis as assuming the formation of the first ring in a Primæval Nebula, we see that the gravitational forces of the ring would be diminished, compared to those in the centre, by the centrifugal element,

and we must also suppose that the electro-magnetic forces of the ring would, at one time, be less than those in the centre ; hence we have a flow of electro-magnetic forces —one form of which is light radiation—from the centre of the ring, and directly the electro-magnetic content of this ring approaches an equilibrium with the electro-magnetic force of the centre, repulsion is the result, *i.e.* the distance must be adjusted to meet the requirements between the respective quantities of the two forces. It is this rhythmical action or equilibrium that probably accounts for the repulsion of each subsequent ring, and ultimately for the relative distances specified in Bode's Law: the regular order of which can be due to nothing but the result of forces in rhythmical disequilibrium and equilibrium : the regular increase of distance of each subsequent planet being dependent on the diminution of gravitational forces due in part to the increased distance, together with the repulsion, not so much altered by distance, and having therefore an increasing effect in each successive ring.

Here we have a series of cycles with a common central focus, and as a result of this action tending to equilibrium, and dependent upon it, we have a supercycle with the secondary focus in the centre of the primary. This supercycle is the rotation of the planets upon their own axes ; the cycle of their satellites originating in, and being part of, this rotational movement. How this arises does not concern us here, except that it is sufficient to realise that this superadded cycle is a rhythmical cycle analogous in origin to an overtone. The fact that these are separate schemes of rotation, though indirectly dependent upon the main cycle, is shown in the discrepancies that have developed in the order of motion : one of which is the Martian satellite Phobos rotating in a lesser time than its primary—a condition which can only be possible if the dissipation of energy were greater in the primary than in the satellite, probably conditioned by a more retaining atmospheric envelope in the latter. Another aberration from the general arrangement in motion in the System is in the movement perpendicular to the elliptic, instead of from east to west, of the satellites of Uranus and Neptune, and probably of the rotation of these two primaries as well. The other discrepancy has resulted in the saturnian satellite, and two jovian ones, in going even farther than this and rotating in a different direction, aberrations that meet the analogy of overtone action ; for they are; in every way, forms of motion originally caused by the same forces, but which complicated themselves independently from the original forces as the result of interference.

In the Systems, as everywhere else, this complication of Forces is the order. A retrogression or simplification has a doubtful

possibility. These rhythms are fundamental, and it cannot be doubted that a rhythm dependent upon the alternating reactions between two forces can only be destroyed by complete interference between the two waves; and if this destruction were to extend to *all* the forces in opposition in order to make a Final or Ultimate Equilibration possible, we would possibly arrive at a Cosmos with NOTHING, not even Matter, for Matter itself is but a manifestation of Force,[1] a conception that is impossible.

Considering Bode's Law in relation to the Law of the Conservation of the Moment of Momentum, and the Theory of Dynamic Stability, we find how forces, most probably electro-magnetic, are gradually causing each planet to recede from the Sun: the visible effect of this force is seen in the outward deflection of light during an eclipse due to the Sun's gravitational field. The beginning of the fundamental cycle of these forces being possibly as we have considered above, the end being only possible by an end to this rhythmical antagonism—a complete disequilibrium between the gravitational and electro-magnetic forces; and since the first is getting less as the distances increase from a planet to the centre, and the second is getting greater with time, it is possible that the general scheme will alter by the outer planet Neptune getting so far beyond gravitational influences that a change will be inevitable.

That our planets are being slowly repelled from the Sun is an established fact, as it is also that our quantity of Terrestrial Magnetism is gradually getting greater. The Law of the Conservation of Momentum in our System holds good now; but we do not know that it will hold good for all Eternity. Neither can we consider that this repulsion would result finally in leaving nothing but the Sun as the remnants of our System; for the Sun itself, and the whole System, has a cycle of its own, of which we are at present utterly ignorant. This much we know, however, that our whole System is moving *en masse* towards the constellation of Lyra, and this movement is probably part of an unknown, undetermined cycle, for we know of no other kind of movement in the scheme of Nature.

Bearing this last fact in mind, a Final Equilibration, as imagined by Herbert Spencer in the *Synthetical Philosophy*, seems beyond all possibility.

There are other periodic movements, such as deviations from the orbital and rotational plane, and probably cometal movements, that contribute to the fundamental rhythm in the assortment of Forces.

Let us consider the matter from a nearer physical aspect.

If we accept Cox's hypothesis[2] of the Evolution of the

[1] Cf. Cox, *Beyond the Atom*, p. 145. [2] *Ibid.*

Atom, we must either agree with Thomson and Helmholtz in attributing the formation of Matter to possibilities that no longer exist to-day in our System, or to another possibility that cannot be overlooked—namely, that the formation of Matter as an overtone or integration of Forces—" knots in the Ether," as Maxwell and Thomson conceived the beginning—is still going on around us to-day, but so very slowly that we have as yet no means of estimating its increase, and therefore, as far as our senses are capable of instructing us, the corollary to the Law of the Conservation of Energy and Matter; that matter cannot be created still remains apparent. Yet in the physical world we are acquainted with other Laws that support the hypothesis that Forces are assorting themselves into heterogeneous complexities, manifested to our senses as new creations. Newland's Law of Octaves, Mendeleeff's Periodic Law, and Moseley's tables [1] have in themselves no direct connection with rhythmical action such as we are considering in " periods "; but these Laws concern us, inasmuch as they represent, as it were, an artificial tabulation of certain recurring properties among the elements that point to their creation as the result of rhythmical action [2]—fitful analogies to Bode's Law. This particular rhythm is beginning to be known now to us in our newer ideas of chemical action.[3]

Further, our knowledge of physical chemistry tells us, not only of the probabilities of a new creation or assortment, but of the certainty that rhythmical action is the basis of all Nature; as we have seen, radio-activity supports our ideas of new rhythms or overtones developing from the interactions of the main cycles, as does the fact that the stability of a chemical substance is conditioned by the assortment of forces—an overtone within itself, dependent on the fundamental Cosmic rhythm; for example, gaseous carbon is rendered more stable by the manifestation of certain forces and the latency of others within itself, as it changes its physical state from the gaseous to the solid crystalline form.

Spectroscopic analyses give us a further insight into rhythmical action. A fitful analogy to the rhythm of sound, where a string of a piano will take up the same sound wave it would emit itself when vibrated, is shown by the fact that the vapour of an element absorbs the same portion of the spectrum as it would emit if incandescent. The relation between the absorption spectra and the emission spectra of the same element is obviously due to the fact that in the former the assorted forces in the element, together with the vibrating forces in artificial light,

[1] Cf. *Introduction to Physical Chemistry*, James Walker.

[2] Cf. *The Radio-Elements and the Periodic Law*, F. Soddy, F.R.S.

[3] SCIENCE PROGRESS, January 1920, p. 376.

have resulted in absorption or wave interference [1] in one place and not in the rest of the spectrum, which still vibrates with artificial light, while in the latter, combustion shows only the vibrations, the same vibrations that in the latter case were invisible owing to interference—vibrations proper to the element itself, and not that of artificial light. The absorption, then, though it represents destruction from interference as we apply it experimentally, is actually due to a construction in the assortment of forces in the element itself, forces manifested to us only in its emission spectrum. The more highly assorted these forces are in the element, the greater is the absorption spectrum; the simpler the element, the more continuous is its spectrum. If we look at a red object through blue glasses, we see it black, like the absorption band of an element; if we view it as it is, we see it red, for it emits only the red rays, like the emission spectrum of our standard element; the rest it has absorbed. This interference in radiation has only one possible explanation—namely, that the object itself possesses the inherent vibrations that enable it to destroy some of the applied light vibrations; that is, a black object possesses certain inherent vibrations in its assorted natural structure that are capable of causing complete interference or absorption of all the vibrations of white light. Such an assortment of forces in the element is naturally permanent—as permanent as the atomic construction of the element and as indestructible. Were a gradual retrogression to the primitive continuous spectrum theoretically possible, it would necessarily be only towards a fresh beginning to the rhythm of a complicated cycle.

Our further examination of Light reveals certain other qualities in its nature that confirm its analogy with electric phenomena: as magnetism represents an overtone of electricity, so does the polariscope tell us that there are some vibrations in white light executed in other planes than that of polarised light, the simplest form of light vibration. This is another aspect of light vibration—a super-overtone independent of, and relative to, colour vibration: an overtone relative to white light.

In considering this quality relative to substances, we must bear in mind that the more highly developed the substance, the less primitive light phenomena it possesses, in a sense that, here as elsewhere, complexity seems to be the aim of Nature; that is, that the properties of a *molecular* substance which is said to be "optically active," when both aspects of the substance's

[1] The analogy between Sound and Light vibrations is more complete as we realise that two sound wave lengths, reaching the ear by two different paths, will cause complete destruction or silence, if the paths of the two vibrations differ in length by half a wave length; similarly complete mutual interference between two rays of light will cause destruction or darkness.

primitive nature are still apparent, "dextro-rotatory" or "lævo-rotatory," when only one aspect is present, are due to the relative assortment of forces within their *molecular* structures, properties that do not vary with the physical state of the molecule, so long as the molecular integrity remains, but only vary with the nature of the particular substance.

When this particular assortment has attained its highest complexity, there is an absence of polariscopic phenomena. That the molecular integrity is also capable of colour phenomena is naturally admitted. Here a crystal of copper sulphate absorbs all but the greenish-blue vibration; if emission were possible without molecular disintegration, it would theoretically emit all vibrations but the greenish-blue; indeed, we see the momentary evidence of this in the first stages of the emission spectrum of a molecular substance.

The biological aspect of overtones is even more apparent, and the development of Life, considered in this light, is more readily understood from our views on Immunity, possibly because here they are an aspect of Nature with which we are better acquainted.

Here it is proved, on very good evidence, that certain substances overproduced by the cells of an organism for its defence —lysins of all sorts—have the same origin and mechanism as certain substances overproduced by the ingestion of foodstuffs into the economy of the cell. It follows, from this and other equally supportive evidence, that a substance ingested either as an unaccustomed food or a poison will, by its degree of toxicity or dissimilarity to the cell, cause the cell to produce a substance to neutralise the harmful effects, and at the same time will cause the cell itself to become so altered in its molecular complexity that these harmful effects are no longer possible. Metchnikoff, in his *Theory of Phagocytosis*, deals more with the retention of these newly formed substances within the cell that alter its molecular structure than Ehrlich does, who pays more attention to the mechanism of production in his *Theory of Immunity*; but the fact remains that the construction of the cell is altered by moderately adverse nutrition; and, up to a point, the more gradual and reasonable, for the economic survival of the cell, be the application of the harmful effect, the greater will be the reaction produced by the cell towards its survival. This is essentially the basis of Wright's vaccine treatment: to regulate the amount of poison administered to the cell, in order to promote its greatest reactive recovery. These curious interactions are in every way analogous to our conception of overtones, only that here the cell itself is altered, and in the case of a more primitive overtone, the fundamental rhythms seem to remain the same: it is probable that the higher in the ladder of complexity be the assortment of force, the more is the effect on

the fundamental cycle; in such an organised system of Forces as our Solar System, the super-cycles or overtones of the Satellites are capable of exerting an appreciable action upon the fundamental cycle, contributing as they do to the Moment of Momentum of the System.

With our knowledge of Immunity, then, we can see what was probably the beginning and development of Biotic Energy: a primitive enzyme existing in an unsuitable medium—for every medium must necessarily be unsuitable in some degree, the converse being only possible with an ultimate equilibrium—must produce a something to render the medium less harmful, and at the same time the enzyme must alter its own complexity to promote its survival; for though no complete equilibration is possible, Nature is nevertheless working in this direction. A slow repetition of these processes will account for the "genesis" and development of Biotic Energy to its present state.

That these overtones or super-rhythms are more permanent than the overtones of sound vibration is clearly shown by certain indisputable facts: the latter are only possible in the medium of our atmosphere; but as Helmholtz, in his vortex theory, proved, were it possible to start such rhythms in an atmosphere of no density, their permanency would be ensured. Again, even in our atmosphere, the more complex these overtones, the more permanent is their nature; it seems as if, in their complexity, they have made provisions to avoid natural resistance. We see this provision and persistence in such overtones as those responsible for Natural Immunity, and in the Laws of Heredity we also meet with a superabundance of similar evidence. A tree imported to the Northern from the Southern Hemisphere, such as the Eucalyptus, will show the persistence of an overtone in manifesting its natural partial autumnal change *out of time with the new climate*, and independently from the rhythms of the fundamental forces of the Sun as it lives in the new climate. Again, in heredity, we see that only the transmission of certain overtones from the parent to the offspring can explain the law that like begets like and the transmission of acquired characters. Further, the rhythmical nature of the operating forces is clearly demonstrated in the obedience to the Mendelian Law. All these facts support the views expressed elsewhere, that the living protoplasm retains some of the periodic forces from Cosmic sources.[1] These two branches of science almost raise the theory under discussion to the dignity of established facts.

In this light, the "pangenetic particles" of matter that convey certain complexes of matter to the organs of reproduction, as assumed by Darwin's theory of Pangenesis, can be considered

[1] SCIENCE PROGRESS, January 1920, p. 418.

to exercise this virtue by conveying in their material selves the necessary assorted forces for their function. This is the only reasonable conclusion we can come to, for no mere particle of matter can become so endowed by absorbing more inert matter only: it is only the absorption of the necessary forces that can possibly give this endowment. The same vein of argument applies to Weismann's theory of the Continuity of the Germplasm. The germplasm can only be considered to possess its virtue because of an assorted form of energy within itself; it therefore deserves the more embracing term "germ-energy." Here the assumed mechanism of Weismann's theory shows the characterisitc superimposition of overtones: the assortment of forces capable of building up the organismal entity or pattern are capable as well of building up another potential pattern, which is manifested to us later in the offspring. The mysteries of "variation," "mutation," "reversion," show the manifestation of these potential portions of the whole assorted complex. Further, in the Mendelian Law itself we have the periodic character of this superimposition—the responsible forces being in every way analogous to rhythmic action and its dependent overtones.

The next assortment of forces from Biotic to Psychic complexity is the logical outcome of this rising assortment. Here, again, the conception of "Bioplasm" and "Psychoplasm" is liable to confusion—for its ambiguity almost presupposes that Force is but a part or attribute of Matter. Here the periodic cycles of the activities of the body, dependent upon the action of the Psychic batteries, is well known to every physiologist. Apart from such primitive rhythms as the vaso-motor functions, the circulatory and respiratory cycles with their accelerator and inhibitory nervous mechanism, we have the higher development of rhythms in the Complex; the diurnal cycle is represented by the rhythmical variations in the pulse-rate, temperature, and general resistance—the cyclic variations of this last is seen as the daily variation in the "Opsonic Index." The menstrual cycle is another example of a longer cycle. The well-known "periodic or alternating insanities" are examples of the exaggeration of imperfectly understood rhythmical changes. The phenomena of Hypnotism are clearly the result of an overtone between the operator and the subject, with whom the latter is *en rapport*. Even the division in psychology between Subjective and Objective is slowly giving way before a deeper insight into the workings of the human mind: we are beginning to realise that our conception of an object is the result of an interaction between the forces in the receiving brain and the impressions originated by certain qualities of the forces in and from the object, and transmitted to that brain by its apparatus of

sensation. The concept is thus nothing but a registered overtone.[1]

There are good reasons for assuming that all nerve impulses are rhythmical in nature, and that the Psychic functions are no exception to the production of overtones. Hence new Psychic overtones are ever in the process of formation. The more our powers of mental assimilation are perfected, the more the properties and complexities of that concept belong to the causative object; if a person learns a language indifferently, his attempts to speak in this language reveal characteristics that are peculiar to himself. When he has mastered that language, some of his eloquence may still belong to himself, but his personal errors or aberration from the conventionalities of that language have diminished, and his exposition belongs more to the language than to himself. So it is with all knowledge: in its imperfection it contains the errors belonging to the Human Race; as it becomes more perfected it necessarily belongs more and more to the object—Nature.

A physician can see this truth well in a case of aphasia. Take, for instance, such a case where the motor speech centres of the brain have been partially destroyed by disease; the patient cannot produce speech; he can write to dictation, though not spontaneously; his attempts at reading, pronouncing, and spontaneous writing, faulty and laborious, are due to his faulty self; but when these faults and incapacities have been corrected, when the adjacent parts of the brain have been educated to meet the requirements, he can more faithfully pronounce and write the thoughts emanating from his higher centres, and more accurately read what is written before him. A condition known as syringomyelia, where an individual is born without the nerve-channels capable of conveying thermal impressions to the brain, will render overtones relative to the temperature of an object impossible of formation: a block of ice and a hot-water bottle fail to produce the normal impression in the patient's mental complex, where the knowledge of the natural phenomena of heat is so wanting as to render the unconscious self-infliction of burning of frequent occurrence in such cases. The assortment of certain forces in the mental complex has here been impossible owing to the lack of the material channels.

Similarly, in the treatment of a case of cretinism, where the development of the potential mental complex has not taken place on account of the absence of the requisite molecularly

[1] Cf. Science Progress, January 1920, p. 475: "The combination of the movement of thought, so that the successive conditions may be produced, depends on an extraordinarily complicated underlying mechanism, the physical correlative to the mental state; and the successive impacts *giving rise to new mental states*."

assorted forces which we know by the name of Thyroid Extract, the molecular energy given in the form of this extract is capable, in part at least, of bringing about the development of the complex. No physician can possibly attribute the improvement to the Matter itself: were the elements comprising the extract separated from their molecular integrity and administered in their proper proportions, but not in molecular combination; not only is no improvement possible then, but the patient would be found in time to be suffering from iodine poisoning! It follows that this improvement is not due to the material extract, but is solely due to the assorted forces present in the material integrity of the molecular complex of the extract. Similarly, any increase in knowledge will be essentially due to the further assortment of Psychic Forces.

We have reached a stage in the development of Knowledge where Evolution, Heredity, and all other changes are being referred to the hidden springs of Forces, and not to an inherent part of Matter. The fallacies of the philosophy of the last century, when Herbert Spencer, almost as an afterthought, conceived the idea of considering the heterogeneous complication of Force along with the Evolution of Matter as two separate processes working towards an equilibrium eventually to be realised, must necessarily give way before the greater truth: that it is the Forces of Nature that are becoming more complex and assorted, and it is through Matter alone that we are able to sense this development. The Ultimate Equilibration imagined between Force and Matter has been attributed a reality it does not possess. It is seen that in the unit undergoing dissolution only some of its assorted forces are dissipated; certain overtones responsible for Civilisation, Evolution, and Heredity necessarily survive. The Main Cycle in the complexity of Force is still on the upward grade, and whether this cycle possesses an actual downward course—a trough after its crest—towards simplification is open to doubt; for in nature, as we see it to-day, we do not find any evidence, other than by analogy to other cycles, of this simplification. The formation of overtones from Nature's rhythms seems the only hypothesis capable of explaining the chief methods of Nature. The development of this conception may be fruitful in its results. All material development must be referred to the right entity, for it is the abstract entity Force that is undergoing steady development towards heterogeny, and we are only conscious of this "relative" to its material counterpart, the counterpart that is more accessible to our older and more developed senses, by which we are beginning to see the fact that, throughout each successive period of simplification, Force retains more and more of its heterogeneous complexities or assortments.

CORRESPONDENCE

To the Editor of "Science Progress"

AN INTERNATIONAL LANGUAGE

From Alexander J. Smith

Dear Sir,—The letter by the Secretary of the British Esperanto Society purporting to prove the superiority of Esperanto over Ido is not altogether convincing. In my opinion Ido is greatly superior to Esperanto as a neutral international language for the following reasons:

(1) Ido is not a one-man creation, but was evolved from Esperanto by an International Committee of eminent scientists and linguists, who rejected Esperanto as unsuitable, and whose object was to give the world a perfect and final International Language, retaining the good points of Esperanto and eliminating its defects.

(2) In Ido the special circumflexed letters, which constitute one of the chief defects of Esperanto, have been done away with, and Ido can therefore be printed, type-written, or telegraphed anywhere.

(3) In Ido certain useless grammatical rules found in Esperanto are suppressed, such as agreement of the adjective, and accusative, except where the accusative is required to avoid ambiguity. Even English is superior to Esperanto in these details of grammar.

(4) In Ido all combinations of consonants difficult for some nations to pronounce, though perhaps easy enough for a Russian or Pole like Dr. Zamenhof, the creator of Esperanto, have been eliminated, and another serious defect of Esperanto removed.

(5) In Ido the ugly *a priori* words and phrases which Zamenhof simply invented, and which make an Esperanto text look so barbaric, have been replaced by international expressions. To take one example, the phrase "all those who" is rendered in Esperanto by "chiuj tiuj kiuj." In Ido it is rendered by "omna ti qui."

(6) Another ocular defect of Esperanto, the ever-recurring "aj" and "oj" endings of plural adjectives and nouns, has been suppressed in Ido, for plural nouns in Ido end in "i" and there is no agreement of the adjective.

(7) In Ido the derivation is made regular and logical throughout. This is necessary for preventing idioms, and for providing a foundation for a complete scientific and technical vocabulary; because a perfect international language must meet the requirements of science and philosophy, as well as of commerce and ordinary life.

(8) In Ido the roots are chosen according to the principle of maximum internationality. Such a formidable task could only be accomplished internationally by eminent linguists, and consequently Ido is more international than Esperanto, and is *already known* by every educated European. Further, as Ido has no useless rules, no exceptions, and no idioms, it is far easier to learn than any national language.

In a word, Ido is the fruit of evolution and the final solution of the international language problem, for in addition to its other excellences it is as harmonious as Italian.

The one drawback of Ido is that it is a comparative new-comer, and

not so well known yet to the outside public as its parent Esperanto, but I, for one, feel convinced that, as time goes on, Ido must increase, while Esperanto must decrease.

I am, Sir,
Yours faithfully,
ALEXR. J. SMITH.

July 24, 1920.

TO THE EDITOR OF "SCIENCE PROGRESS"

COINCIDENCE, OR CONFIRMATION?

A DISCOVERY RELATING TO DRAYSON'S CENTRE OF POLAR MOTION

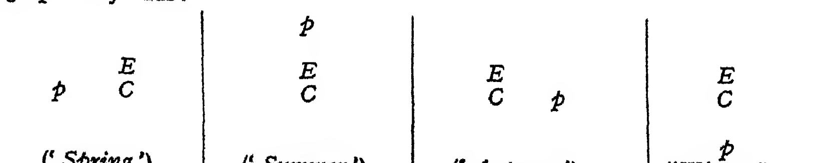
FROM ALFRED H. BARLEY

DEAR SIR,—A letter appeared in the July number of SCIENCE PROGRESS pointing out the fact that Drayson calculated, half a century ago, a position for the Apex of Solar Motion which agrees more closely with the latest determination (1908) than the latter does with any other; and the inference was drawn, that this coincidence was due to sound astronomy and not to chance.

Since that letter was in type another coincidence has been brought to light, quite accidentally, in the course of calculations made more or less out of curiosity. Before relating how the discovery came about, it may be useful to point out that in questions of evidence—circumstantial evidence that is,—(and all vital matters concerning geological or archæological questions really do rest on circumstantial evidence);—in all questions of circumstantial evidence where a number of items are brought forward any one of which is liable to dismissal as mere coincidence not causally connected with the point at issue, it is not enough for Opposing Counsel to depreciate, seriatim, each separate item on this ground and then claim a judgment in his favour because of the "poverty" of the evidence. The *coexistence of the coincidences* is itself a piece of evidence, one that requires proper examination and should be given due weight. The presence of a small boy in a pantry may be a coincidence, his chin being smeared with jam may be another coincidence, the parchment of the jampot being broken may be a third, but no housekeeper would be blind to their collective significance; and in like manner the cumulative value of "side-lights" of evidence (severally of small importance perhaps) gathers both mass and momentum from each addition.

Now to the subject of the present letter. According to Drayson, the Pole of the Heavens, *P*, traces a circle in the sky round a point *C*, the radius of this circle being 29° 25′ 47″ and the period of revolution 31,756 years. The point *C* is situated 6° 0′ 0″ from *E*, the Pole of the Ecliptic, and *P*, *E*, and *C* will be in alignment in A.D. 2295. These are the essential details of his discovery.

On pondering this brief statement it will be seen that the cycle of 31,756 years reproduces on a grand scale the main features of the common year; it has a 'summer' and a 'winter,' a 'spring' and an 'autumn'—'summer' in A.D. 2295 when the distance *PE* representing the obliquity is least (23°·4), and 'winter' A.D. 18172 (or 13583 B.C.) when *PE* is 35°·4, twelve degrees greater. The four quarters of this Great Year may be shown graphically thus:—



(' *Spring* ') (' *Summer* ') (' *Autumn* ') (' *Winter* ')

the distance between P and C remaining constant, while PE varies between

$$23^\circ\ 25'\ 47'' \text{ and } 35^\circ\ 25'\ 47''.$$

The conditions which may be presumed to have prevailed during the 'winter' have been ably described by Major R. A. Marriott in his article "The Ice Age Question Solved" (SCIENCE PROGRESS for April 1919), while those of the 'summer' must be very like those of the present era. It occurred to the present writer that it would be interesting to examine the 'spring' conditions, and he therefore calculated the obliquity at that epoch when PC and EC are at right angles to each other. (This is a simple problem; PC and CE being given, in a right-angled spherical triangle, to find PE; $\cos PC \times \cos CE = \cos PE$.)

What was his amazement on finding it worked out to $29^\circ\ 58'\ 53''{\cdot}0$! !

The reason for this amazement may not be obvious to the reader, unless he happens to remember that the Great Pyramid of Egypt, the most stupendous astronomical monument of antiquity, is situated in North Latitude

$29^\circ\ 58'\ 51''$ (Piazzi Smyth).

From which it follows that, at this epoch (5644 B.C.), the Midsummer Sun must have culminated exactly over the centre of the Great Pyramid. The exactness of this accordance will be appreciated when it is remembered that the whole discrepancy, viz. two seconds of arc, when reduced to linear measure, is only 202 feet, whereas the side of the Pyramid itself is about 750 feet, or nearly four times as great.

It is difficult to attribute this coincidence to chance. It looks uncommonly like design; for if the ancient architect of the Pyramid wished to erect a *gnomon* by which to determine the chronometry of the Great Year, he surely would relate it to one of the cardinal points of the cycle, 'spring,' 'summer,' 'autumn,' or 'winter'; and of these points in the common year, spring is usually that upon which the thoughts of mankind—of poets and idealists at any rate—are focussed. This assumption of course presupposes on the part of the architect a knowledge of the length and other details of the cycle. Yet it also affords criteria by which the feasibility of such an assumption may be tested; for surely, in that case, the *gnomon* should be so constructed as to indicate not spring only, but other crucial points in the grand cycle or Great Year. And the question may be asked, Can it do this?

Let us see. In the common year, March 21st is referred to as the 'commencement' of Spring; but June 21st is referred to as mid-summer, not as the 'commencement' of summer: from which it would seem that March 21st would more fitly be termed mid-spring, and that February 4th when the Sun is half-way between winter and spring should be regarded as the "commencing" of spring. Certainly in the Great Year whose phenomena we are considering, the 'commencement,' in this sense, of 'spring'—that is to say, the point in the cycle at which the angle PCE is 135°—must have been an era anticipated by prehistoric (not preintellectual) mankind only less eagerly than spring itself. And the question arises, Could the Pyramid say when this point had been reached?

When the angle PCE is 135° the distance PE, or in other words the Obliquity of the Ecliptic or the Sun's extreme declination in winter, would be

$$33^\circ\ 24'\ 1''{\cdot}3$$

and calculating its altitude on December 21st we have:—

	°	′	″
Altitude of the pole in lat. $29^\circ\ 58'\ 51''$ . .	29	58	51
Polar distance of sun	90	0	0
	33	24	1·3
Gives for angular distance from N. point of horizon	153	22	52·3
take from	180	0	0
Gives altitude of Sun at midday Dec. 21st .	26	37	7·7

Now there are three long passages in the Great Pyramid, of which one points northward and two southward; the inclination of these passages is approximately equal, and Piazzi Smyth concludes the angle of inclination originally to have been, in all cases,

$$26^\circ\ 18'\ 10''.$$

However, as Colonel Howard Vyse in his earlier measurements gave 26° 41′ for the northern passage, it would seem (assuming all three had originally the same inclination) that

$$26^\circ\ 37'\ 7''{\cdot}7$$

is not extravagantly wide of the mark. In any case, the accordance is near enough to be striking, and *more than near enough* to be practicable. For in spite of its length of 109 + 156 feet the southward-pointing passage, having a height of 47 inches, permits of a variation of ± 2° 3′ in the direction of vision when using the passage as an instrument of astronomical observation. That is to say, altitudes ranging between

$$24^\circ\ 15'$$

and

$$28^\circ\ 21'$$

could be taken by using suitable " sights "; and this, too, even if we accept the smaller value of 26° 18′ 10″ (Piazzi Smyth).

The last sentence prompts the reflection that the ' mid-winter ' point could likewise be observed; for the Sun's south declination on December 21st would then be 35° 25′ 47″, whence altitude = 24° 35′ 22″, which is within the range given.

To summarise briefly: It has been shown that, assuming Drayson's cycle to be a fact in nature:—

(i) The Great Pyramid is so situated as to have the Sun exactly—*exactly*—in its zenith at the epochs of 'spring' and ' autumn ' in the Great Year.

(ii) A southward-pointing passage, of great length, exists in the Pyramid, and is inclined at such an angle as to render possible a minutely accurate observation of the Sun's altitude, sufficient to determine just when those points in the cycle corresponding in the common year to December 21st (mid-winter) and February 4th (' commencement ' of spring) had been reached.

(iii) These facts, if not mere coincidences, certainly appear co-related. And *if* they are only coincidences, then, as the reader has been reminded in the first paragraph of this letter, they are not isolated coincidences; for there is another, independent of them, which also demands explanation.

It is true that all this implies an antiquity for the Great Pyramid greatly exceeding that usually attributed to it. But does not Herodotus state that the Egyptian priests claimed that their archives recorded astronomical observations extending over more than three cycles of precession; and are we altogether justified in an *a priori* disbelief in such a claim?

One more point may be mentioned. The *north-pointing* passage in the Pyramid, as has been stated, is given an inclination of 26° 41′ by Colonel Howard Vyse [1]; and since the point of the heavens thus exposed to view is 3° 18′ below the pole, it has been argued with great confidence by various writers on the archæology of the Pyramid, that it dates from the time when—by the orthodox motion of the pole—the fixed star Alpha Draconis

[1] This value would appear to be accepted if not endorsed by Sir John Herschel, according to the note on p. 771, vol. vii, *Encycl. Britt.* (ninth edition), article " Egypt." Writing away from books of reference, I cannot look up sources.—A. H. B.

(mag. 3·5) was at this distance from the pole: and a choice of two dates is possible, (i) when this star is approaching, and (ii) when it is receding from the pole. Obviously this theory requires the compound assumption that (*a*) this inclination of the passage was casual and not intentional on the part of the architect, and (*b*) that the motion of the pole was not known in those days (otherwise how account for their erecting a piece of architecture of such astonishing quality of endurance, when a few centuries would suffice to make it astronomically out-of-truth, or at least out-of-date ?).

But if we assume the Drayson movement to be that of Nature, the argument for design and knowledge, as against ignorance and chance, becomes much stronger. For on calculating the *minimum* distance of Alpha Draconis from the pole according to this motion, one finds it to be just 3° 18′, the quantity required! Furthermore, the angle *PCE* is found to be 59° 45′, which is sufficiently near 60° to be remarkable, as we shall see (indeed, if we make it exactly 60° the change in the polar distance *αP* thereby occasioned is imperceptible, and the difference in time is only 22 years).

We see, then, that the Great Pyramid, with its south- and north-pointing passages, furnishes mankind with an enduring astronomical transit-instrument whereby the stages of the cycle represented by *PCE* assuming successively the angles,

180°, 135°, 90°, 60°

can be determined by solar and stellar observations with great precision. These positions in the cycle or Great Year correspond, it will be remembered, to the dates

Dec. 21	Feb. 4	March 21	April 21
Mid-winter	*Commencement of spring*	*Mid-spring*	*May-time*

in the ordinary year, and their significance as seasonal points is obvious. May not the Great Year, perhaps, have a similar significance as regards the flowering of that hardy perennial, MANKIND?

ALFRED H. BARLEY.

July 1, 1920.

P.S.—A propos the last sentence, since writing this letter I have come across this curious passage in a work dealing with the Evolution of Symbolism: —"They (the builders of the Pyramid) had it [that is, astronomical knowledge] assuredly; and it is on this knowledge that the programme of the Mysteries and of the series of Initiations was based; hence, the construction of the Pyramid, the everlasting record and indestructible symbol of these Mysteries and Initiations on Earth, as the courses of the stars are in Heaven. The cycle of Initiation was a reproduction in miniature of that great series of cosmic changes to which astronomers have given the name of the Tropical or Sidereal Year [*i.e.* the precession cycle]. . . . Moses, an Initiate into the Egyptian Mystagogy, based the religious mysteries of the new nation which he created upon the same abstract formulæ derived from this Sidereal Cycle. . . ."

Note.—The "south-pointing" passage in the Pyramid, whose inclination I give as 26° 18′, is evidently that known as the Grand Gallery; but this does not extend upwards to the south face. A south-pointing passage which does so extend, and is often represented in Pyramid diagrams as parallel with the Grand Gallery, must have misled me into thinking the whole passage practically continuous. The conclusions in (ii) of my summary must therefore be maintained with reserve until further details of this passage are available. But they could be abandoned altogether without impeaching the main points referred to in (i) and (iii).—A. H. B.

To the Editor of "Science Progress"

THE DATE OF THE LAST ICE AGE

From Major R. A. Marriott, D.S.O.

Dear Sir,—Before replying to Mr. Tyrrell's letter in the July number of Science Progress, I have something to say about the improved prospect of solving this question geologically.

It seems that the time has arrived when so much evidence has been collected bearing on the recency of the last glaciation that the question may be now treated primarily on geological lines of evidence, showing that on the geological side alone we are in possession of facts which enable us, quite as much as in any other ordinary geological assumption, to deduce an acceptable scheme of successive cosmical events in late earth-history.

Adopting the truism that the obliquity of the ecliptic (O.E.) entirely rules the character of the seasons in these higher latitudes, surely, if opinions all over the world, based on the character of rock surfaces, and of morainic and sedimentary deposits, favour the occurrence of a greater obliquity in the near past, while observation records for the last 3,000 years also show a progressive increase in the obliquity, as we go back in time, there is an inherent probability, possessed by no other competing theory, that we are in touch with the real cause of glacial epochs. In addition, it can be shown that astronomical opposition to the acceptance of a marked increase of the O.E. is based on a paradox, which, after being tenaciously held for several generations as a dogma, has become so unsatisfactory that it has had to be now reconstructed by astronomers on the lines of a theory (Drayson's) which dots the *i*'s of geological inferences, and gives a full explanation of the cause of a recent glaciation. Thus, the inherent probability that such geological deductions are sound becomes greatly, and in geometrical ratio, increased.

With one exception, that of the submerged forests, it is only plain rock-script that has to be read, and yet these forests furnish in themselves sufficient evidence on which to build a working theory of the date and duration of the last glacial period, inasmuch as such evidence explains fully the diagnosis made by Clement Reid as to the date and phases of the forests' submergence. A recognition of this by British geologists is the only link in the chain of a world-wide evidence as to a recent glaciation that is still awaiting authoritative acceptance; and if judged on the evidence, without prejudice, will be found to fully respond to the requirements of the case, as phenomena which must have accompanied the process of ice accumulation at the poles, and the subsequent process of a steady and continuous refilling of the ocean reservoirs accompanying the final melting of the ice. (See *The Submerged Forests and the Last Glaciation*, Torquay Nat. Hist. Soc.)

The controversial underlying astronomical question need not stand in the way of a decision, but may be left to astronomers to work out at their leisure, since geologists can now form their own judgment independently. In pursuance of this method of handling the question, geologists have a further justification from consideration of the following.

The astronomers say that the cycle of precession lasts about 26,000 years; but it also appears that in the last century or so, during a time of presumably much more accurate observations, the rate of decrease of O.E., as one goes back in time, increases; and since in the nature of things this increase of rate and the before-mentioned increase of the obliquity cannot go on indefinitely, but must change on going back in time to the opposite condition of decrease, to enable the whole cycle to be compassed within 26,000 years, there must be a culminating point beyond which the O.E. cannot go. But whatever this point may have been, from the geological evidence it is clear

that it was sufficient to cause an extensive glaciation, and the privilege of finding the exact extent of change comes within the scope of astronomy.

This view of the subject was primarily written to be translated into French in the interests of M. Marcel Baudouin—a convert of my first convert Dr. Allen Sturge—whose studies of the dolmens and menhirs of Brittany and their orientations impressed him so much with the need of an astronomical interpreter that he informed me that he would have had to invent an explanation on these lines, had Dr. Sturge not brought Drayson to his notice. Again, Mr. Hadrian Allcroft has lately, in the April number of the *Nineteenth Century*, argued that the astronomical date given for Stonehenge is on other considerations far too distant in time, and that the date is really somewhere about 500 B.C., and not 1680 B.C. as given by Sir N. Lockyer, who bases his calculations on the orientation of the Hele stone, which he states to be 23° 54′ 30″. If de Horsey's curve is consulted for this obliquity, it will be seen that this cuts the date-line at about Mr. Allcroft's figure.

This is far from exhausting the surprising results in the department of archæology accruing from a study of the Drayson polar movement. Mr. Barley, whose letter to SCIENCE PROGRESS of July 1920 provides extraordinary strong evidence for Drayson based on his figures for the solar apex, has lately discovered that the Great Pyramid, from its geographical position and from the orientation of its passages, constitutes a gnomon by which to measure the epochs of a great cycle of Precession; and that the outcome of this interpretation is an almost exact agreement with the cardinal points of Drayson's cycle. These facts should act as a spur to geologists to assure them that with further endeavour the same light will guide them into all truth regarding the glacial periods.

Whatever astronomers may now say, or certain geologists may imagine, there are now sufficient grounds for asserting that an ice period occurred geologically lately, and was still holding sway in the temperate zones contemporaneously with times of which we have fragmentary historical records, as found in the seats of the ancient civilisations of Assyria and Egypt.

We need only consider what is merely a truism that it is the tilt of the earth to its orbit round the sun which alone produces the phenomena of summer and winter. If the obliquity becomes greater, the contrast between them is more marked: ice margins would be extended during the winter, while the summer, though hotter, would not suffice to melt the whole of the winter accumulation; glaciers would thus creep further and further down the valleys, and the process continuing would give rise to permanent ice-sheets obliterating regions once supporting abundant life.

The present signs denote that the same factors are in operation, but in the converse direction. The obliquity is decreasing; the ice is retreating rapidly at both poles, and seemingly a critical point in the process has been reached which enables us to observe a general decrease of severity in the winters of temperate latitudes over large areas, emphasising, as will be shown, the fact that we are still in process of emerging from a glacial period.

Now it is a fact, not a theory, that the obliquity has been decreasing, certainly for 3,000 years, *i.e.* since 1100 B.C. It is also a fact, in accordance with the above, that the fringes of the polar ice-caps have been retreating of late [1] to the extent of thirty-six miles in forty years in the South Arctic, with a corresponding melting of the confines of polar ice in Alaska, and elsewhere in the north. There is probably some cumulative effect of various factors which makes itself felt intermittently, because for many years the Swiss glaciers, with the exception of two unimportant ones, have been steadily shrinking.[2] This quick retreat of glaciers began about 1850, so that it seems

[1] And not only lately, as will be shown further on.

[2] The recent advance of the Grindelwald glacier, I think, must be due to some new affluent diverted from its original course.

as if various factors making for change obtain an increased effect from time to time. This also is shown by the formation of the moraines which marked the stages of ice-retreat in prehistoric times.

Steenstrup shows that within the last 6,000 years the climate in Denmark has favoured the pine, the oak, and the beech in turn. Brückner and various meteorologists have been at pains to show that there has been no change of climate for 2,000 years. Their statements will be seen to be fairly justified by consideration of de Horsey's curve, which has scarcely altered for 2,000 years, but has altered sufficiently in 6,000 years to be favourable to these three kinds of vegetation in turn. A recent pronouncement on our present mild winter by an eminent meteorologist is based on the assumption that the circulation of winds and waves are the main causes. These do affect the *weather*, but the world's *climate* is undoubtedly dominated by its degree of tilt to its orbit, though even this has been disputed by individuals, who ought to know better. In the April number of SCIENCE PROGRESS 1920, I have already recounted the various evidences of a recent glaciation from all parts of the world, though I omitted to add the evidence from Scotland and Norway, and the full evidence of the very striking corroboration supplied by the submerged forests. If this last evidence stood alone, it should compel attention. It is sufficient here to say that they represent the effect of a rise of sea-level caused by an enormous melting of the ice, in the spring, as it were, of the cycle, which was returned to the sea in repayment of the over-draft accumulated at the poles during 15,000 years of cold.

All these facts cannot, without reproach, continue to be ignored by science. Indeed, without regarding Drayson's discovery at all, there is quite sufficient evidence on which to postulate a recent glaciation, and to show that we have now entered a "genial period," reducing the extent of the polar ice, and heralding, in consequence, an amelioration in the severity of our winters, by slow but certain degrees, for very many years to come.

Geologists are quite unprepared with any solid opinion wherewith to confront the evidences all over the world which I have before summarised in their entirety for the first time, and which ought to convince the most resolute sceptic that there has been a glaciation geologically recently, and therefore only to be explained by some compelling cosmical cause, exerting its effects slowly but surely through the ages, causing recurrent glaciations with periods long enough to completely alter the *fauna* and *flora* of certain latitudes over and over again.

In the science of anthropology it is established that in the study of many flint implement cultures there is geological evidence to show that the period in question terminated in cold conditions supervening after a period of temperate climate. Thus, to quote from my previous work, *The Change in the Climate and its Cause*[1]: "In reviewing the history of mankind, one sees in the evidence of caves and 'drift,' and accompanying fossil deposits of the temperate zone, an alternating pageant, in the same area, of sub-tropical and cold conditions; of man and the mammoth and woolly rhinoceros in close relationship, followed by the horse and early type of ox; then, perhaps, a period of reindeer and Arctic animals succeeded by a more temperate fauna; with evidence, here and there in caves, of a sterile deposit, when the same area was for long untrodden by man or beast of any kind. The alterations of obliquity are alone responsible for these changing views, and it is only through Drayson that we can obtain an understanding of the cause of these periods and a measure of their duration."

In view of such researches, it seems useless to contend that there has been only one glaciation; but it is a contention that has been made by more than one geologist of note, and illustrates the chaos of present opinions. It is the

[1] E. Marlborough & Co.

ignorance of science on this subject which, more than on any other, forms a bar to progress in geology and in other kindred questions.

When all the evidences, supported by the elements of de Horsey's diagram of the curve, are summarised, they constitute a convincing list. Thus, by the light of Drayson we have revealed—

1. A reason for the retreat of the ice-caps at both poles.

2. A reason for the traditions of the Deluge.

3. A reason for the lake pile dwellings of 7,000 years ago.

4. A reason for the recent submergence of forests round the coasts of England, and those of the Continent for the same and lower latitudes.

5. A reason for the remains of pine forests in St. Patrick's and other islands within 15° of the North Pole, and for the remains of forests of cedars buried by ice in Alaska (Croll in *Climate and Time*, and Wright's *Ice Age in North America*).

6. A reason for pronounced climatic changes as shown by the vegetation of Denmark previous to A.D., while very little change, comparatively, has taken place since the Christian Era.

7. A reason for marks of ice striation on flints of Neolithic culture, and of Neolithic flints being found in the submerged forests.

8. A reason for the fact that in the sequence of the various cultures of flint implements the changes of culture are generally accompanied by very marked changes in climate intervening.

9. A reason for the noticeable change of climate on which we have entered, during the last sixty years or so, in England and in the southern latitudes of the temperate zone, which has reduced very rapidly the size of Alpine glaciers, made the winters milder and the summers cooler than formerly, and is causing the ice fringing the Arctic regions to retreat measurably year by year.

10. Finally, proof positive, from the general consensus of opinion among geologists abroad, and notably from the close agreement between Dr. Holst's estimate and de Horsey's curve for the dates of the oncoming and the passing away of glaciation in Southern Sweden, that there has been extended from both poles a glaciation from which the earth has not completely emerged—a universal and geologically recent glaciation which cannot be accounted for by any of the current theories, nor by all of them combined, but which is fully explained by Drayson's polar motion round another centre, not for a "few centuries" only, but during the whole cycle of 31,756 years.

These arguments show that the world has been subjected to glacial conditions which only passed away under 10,000 years ago. It is also evident that there have been several glaciations. Now, all the geological and physical theories either fail to respond to so recent a date, or fail to account for a recurrence of glaciations in the post-tertiary period at such comparatively short intervals, except the one theory, both rational and logical, that glaciations are due to a periodical increase of the obliquity of the ecliptic.

The only reason that might make geologists hesitate before endorsing the evidence herein given of a recent glaciation is that astronomers have for so long opposed any suggestion that the obliquity may alter considerably within ascertainable time-limits, that it would seem an outrage or a trespass on a sister science to fly in the face of its teachings. But let us see if the statements put forward by astronomers from time to time are consistent and coherent, or are such as to furnish any real barrier to the acceptance of the principle of the Drayson glacial theory, and it will become plain that certain established facts and statements indicate that this principle has now become interwoven with ideas that are professedly orthodox.

I will here quote some cosmical facts, and summarise the diverse astronomical fancies regarding them.

The main facts are:

1. The continuous decrease of the obliquity of the ecliptic during the last 3,000 years.

2. The steady reduction of the O.E. by half a degree during that period, and the increasing rate of secular change as we go back in time.

3. The past and present retreat of the polar ice-caps accompanying the diminishing obliquity.

The diverse statements made regarding the O.E. from time to time by astronomers are:—

(*a*) The O.E. is constant at 23° 28′, though it varies some 48″ per century (Herschel's *Outlines of Astronomy*).

(*b*) That only a change of O.E. to the extent of 1° 41′ on either side of the "invariable plane" is possible (Laplace).

(*c*) Some years after Laplace's researches, Leverrier found that the possible change on either side of the "invariable plane" must be increased to 4° 52′ (Leverrier).

(*d*) It has since been found that Laplace and Leverrier are both wrong, and that there are no limits to changes in the obliquity (*Encyclopædia Britannica*, 1906).

(*e*) But the above authority says that to bring about a change of two or three degrees would take a million years of our epoch. (See fact (2) above.)

(*f*) That to accept Drayson, *i.e.* not to accept the pole of the ecliptic as the centre of the polar movement, was equivalent to throwing overboard the laws of gravity (Astronomer Royal and others).

(*g*) But in order to explain why there is a decrease in O.E. which, if the text-books are right, should not occur, astronomers have to admit, in spite of gravity laws, that "for a few centuries" the ecliptic pole is *not* the centre of the circle made by the pole of the heavens. Now to make all these statements appear to agree, we want primarily an explanation, not of the "few centuries," but of the at least thirty centuries during which the O.E. is known by the records to have been decreasing. (See fact (1) above.)

The geological deduction to be drawn from all that I have written is as sound as any other geological fact which has been accepted as conclusive in geology, and it is my hope that geologists will see the force of all the evidence in favour of a recent glaciation and will accept the deduction as a working theory without waiting for a mandate from astronomers, who, as is made very evident by the above tabulated statements, are groping amongst difficulties, which have only been created by their own adhesion to paradox, difficulties which will entirely disappear when the question is openly handled on Drayson's lines.

That astronomers are unconsciously drifting into the Drayson position is apparent from the quotation referred to under (*g*) from Sir Oliver Lodge's *Pioneers of Science*, in which he says that the path of the pole "for a few centuries may without error be regarded as a conical revolution about a *different axis with a different period*," and adds that "Lieut.-Col. Drayson writes books emphasising this simple fact under the impression that it is a discovery"!

The dilemma of orthodoxy is sufficiently obvious.

In this letter I have endeavoured to keep the geological evidences apart from the more technical astronomical data as far as possible. I maintain that the former establish a reasoned and reasonable conviction quite as worthy of forming part of the geological structure as the assumption of the gradual evolution of organic existence in the fossils of the rocks; while the latter invite attack by the paradox involved by a changing length of radius, during a long period, to a movement which is regarded by astronomers as describing a *circle*.

Convinced that in the material universe paradox should have no place,

I have lately propounded another simple theory explaining the cause of the various phenomena of comets' tails other than by an enormous force of repulsion residing in the sun; and apart from the proofs of my theory, have shown that the behaviour of certain comets establishes, beyond doubt, the truth of J. H. Jeans's recent tidal theory of the formation of the solar system (*Journal of the Torquay Nat. Hist. Soc.*, 1920).

This, like Drayson's theory, is an apt illustration of a remark of Herschel's, in his *Discourse on Natural Philosophy*, that any occurrence which, according to theory, should *not* happen (*i.e.* which appears paradoxical) often serves as a clue to new discoveries.

Now that I have established the independence of geologists, permitting orthodox astronomers to retire from the controversy, which has become more purely geological, I have a few remarks to make in reply to the letter of Mr. Tyrrell. It is rather typical of the confusion of ideas regarding the Glacial Epoch. The one controlling factor for ice periods having been disallowed by astronomers, geologists have hitherto run wild, assimilating ideas, now from one theorist and then from another, in fruitless attempts to pierce the mists surrounding this question, while they fail to recognise facts lying at their very feet.

Owing to this tangle of opinions I purposely confined myself to bringing to light the great fact of a geologically recent, I might say historic,[1] glaciation, and though I have definite ideas about other glaciations in the post-tertiary period and about other glacial epochs, I am not prepared to be cross-examined on all points, say such as the cause of the Permian glacial epoch, or the presence of coal at Spitzbergen, as if my credit was imperilled by my inability to answer. I might as well be asked to describe why mammoth are found in such vast numbers in Siberia and remains of hippopotamus in great quantities in some caves of Southern Europe, which is probably due to a glaciation, but is not directly material to the main question of a *recent* glaciation. I have not space to fully traverse every statement made, but Mr. Tyrrell speaks as if "crustal unrest" was the precursor of glacial epochs. On the contrary, it is the immense weight of piled-up ice which depresses the crust in one place and elevates it in another. This subject has been fully studied in North America, and accounts for many of the raised beaches round our coast. That is the real reason why crustal unrest is associated in time with glacial periods. I admit, however, that but for Greenland our northern glaciations would have been much less severe. It is difficult to imagine a continent nearly as large as Europe without Russia, covered all over with mountains as high as the Bernese Oberland: this condition must be almost unique in the world's history. Its gradual uplift (it is now falling, so far as is known) would account for the severity of glaciations working up to a maximum, but these matters can best be left for discussion in the years to come. Until we establish the last and recent glaciation as a working theory, we shall never make progress.

Nor are we yet in a position to object to the "regularity of astronomical cycles" which, moreover, has not been established, nor does it necessarily follow. Slight differences in the centre of gravity of the earth might in time greatly affect the centre of polar movement,[2] and therefore the intensity

[1] According to Aristotle, the Chaldeans had records of the conditions of the seasons of this period, which were distinguished by alternating "ekpyrosis" and "kataclysmos."

[2] If the true centre were to be 6° nearer than the ecliptic pole, the O.E. would only attain 23° at its *maximum*, instead of 35° as now. Again, there is thought to be a probability, from a gyrodynamic point of view, of this movement being a spiral decreasing to nothing of obliquity, and then increasing (Crabtree).

of the cold cycles; in fact, for all we know, it might not involve a tilt great enough to cause a pronounced glaciation at all during several cycles together.

I am sorry to see the objection put forward again that an increased obliquity does not *ipso facto* cause the extension of polar ice. Persons holding this view must have very crude notions about this substance, and probably judge from their home experience of ice, which never attains a low temperature in our winters. Ice can store cold to any degree, and resists melting to an extraordinary extent. One might ask why, with our present obliquity, we have any polar ice in the Arctic regions at all with nearly six months of continuous sun, or why glaciers do not melt right away in the valleys. Scott says the temperature of surface snow in Lat. 77 South never was above 0° F. during midsummer, and Nansen records a similar experience. It is not a question of heat distribution, *pace* Mr. W. B. Wright, but of ice temperatures, which is quite another thing. With an increase of obliquity the "Arctic circles" must encroach on the temperate zone with an enlarged ice area, or this term would have no place or meaning. The very word "climate" is in origin "inclination," be it observed.[1]

Again, Messrs. Chamberlain and Salisbury have not it all their own way about the rate of advance of the polar ice. American and Swedish geologists estimate that it spread much more rapidly—700 miles in less than 8,000 years[2]—and we have the testimony of Mr. Ponting, a member of the Antarctic Expedition, who stated that the ice had receded thirty-six miles in the last forty years. The rate of the retreat of the Muir glacier in Alaska has been almost as rapid. Captain Scott also gives a picture of a valley from which 3,000 feet depth of ice had disappeared, showing that this process was by no means a temporary change of climatic conditions (*The Voyage of the "Discovery,"* vol. ii, p. 293).

One might think, also, from this correspondence that the conclusions of Swedish geologists stood alone, and that they were based on very hypothetical data, instead of being fully endorsed by opinions in North America, India, Australia, and New Zealand, which remove all possibility of regarding their agreement with Drayson as a coincidence. Mr. G. F. Wright, of Ohio, writes in *Origin and Antiquity of Man* as follows:

"The combined effect of all this evidence is irresistible. Large areas in Europe and North America which are now principal centres of civilisation were buried under glacial ice thousands of feet thick while the civilisation of Babylonia was in its heyday. The glib manner in which many writers, as well as many observers of limited range, speak of the Glacial Epoch as far distant in geological time is due to ignorance of facts, which would seem to be so clear that he who runs must read them."

I am glad to infer that my critic has not yet read the evidence of the submerged forests, as given by me, which seems to me wholly convincing, though a sometime president of the Geological Society said that he had studied these forests and saw none of the signs of glaciation. Did he expect to find glacial moraines or striated beach pebbles?[3] Another geologist, equally eminent, said he agreed with the conclusions of Swedish geologists regarding a recent glaciation *in Sweden*;[4] but it seemed to be of limited importance to

[1] Greek "klinein."

[2] Estimate based on the rate of advance of ice at present. Doubtless the rate was more rapid during the abnormal conditions of the ice phase.

[3] The late Mr. Clement Reid, the author of *Submerged Forests* (Cambridge University Press), told me he was quite ready to accept the fact of a recent glaciation.

[4] The region of glaciation extends to the province of Skania quite in the south of Sweden, in 57° lat., corresponding to that of Aberdeen.

him, since he added that "these conditions of cold had no application to our islands"!

Everything, without exception, that has any bearing on, or is in connection with, glaciations points in one direction. It is useless, and obscures the issue, to talk of "diastrophic deformation" and "intricate compound rhythms." We have had too much of word-coining and too little progress made in geology during the last half century; "the multitude of words has been the grave of knowledge," and as regards this question, in a lecture given at the Society's rooms on "The Relation of Man to the Ice Age," no hint as to its date was hazarded, though the Drayson solution of the mystery was known to the lecturer. It is time to relinquish this attitude now that I have shown geologists that they are independent of astronomers. There seems to be a tacit understanding that Drayson must be "taboo." For instance, a foreign geologist, after having written me a letter saying, "Your theory is correct and most welcome," was persuaded, I presume, to ignore Drayson, for, in a work published two years afterwards, he professed inability to obtain any aid from astronomy: showing how all-powerful a factor conventionality is in science.

The two cycles of astronomy, the orthodox and that of Drayson, are facetiously alluded to as if there was not much for geologists to choose between them; but of course the former gives no clue to a pronounced change of obliquity, while with Drayson it is shown to increase fifty per cent. To cap the allusion: the one should be styled Tweedle-dumb, and the other Tweedle-Q.E.Dee.

According to Mr. Tyrrell, it is "unfortunate" that such as I should butt in, irrupt he would call it, into a question which I have no right to discuss; but I have studied geology on and off for fifty-five years, though I have not taken what appears to be a pledge of orthodoxy by subscribing to the Geological Society. I am jealous for the reputation of my country as the *doyen* of geological science, and do not like to contemplate the possibility of this question being resolved elsewhere. It is vital to all progress.

Referring back to Mr. Spencer-Jones's letter in the Correspondence of October 1919, instead of "widely different conclusions" we can see established a perfect harmony between the two sciences, which could hardly exist were the *data* not true. In the case cited by him—the age of the earth—the astronomers were in the wrong, and their "incomplete knowledge" in this question also is by inference admitted by them.

Geologists may now fearlessly accept Drayson's conclusions without danger to their reputation. Once adopted, the clearness of vision regarding a subject, which has taxed the ingenuity of theorists for over a hundred years, will lead to many valuable results in other departments of science, and instead of an outlook of "many weary years yet," the future will be full of promise.

I am, Sir,
Yours faithfully,
R. A. MARRIOTT.

August 6, 1920.

NOTES

The British Science Guild

The British Science Guild held its last Annual Meeting in the Goldsmiths' Hall on June 8, during which much information was given to the members through the medium of the speeches of Lord Sydenham and the Rt. Hon. Lord Montagu of Beaulieu. The Address of the former on " Science and the Nation" reviewed the post-war conditions of this country, with special reference to the greatest factor of present-day unrest—the strike. Lord Sydenham dwelt largely on the " psychology of the manual workers," upon whom, he said, " the reign of the machine exercised a profound influence," and traced the bad effect of that machine on the mind of the worker, showing afterwards how this bad effect might be overcome by science. To quote his own words, " Can the reign of the machine be rendered beneficent to all alike ? Can science, which has been the creator of conditions which have favoured the spread of revolutionary ideas, help to provide the antidote ? It is a remarkable fact that, while scientific discovery was transforming the national life, there was no corresponding development of science-training in circles entrusted with government and administration. When foreign competition began to be felt, it was quickly realised that some of our industries had fallen behind in utilising the gifts which our leaders of science offered them, and that the Germans had passed us—in the great domain of chemistry especially. If this neglect showed itself in quarters where there were strong inducements to turn science to the fullest account, it was certain to prevail in Government departments with far more serious consequences to the nation. In a lecture delivered nearly seventy years ago, Prof. Tyndall said : ' I state nothing visionary when I say that in a country like ours, whose greatness depends so much upon the applications of physical science, it would be a wholesome and natural test to make admission to the House of Commons contingent on a knowledge of the principles of Natural Philosophy.' And he went on to make a powerful plea for the study of physics among the manual workers, ' not only as a means of mental culture, but also as a moral influence.' Science has immensely extended its dominion since that day, and has become far more closely connected with the welfare of the nation, and, therefore, more urgently needed in every branch of Government. Yet in Cabinets, in Parliament, and among our representatives abroad, Governors, Ambassadors, and Consuls, training in science is conspicuously absent. . . . As the result of the stern lessons of the war, we have set up much useful machinery for co-ordinating scientific progress, and for providing expert advice. Research has received a new impetus, both from Government and from industrial leaders. These are undoubted gains ; but they will not suffice unless the application of what can best be described as the scientific spirit to the solution of national problems is secured, which is the main object of the British Science Guild. We believe that our objects can be attained only by radical changes in our system of education. . . . The British Science Guild holds that it is vital to ordered progress that trained specialists should wield adequate authority in the national councils, and that it is essential and possible

to impart such a scientific trend to national education, from the bottom to the top, as will affect the psychology of all classes and help to remedy many patent evils." Lord Sydenham concluded by saying, "Upon Parliament and the Departments of State will devolve increasingly heavy responsibilities. Here, also, a new mental attitude is sorely needed in order that the gifts of science may be brought to bear without stint upon the national welfare. We want in Parliament a group of Members with sufficient knowledge to submit legislation and all public questions to the test of scientific examination as a corrective to the political and opportunist considerations which have led to grievous mistakes in the past."

Lord Montagu, in his address "On Some National Aspects of Transport," discussed its existing difficulties and suggested some remedies. He said, "The time has now arrived, therefore, when we should consider whether special roads for mechanical road traffic, from which all other traffic should be excluded, should not be provided, both from the point of view of alleviating the unfair burden borne by the present roads—which, except in respect of about half the main road mileage, are unfitted to bear this traffic—and because only at very great expense can they be made suitable for carrying heavy mechanical traffic. . . . I see no reason why overhead road traffic should not exceed the speeds of the ordinary suburban train services. I have always been convinced of the possibility and desirability of roads specially built and reserved for mechanical vehicles, and I am of opinion that to-day it is probably the best immediate solution of our increasing suburban traffic difficulties, and possibly of many other transport problems concerned with greater distances. In a brief paper such as this, it is impossible to discuss every national aspect of transport. But there is one suggestion as to how increased transport facilities should be assisted on which a word might be said. In the case of the extension, by private or public capital, of tramways, railways, or roadways, either outside large towns or into the areas which certainly benefit from better transport facilities, it may be asked whether the increased local values due to such construction should not contribute to the cost. To take a concrete instance, when the tube railway was extended to Golder's Green, a rise of between 300 and 400 per cent. took place in local values within the next two or three years. Towards this increase in land and house values the investor in the railway largely contributed, but the owners of the local property reaped the advantage without risking or paying anything themselves. No one will accuse me of being prejudiced in favour of the undue rating of property or the undue taxation of land, and hardly anyone is now an advocate of the kind of land taxes brought in under the famous and now defunct Budget of 1909. But it is quite conceivable that what might be called a local transport benefit tax might be levied on those who become richer by the extension of traffic facilities in their areas. The method of assessment and manner of collecting such a tax requires a great deal of consideration, and I am not prepared to go into details to-day. But there can be no doubt as to the inherent justice of asking that property of any kind, benefited by the employment of public or private money, should pay something for the benefit conferred. The revenue thus raised could be used in at least three directions: (1) To cheapen the rate at which the capital could be raised to carry out these services; (2) to diminish the amount of capital employed, for instance, by the land required for the line being secured free or at a low rate; or (3) to enable passengers' fares and goods rates to be fixed on a cheaper basis owing to the annual or other kind of contribution made by the surrounding property." But at the same time Lord Montagu recognised that in some instances, such as Clapham Junction and Willesden, the advent of the railway depreciated rather than enhanced the value of the adjacent property. He suggested also the advisability of endowing a "Chair of

Transport at Oxford or Cambridge or one of the newer Universities, in order that the science of transport might be studied apart from the unscientific and disturbing influences of politics and the clamour of trade disputes."

National Physical Laboratory

The Stationery Office, Westminster, S.W.1, issues a circular calling attention to the Report of the National Physical Laboratory for the year 1919, which has recently been published. "It contains the usual account of the activities of the Laboratory during the year, and on this occasion, in addition, a brief account is given of the assistance rendered to the various War Departments of the Government during the war. The description of the methods employed, and the apparatus devised for the testing, for the Ministry of Munitions, of the gauges required in the manufacture of shells and other munitions of war, will be found of special interest. Practically all Departments of the Laboratory were mainly occupied with war work. In the Heat Department, attention was given during 1919 to refrigeration problems, especially the heat-insulating properties of various materials. The heat losses in furnaces were also studied. The Optical Department was much occupied with calculation and design of optical systems. Improvements were made in the methods of determining the optical constants of glass. New methods were employed for the measurement of the radium content of radium samples, and equipment was provided for the investigation of protective materials used in X-ray work. The Electricity Department carried out investigations with regard to a number of wireless problems. Methods devised during the war for measuring the velocities of gun projectiles and gun recoil were further investigated. Research on the heating of buried cables, and on resistance materials (manganin, etc.), made good progress. The Metrology Department resumed its normal work in the verification of length standards, gauges, etc. Arrangements were made for the testing of volumetric glassware. The report of the Engineering Department describes much interesting research work, including experiments on the lubricating efficiency of oils, the transmission of heat from surfaces to fluids flowing over them, friction between fluids and surfaces with which they are in contact, methods of hardness testing and notched bar intact testing, the photography of projectiles in flight, etc. An account is given of experiments made during the war on the location of sounds. A short account is given of the advances made in Aeronautics research carried out for the Air Ministry. The researches carried out in the Metallurgy Department, described in the Report, include much work on light alloys both cast and wrought, investigations during the war as to substitute alloys, the causes of failure of materials, methods of hardening and case hardening, etc. The work on pots for the melting of optical glass made good progress. An interesting account is given of experiments in the William Froude National Tank during the war on mines, nets for defence against submarines, and for catching enemy submarines, the firing of torpedoes, seaplane floats and flying-boat hulls. A very complete investigation was carried out of the forms of ships with straight frames. The Report forms a volume of 150 pages, 10½ in. × 7¾ in., and is published at 5*s*. net (post free 5*s*. 5*d*.).

The Sister of Science (R. R.)

Like Hippocrene, the spring of English poetry runs on for ever, though there be none beside it to mark its beauty or to drink of its waters. Men of Science should, however, always endeavour to visit the fountain from time to time; and we may therefore say a few words about some good verse which has recently appeared and which others may like to taste.

In his *Anniversaries and Other Poems* (Murray), Mr. Leonard Huxley presents a form of art which is the commonest among to-day's poets—a series of beautiful word-pictures of natural sights or of simple sentiments, without many attempts at invention, constructive design, or philosophic teaching. It is a kind of corpuscular poetry, rich in particles and bright in the granule, to which selection of words and euphony of successive syllables give their distinction. English countryside and Alpine heights yield most of the pictures, many of which are drawn for young people. Especially good are *A Midwinter Birthday* and *Sylvester Eve*; but in his fine rhetorical piece *Enceladus*, the author figures the war—the escape of Enceladus from beneath Etna, his defeat by the gods, his return to imprisonment, and his appeal to Chaos (let us say, Bolshevism)—a short but elemental apologue.

Mr. Cloudesley Brereton's *Mystica et Lyrica* (Elkin Matthews) has an equally beautiful granular texture, but appears to be devoted to a spiritual, or at least a dualist, philosophy. Two of the pieces appeared in SCIENCE PROGRESS; and the description of the Norwegian fjords at sunset in *The Mystical Union of Earth and Heaven* is a most lovely thing, evidently a classic of pure beauty which must find a place in every anthology. The passages dealing with old age and the lyrics addressed to France have almost equal uniqueness; but scientific men are scarcely likely to accept the philosophy —which seems to be of the kind that Dr. Craggs calls *sit ergo est*. Thus when the poet exclaims in his verses called *After Reading Bergson*—

From the grey and grim bethels of Science,
  Squat, ugly, and meanly designed,
With its triple unholy alliance
  Of Logic, Mechanics, and Mind,
That pretends with a ruler and compass
  To plot out the soul of mankind—
From such arabesques, dodecahedral,
  Sham symbols of Life and its lore,
I return to the old-world cathedral
  That a new race of prophets restore,
Where the starved soul may wonder and worship
  The visions it harboured of yore—

most people will see, as in the alleged philosophy of M. Bergson himself, nothing but illusion. After all, truth, and not the feeding of starved souls, is the first concern of real philosophy. What Dr. Leonard Huxley says so beautifully of childhood may be applied bodily to such strains of thought:

Childhood lives in a fairy world
  Where fancy mints the sterling gold,
And thought's free charter grants for truth
  The strangest tales by the senses told.
'Tis a little world with a crystal roof
  Where the world without comes shining through
In tangled pictures oddly bent
  Like a bather's limbs in the stream askew.

Dr. Ronald Campbell MacFie's *War* (Murray—already reviewed in SCIENCE PROGRESS for 1918–19) is a fine single massive structure designed as a monument of recent events. It is only a short poem, divided into four parts with subsections; beginning with the formation of the earth and the appearance of life; describing human wars in the past and the recent one in detail; and ending with some large philosophies on the subject.

We have all known Dr. MacFie as a brilliant poet, and this work is full of good things. The construction is simple; but the isolated passages are often perfect of their kind, the descriptions brief but complete, and the wording various and euphonic. The author sees the immense effect of war in evolution:

> Bodies and souls from a furnace came, and lo in a furnace still
> War is moulding the human heart, smelting the human will.

and

> These are the throes
> That make the rose,
> These are the precious pangs of birth,
> These are the woes
> Whence ever grows
> The myriad Beauty of the Earth.

The poem is written mostly in (rhymed) *vers libre*—which Swinburne rightly detested. Lines of all possible lengths, sometimes truncated even to a single syllable, are not consistent with any real rhythm, and jog and jar the reader like a jazz band. The line in verse is equivalent to the bar in music; and without rhythm we simply return to prose—

> Spawn
> Of a monstrous dawn

suits the modern drawing-room elocutionist only; and the rhymes do but make the jogging more painful. I should have preferred, in a poem of this serious nature, more august measures. In all these books, moreover—not so much by defect of the authors as of the fashion of the day—there is very little invention; which to me is the chief thing lacking in modern English poetry.

This is not a fault, however, in Mr. Masefield's *Reynard the Fox* (Heinemann). The title is inadequate; for the book may almost be called the characteristic English epic—not of war but of the fox-hunt—bound to remain as a picture of our life of to-day. In the beginning it contains much of Chaucer; in the course of it, most of the sporting novelists; and in the end, pure Masefield. We commence with the Meet at the Cock and Pie—a series of wonderful pictures, with vignettes of all the sportsmen of the countryside, faces, figures, dress, characters, histories, touched one after the other with consummate pencil—like the gathering of Xerxes' armies in Herodotus; but this is England, and the poet has pictured English men and women at their best as perhaps they have never been quite drawn before in so few words. Then he introduces the hero in Hilcote Copse; then the find, and then the run. The reader runs with the fox and becomes that animal himself! The excitement, the fear, "the red heart of the beast," the cunning, the desperation, the exhaustion, and finally the hope of earth, and then—the earth is stopped! But the hero is not done yet; he makes for another earth; it is stopped again! What happens in the end?—well, the reader must find out.

The poem is written throughout in brisk four-foot couplets, often split between paragraphs in order to provide continuity—which I see a critic, who has not studied technique, condemns. The design is perfect; and every episode is full of observation, knowledge, humour, and invention. The Master of one of our Hunts told me that he could not detect a flaw in the book except some detail regarding grooming—which I have forgotten. To a scientific man the description of the psychology of the fox is wonderful and obviously true. Yet animals have no souls!

Formal Opening of Leplay House

The formal opening of the new home of the Sociological Society, Leplay House, at 65 Belgrave Road, S.W.1, was participated in on June 29 by more than threescore guests. The work of the Society has been carried on under a serious financial handicap since the outbreak of the war, and it is hoped that this new venture in providing it with suitable quarters will attract the support which is necessary in order to establish firmly the position of British scholarship in Sociology. The generation which at the inception of the Society in 1904 gave it generous support, has tended during the last decade to drop into the background, and therefore the situation demands that a younger generation should be given the opportunity to take up the work of sociological research and discussion, and give it a fresh impetus. Through its organ, the *Sociological Review*, the Society aims to offer a common ground for bringing together various sociological schools and traditions, while in Leplay House it is now ready to provide a common meeting-place for workers in every field that is concerned with social phenomena.

The opening meeting was held in the Council Room of the Society, and the address of the Chairman of the Council, Mr. Victor V. Branford, was an explanation and an interpretation of the decorative frieze which borders the walls of this room. Mr. Branford pointed out first of all that the purpose of calling the new home of the Society " Leplay House " was to emphasise the importance of the somewhat neglected work of the great French sociologist, Frederic Le Play, who had introduced into sociology the tradition of naturalist observation. The prime contribution of Le Play was his conception of the valley section, and his detailed examination of physical and spiritual life of each of the great rustic types which are developed by the work of the mine, the forest, the pasture, the field, and the ocean. The rustic formula of the valley section was complemented, Mr. Branford explained, by the civic formula of Auguste Comte, the analysis of the social order into chiefs and people, emotionals and intellectuals. Both these formulæ were symbolised in the frieze. One of the conditions that had kept sociology in the stage of abstraction was that it had been insufficiently oriented in time and place: the " Society" it had dealt with had existed too often in the imagination of the thinker who was attempting to analyse it. That was why other sections of the frieze definitely located Leplay House with respect to the environment outside on one hand, and to periods, both past and incipient, on the other. The sciences that treated specially the spiritual and the temporal aspects of society were, respectively, geography and history. More and more geography tended to be a synthesis of the natural sciences, whilst history gathered together the humanities; and accordingly the school of sociology that took over the resources of these two sciences was on its way to effecting a synthesis of the various specialisms which had hitherto lost some of their effectiveness in both thought and action by their isolation. This task of synthesis, this reconcilement of heretofore aloof or antagonistic schools of thought, with a renewed application of sociological methods to social life in definite cities and regions, was one of the main contributions which the Sociological Society might hope to make.

P.R.

The Proportional Representation Society has issued the thirty-seventh number of its *Journal* (August 1920). We are glad to see that this method of representation is coming more and more to the front. In every country or province where it has been tried, the result has nearly always been successful, and has been warmly applauded. The most notable instance in Canada was in the province of Winnipeg, and the *Journal* quotes extracts from daily papers of that State as follows: " ' P.R. is a success. That is the consensus of

opinion to-day as a result of Winnipeg's first experience with the new election system' (*Toronto Daily Star*, June 30, 1920). 'Winnipeg has put Proportional Representation upon the Canadian political map. Its extension to the rural constituencies in Manitoba is assured, and its ultimate adoption for all elections—civic, provincial, and federal—may now be looked on as inevitable. In Winnipeg the test was classic, and Proportional Representation came through with flying colours. Proportional Representation eliminated the excitement and bitterness from the election campaign; the knowledge that each party could only get its fair proportion, and no more, of the valuable seats made the old-time strategy and electioneering useless' (*Manitoba Free Press*, July 5, 1920)." In the United States, the best disinterested political opinion "is alive to the merits of Proportional Representation. It is the subject of an increasing degree of favourable comment in the organs of political and municipal science. A few cities and a large number of private organisations have already put the system in practice in their elections. At the present time, favourable opportunities for the adoption of P.R. in State constitutions are afforded in New Jersey and in Pennsylvania, and on these, the American P.R. League proposes to concentrate its efforts in the immediate future. New Jersey Legislature has had under consideration a Bill providing the Hare system of P.R. (*i.e.* the single transferrable vote) for the election of members of the State Assembly in counties returning three or more members. The Bill was favourably reported on by the Committee on Elections, passed the second reading unopposed, and will be reintroduced next session. The P.R. Committee of New Jersey has done excellent educational work in support of the Bill." Progress also has been made with the scheme in New South Wales, Malta, and Holland. "The last issue of this *Journal* described the application of P.R. to the Irish municipal elections of January 1920. Some 126 separate councils were elected; there were few uncontested seats; the persons voting were 69·79 per cent. of the whole; the spoiled papers were 2·79 per cent.; the machinery for counting votes worked without a hitch. There was a chorus of praise from the Irish Press, the Belfast Unionist Journals excepted. "In not one of the divisions of Belfast was a solid party return achieved"; P.R. in Belfast, as elswhere, had done justice to minorities. Labour and Nationalist citizens residing in Unionist wards, Unionist citizens residing in the Nationalist ward, all secured their full share of representation. Additional local government elections took place in May and June. These were for county councils, rural district councils, and, in county boroughs and in urban districts, for poor law guardians. All were held under the P.R. System.

"P.R. is now firmly established in Germany. The main principles of the Reichstag law, including P.R., were applied to the municipal elections for Greater Berlin on June 20, 1920." One of the chief aims of the Society, however, is to get P.R. firmly established in this country, and already many Members of Parliament are greatly in favour of its trial. "'We want a fully representative House of Commons' was the keynote of a large and enthusiastic gathering—numbering over a thousand—held at the Central Hall, Westminister, on May 13, 1920. It was an excellent tribute to the educational work of the Society, and recognition of the important fact that, even after equality has been gained in the matter of the franchise, the House of Commons will not possess a fully national authority until a method of election is adopted which will give every vote an equal value, and an equal share of influence in the legislation of the nation." The meeting carried unanimously the following resolution, which was moved by Lord Robert Cecil: "That this meeting, convinced of the necessity for making the House of Commons completely and fairly representative of the nation, calls upon His Majesty's Government to introduce the necessary legislation in order that the principle of Proportional Representation may be applied at the next General Election."

Awards for Medical Discovery (R. R.)

I have nothing much further to report on this matter since our last issue. At the deputation to the Lord President of the Council on March 2, he raised various difficulties as to the scheme which we had proposed; and when we asked him for a final decision, he replied that he could not adopt the scheme owing to the said difficulties. Personally, as I have said, I cannot attach much weight to them. It is difficult to see why pensions cannot be given for medical discoveries in consequence of trouble which may occur in selecting the men who have actually made them. The same trouble occurs whenever any awards are made; but Fellows of the Royal Society continue to be elected, and prizes are given, and numbers of persons are honoured in various ways by Governments and learned bodies. Why cannot more solid rewards be allotted in the same way? We also wrote again to the Chancellor of the Exchequer asking him why exactly he had refused to allow petitions on the precedent of Jenner to proceed to the House of Commons, and he has replied simply with the old formula that this is no longer the custom. Personally, I cannot see that this is any definite reason for his refusal. We have, however, loyally accepted the objections both by Mr. Balfour and Mr. Chamberlain, and have now asked the Prime Minister whether he will be so good as to allow some other means by which medical men can receive remuneration, or at least compensation, for their discoveries and inventions, just as inventors and those who serve the State in other lines receive them. The mills of Government grind slowly, but I hope that they will finally produce a little flour!

A Word of Appreciation

It is always agreeable to be appreciated, and we are glad to quote the following passage from the *Japan Advertiser*, Tokio: "SCIENCE PROGRESS, the remarkable and unique review of scientific work, thought, and affairs, which Sir Ronald Ross edits, needs no introduction. No periodical publication covers so wide a field or serves so useful a purpose in its own field. It is not 'popular' as the word is used, yet one is glad to think that there is a widening circle of readers, not all of them scientists, who find its pages invaluable as an essay and chronicle of scientific progress. At this time, when the language problem of Japan is arousing interest in Esperanto, a contribution in the current number of SCIENCE PROGRESS on Ido as a universal scientific language, 'the highest common factor of the modern languages of Europe,' is of special interest." Scientific publications, like prophets, seem to be appreciated anywhere except in their own country.

Learned Societies—A Plea for Reform (O. A. C.)

Temples and monasteries played a distinguished part in the early history of civilisation, for the familiar reason that in the old times they formed islands of culture in the midst of a sea of barbarism. The temples of ancient Egypt and Greece seem to have been hospitals, and even universities, as well as fanes for the worship of the gods; and Buddhist monasteries perform somewhat the same function to-day. But as civilisation spread outwards from them, they tended to lose their influence for good, and indeed ultimately to become negative foci of bigotry and corruption in the middle of a more enlightened populace—especially when they were divorced from the pursuit of the sciences and arts. The abolition of the monasteries by the virile English people of the sixteenth century was evidently due to recognition of this fact.

In latter days, learned societies have played a precisely similar part. Originally they were certainly invaluable. Thus when it was founded, the Royal Society must have been indispensable for the discussion of themes which could not easily be dealt with on paper when printing was difficult and when articles were written in Latin. And learned societies have always

given the additional opportunity for social intercourse between different persons interested in the same subject.

It is possible, however, that learned societies are now beginning to hear the footsteps of the fate which overtook the monasteries. An immense specialised Press has recently grown up, in which papers cannot only be published more easily and quickly than by reading at societies, but may also be studied more carefully and quickly than by attendance at meetings—while the same Press gives ample opportunities for written discussions which are usually much better, because more precise, than oral discussions by persons who are often little practised at public speaking. And it is a common complaint that both readings and discussions at societies are prolix, or dull, or—sometimes—too heated; and the attendance is often or generally very small. The same is frequently said of the meetings regarded merely as social gatherings.

Quite recently, however, the societies have received a new impetus from the fact that, being looked upon as the ultimate authorities on their special branches of knowledge, they are often consulted by Governments and other bodies. It is precisely this fact which has led to the present discussion. Have they indeed the right to be considered as the ultimate authorities on their subjects? If not, the administration of the country may suffer seriously from the error, especially during war.

Here we observe that by no means all the leading men of science or learning necessarily belong to societies at all—witness Darwin, Carlyle, or Tennyson; while others who do belong to them cannot find time or inclination to take much interest in their proceedings. In fact, it is quite arguable that, with occasional exceptions, the best men tend to ignore them considerably—which narrows their authority. Then, again, when we talk of the opinion of a society, we seldom mean that of the whole body of members, but almost always that of the officers and council for the time being, and perhaps a few members whom these have nominated to assist—which narrows the authority of the society still further. Lastly, the methods of election of officers, councillors, and even of members, are by no means always beyond criticism.

Apparently, in most societies, all such elections are made upon the recommendation of the council. This almost invariably means that the recommendation of the council is accepted at the elections—because, the members of the society being drawn from all parts of the country, they seldom have an opportunity to consider the council's nominations beforehand with such care as will enable them to collect a sufficient majority at the election to upset a given nomination if it be a bad one. Practically, therefore, the council possesses an almost autocratic power; and, as it nominates new members of itself, its power continues in its hands indefinitely, the large body of members remaining mere passive subscribers with little or no influence within it. Still further, the council generally consists of members from different parts of the country, who remain under a similar disability as compared with the officers; so that the power is still further limited, practically, to the officers. Now this method has often worked quite satisfactorily, and may do so still; but it is obviously open to grave abuses. Certain individuals, who have little else to do, who live on the spot, and who are often men without much distinction in the subject dealt with by the society, may gradually acquire power which they do not deserve and have no capacity to use. Recently, moreover, there is a tendency for State honours to be given very largely to officers of learned societies—so that, ultimately, it comes to this, that schemers may sometimes (let us hope, seldom) obtain rewards which they scarcely deserve, but which are really meant by the nation, not for successful wire-pulling, but for distinguished work.

Many feel that learned societies which are countenanced, subsidised, or consulted by Governments should be reorganised upon a more democratic

basis, by which all the members shall have equal powers as regards the election of officers, councillors, and new members, and officers and councils will be deprived of rights of previous nomination. Of course, a mere private society may organise itself as it pleases; but it is another matter when a society receives public funds or confidence; and we may therefore urge that the time has come when the latter class of societies shall be reorganised as indicated. For obvious reasons, this will never be done if the matter is left to the societies themselves; and a Government commission to inquire into the matter may therefore be suggested.

In minor matters also, many learned societies seem to be very behindhand. Thus the selection of papers for reading is often done by antiquated methods. The papers are usually subjected to the criticism of secret referees, who may quite possibly be rivals of the author, and may even (so, at least, it is often thought) reject his article and at the same time absorb much of the matter contained in it. An altogether excessive and indeed pharasaical attitude seems generally to be adopted by societies as regards the merit of papers. It really does not much matter to science or learning if a given paper is somewhat below par, because such a paper will be soon forgotten in consequence of its own demerits; but it does matter very much if a good paper is rejected, especially after long delay, since authors may thus lose both priority and time.

Moreover, few learned societies seem ever to trouble themselves about the interests of the workers in art, science, or literature. Yet, really, the work depends upon the interests of the workers.

If learned societies are considered to be only vehicles for publication and discussion, they are almost useless, since these can be much more easily obtained without them. If they become (which we hope is seldom) mere centres for the advancement of individual members, they are injurious. They will be really useful only if they are placed upon a new footing, and become not only genuine depositories of special branches of knowledge, but also organisations for the betterment of such branches of knowledge and of the workers. And if they are to be subsidised and consulted by the State, the State should see that they are properly constituted and conducted.

Another course is possible. A thing which requires reformation is seldom worth it; and it may be better, in place of attempting to reform old societies, to construct new ones upon a better basis, leaving the old ones to die a natural death, and to remain, what they now often are, antique ornaments—on the shelf.

Do English People Read? (O. A. C.)

Mr. H. G. Wells, owing to his knowledge of facts and to his powers of invention, is certainly the leading British author of to-day; and in that remarkable work, *The Time Machine*, in which he pictures the final decadence of the human race at, let us hope, a very far future, he gives as a proof of that decadence that the Eloi, though they possessed ancient museums and libraries, never entered them, but spent the day in eating delicate banquets of fruit and playing childish games with each other. Is it possible that we are already beginning to fall to this stage? I myself have entered museums in which there was no one except the caretaker, and perhaps a tutor or an uncle taking out a little boy for a treat, and in our libraries silence is always enjoined—and ensues because there is seldom anyone in them. We are the heirs of all the ages, but what do we do with the riches bequeathed to us by our ancestors? Liberated for a moment from my abstruser studies, I betook myself the other day to a seaside resort, where, *mirabile dictu*, there is a library close to the bathing-machines. This library was evidently put there fifty years ago, because it is full of books of that period. True, most of the books are the

novels of Miss Braddon, White Melville, Anthony Trollope, and so on ; but there are also shelves stocked with musty old volumes containing translations or biographies of great men of the musty old past. I asked the caretaker whether there are many readers. He said that the novels are now out of fashion, and that no one ever calls for one of the other books. Yet at this seaside resort there are hundreds of idle people sitting about on the sands with nothing to do. One can glance down at such of them as are engaged in reading, and one will find only newspapers, shilling magazines, and ninepenny novels in their hands ; and when one talks with them, only newspapers, shilling magazines, and ninepenny novels in their heads. Yet everyone nowadays asks for more education, for larger sums for universities and for scientific laboratories. I am tempted to ask, *Cui bono ?* In the England of to-day the most prosperous men are often the fools—and sometimes the rogues. Why, then, trouble to be anything else ?

Some Miscellaneous Publications

The *Sociological Review* (Spring 1920, vol. xii, No. 1) quotes a large part of Sir Ronald Ross's essay called "A Great Default" in SCIENCE PROGRESS of April last, and comments on the whole story from the sociological point of view. The comment is called "A Study in the Third Alternative."

The Addresses of the President and the Presidents of Sections of the British Association at its Cardiff meeting in August 1920 have been published by Mr. Murray in book form (price 6s.) under the title *The Advancement of Science : 1920*.

The Research Defence Society (11 Chandos Street, W.1) publishes Major-General Sir David Bruce's paper on *The Prevention of Tetanus during the Great War by the Use of Anti-tetanic Serum*. Like all Sir David Bruce's work, this one is very short, but reaches definite results ; and it is a pleasure to see that this dreadful disease may be prevented by such a simple process as inoculation. It is due to a bacillus which lives very largely in the soil, and which can enter our tissues through a mere scratch of the skin to which soil has gained access. In the tropics, tetanus follows such wounds very commonly, and we all remember the dreadful case occurring many years ago, in which a number of people died because the instrument used for inoculating anti-cholera vaccine was polluted with dust or mud. Such cases are much rarer in England ; but almost at the same time as Sir David Bruce's paper appeared, there appeared the reports of two cases in *The Times*. Both were fatal, and no preventative inoculation seems to have been used when the wounds were first inflicted. In many respects military medical practice is far ahead of civilian medical practice.

The Royal Statistical Society (9 Adelphi Terrace, W.C.2) publishes in its *Journal* an important paper by Major Greenwood and G. Udny Yule on the Nature of Frequency Distributions Representative of Multiple Happenings, with particular reference to the occurrence of multiple attacks of disease or of repeated accidents. The mathematics of all epidemiological matter should be considered much more than it is ; and it is singular that scientific researches on subjects concerned with the life and death of large numbers of people receive much less attention than their importance would indicate. The Annual Report of the County Medical Officer of Health, London County Council, for 1919, pp. 86 *et seq.*, discusses recent theories of epidemics advanced by Dr. Brownlie and by Sir R. Ross, whose conclusions appear to be somewhat in opposition.

The *Journal of Philosophy, Psychology, and Scientific Methods* for July 31, 1919, contains two articles on Dr. Strong's Panpsychic Theory of Consciousness and Perception. We fancy that few scientific men, especially if they are also biologists, are likely to accept this kind of alleged philosophy ; and most of

the arguments appear to be of the kind which confounds hypothesis with fact. Many people seem to think that theories are true simply because they happen to "flash" into their own minds, and then proceed to expound them in essays tricked out in the most extraordinary "philosophical" vocabulary.

We have received an entirely undated paper by Andrew Norman Meldrum, I.E.S., published by Humphrey Milford, principally discussing the origin of the atomic theory, and maintaining that John Dalton was anticipated by William Higgins. The author also shows how much Newton's work on a "simple elastic fluid" inspired both writers.

Dr. Charles Singer, of Oxford, the well-known specialist on Medical History, read a very interesting address before the British Academy on January 28, 1920, on "Early English Magic and Medicine," containing many amusing Anglo-Saxon superstitions. It is singular how the Anglo-Saxon mind continues to indulge in this kind of thing, but nowadays, instead of white, blue, yellow, and green venoms, we deal with ghosts, clairvoyance, telepathy, and similar tales.

Towards the end of 1919 the War Office published *Observations on Malaria by Medical Officers of the Army and Others*, edited by Colonel Sir Ronald Ross, Consultant in Malaria, War Office. This valuable book gives the results of the treatment of large numbers of men by various methods advocated by doctors for many years past, and mostly shows that, while almost any form of quinine treatment can cure attacks of malaria temporarily, scarcely any of the vaunted methods can cure it permanently. Apparently the best method depends upon the continuation of small doses of quinine for several months. The book also contains important papers on quinine, on the outbreaks of indigenous malaria in England during 1917 and 1918, and the prevention of malaria at Taranto in 1918. There is also a paper showing that two divisions which were heavily infected in Salonika were brought into France, and sent into the firing-line, completely cured, after three months' quinine treatment.

In the House of Lords, March 3, 1920, Lord Sudeley called attention to the considerable waste of money and resources now incurred at museums, galleries, and other similar public institutions, owing to the neglect to utilise them to their fullest extent in aid of education and general national interests. The discussion will be found in the *Parliamentary Debates*, vol. xxxix, No. 9.

Notes and News

The Honours list published on the King's Birthday contained only the following names of interest here:—*Baronet:* Mr. P. J. Mackie, who financed the Mackie Anthropological Expedition to Uganda; *Knight:* Mr. W. B. M. Bird, founder of the Salter's Institute of Industrial Chemistry; Dr. H. H. Hayden, Director of the Geological Survey of India; and Prof. J. B. Henderson, Professor of Applied Mechanics at the Royal Naval College, Greenwich.

Sir Douglas Mawson, Dr. T. M. Lowry, F.R.S., and Prof. P. F. Frankland, F.R.S., have been made *Officers* of the Order of St. Maurice and St. Lazarus by the King of Italy; while the King of the Belgians has conferred the following honours:—Order of the Crown: *Commander*, Prof. W. Sommerville; *Officer*, Dr. E. J. Russell, F.R.S.; *Chevalier*, Mr. C. E. Fagan, Mr. A. R. Hinks, F.R.S., and Mr. T. McRow. Order of Leopold II: *Commander*, Dr. W. R. Dunstan, F.R.S., and Prof. G. H. F. Nuttall, F.R.S.

The Rt. Hon. H. A. L. Fisher and Sir James G. Frazer have been elected Fellows of the Royal Society on the ground that they have rendered "conspicuous service to the cause of science."

The Rumford Premium of the American Academy of Arts and Sciences has been awarded to Dr. Irvine Langmuir, of the General Electric Company, for his work on thermionic and allied phenomena. The Barnard Medal of Columbia University goes, this year, to Dr. Albert Einstein.

Prof. H. A. Bumstead, professor of physics at Yale, has been elected Chairman of the National Research Council of the United States. Dr. J. R. Angell, the retiring chairman, has been made President of the Carnegie Corporation of New York.

Sir W. J. Pope, F.R.S., who has just been elected an Associate of the section for mathematical and physical sciences of the Académie Royale de Belgique, will be President of the Society of Chemical Industry for the year 1920–21. The gold medal of the society has been presented to M. Paul Kestner, President of the Society of Chemical Industry of France.

The Albert Medal of the Royal Society of Arts for 1920 has been awarded to Prof. A. A. Michelson, For. Mem. R.S., professor of physics in the University of Chicago. Mr. Alan A. Campbell Swinton has been elected Chairman of the Council of the Society for the session 1920–21.

Sir Ray Lankester was awarded the Linnean Medal by the Linnean Society this year, and Dame Helen Gwynne-Vaughan the Trail award and medal. Dr. A. Smith-Woodward will be President of the Society next session.

Dr. Simon Flexner, director of the laboratories of the Rockefeller Institute for Medical Research, has had the Hon. LL.D. degree conferred on him by the University of Cambridge.

Sir J. Cadman, Mr. W. B. Hardy, and Prof. S. Young have been appointed members of the Advisory Council to the Committee of the Privy Council for Scientific and Industrial Research.

Dr. G. C. Simpson, F.R.S., Meteorologist to the Government of India, has been appointed to succeed Sir Napier Shaw as Director of the Meteorological Office. Dr. Simpson was meteorologist and physicist to the British Antarctic Expedition 1910–13.

Dr. J. S. Flett succeeds Sir Aubrey Strahan as Director of the Geological Survey and Museum.

Prof. G. H. Carpenter has been appointed to succeed the late Prof. J. A. McClelland as secretary of the Royal Irish Academy.

Prof. K. Röntgen has retired from the chair of Experimental Physics at the University of Munich and from the control of the Physikalisches Institut.

Dr. T. M. Lowry, C.B.E., has been appointed first occupant of the new chair of Physical Chemistry at the University of Cambridge.

Dr. L. V. King has been appointed Macdonald Professor of Physics at McGill University; Prof. J. Wemyss Anderson to the John William Hughes Chair of Refrigerating Engineering at Liverpool University; and Dr. W. J. Dakin, professor of biology in the University of Western Australia, to the Derby Chair of Zoology in succession to the late Prof. Leonard Doncaster.

The following appointments have been made by the Council of University College, Swansea:—Professor of Metallurgy, Prof. C. A. Edwards; of Chemistry, Dr. J. E. Coates; of Physics, Dr. E. A. Evans; of Mathematics, Lt.-Col. A. R. Richardson, D.S.O.; Lecturer in Geology, Dr. A. E. Trueman.

The Council of the University of Leeds has appointed Mr. G. C. Steward, M.Sc., B.A., of Caius College, Cambridge, and the University of London, assistant lecturer in applied mathematics, and Mr. W. B. Grist, B.Sc., secretary of the newly formed Appointments Board.

The Department of Scientific and Industrial Research has now established four Sub-Committees to assist the Radio Research Board in the investigation of certain problems which have arisen in the course of its work. The first Sub-Committee is to deal with the propagation of wireless waves; it consists of Dr. E. H. Rayner (Chairman), Prof. Barton, Major Erskine-Murray, Prof. H. M. MacDonald, and Prof. Nicholson. Sub-Committee B, on Atmospherics, has Col. H. G. Lyons as Chairman, the other members being Mr. A. A. Campbell Swinton, Prof. S. Chapman, Major H. P. T. Lefroy, Mr. G. I.

Taylor, Mr. R. A. Watson Watt, and Mr. C. T. R. Wilson. Sub-Committee C, on Directional Wireless, consists of Mr. F. E. Smith (Chairman), Mr. N. P. Hinton, and representatives of the R.E. and R.A.F. The fourth Sub-Committee, on Thermionic Valves, is a very large one, the scientific members being Prof. Richardson (Chairman); Mr. E. V. Appleton, Mr. B. Hodgson, Prof. Horton, Mr. H. Morris Airey, and Prof. R. Whiddington. The Radio Research Board itself is at present made up as follows:—Sir Henry B. Jackson (Chairman); Commander Salmond (Admiralty); Lt.-Col. A. G. T. Cusins (War Office); Wing-Com. A. D. Warrington Morris (Air Ministry); Mr. E. H. Shaughnessey (Post Office); Sir J. E. Petavel (N.P.L.); Prof. Rutherford and Prof. Townsend.

We note with great regret the announcement of the death of the following scientific men during the past quarter:—M. Adolphe Carnot, the French chemist; Prof. Leonard Doncaster, F.R.S., Derby Professor of Zoology in the University of Liverpool; Maj.-Gen. W. C. Gorgas, K.C.M.G., Surgeon-General of the United States Army and President of the American Medical Association; Dr. Wheelton Hind, geologist and surgeon; Mr. A. H. Hiorns, at one time Head of the metallurgical department of Birmingham Municipal Technical College: Sir Norman Lockyer; Dr. G. E. Morrison, "of Peking"; Dr. R. Munro, the well-known Scotch archæologist; Prof. John Perry, F.R.S., emeritus professor of mechanics at the Royal College of Science, and general treasurer of the British Association; Prof. A. Righi, For. Mem. R.S., of the University of Bologna; Prof. J. R. Rydberg, For. Mem. R.S., professor of physics at the University of Lund, Sweden; Prof. S. Ramanujan, F.R.S., the first Indian Fellow of the Royal Society; Dr. F. A. Tarleton, of Trinity College, Dublin; Prof. C. A. Timiriazeff, For. Mem. R.S., emeritus professor of botany in the University of Moscow.

The memorial to the late Lord Rayleigh in Westminster Abbey is to take the form of a mural tablet which will be executed by Mr. Derwent Wood and erected near the memorials to Sir Humphry Davy and Dr. Thomas Young. The balance remaining after the expenses of this memorial have been met will be used to establish a library fund at the Cavendish Laboratory, Cambridge.

Among the many donations to the cause of science last quarter, we must note first the magnificent gift of 6,000,000 dollars from the Rockefeller Institute to the Medical School at University College, London, and to University College Hospital. From the same source the Medical Research Foundation of Elizabeth, Queen of the Belgians, Brussels, receives 1,000,000 francs for the general purposes of medical research. Medical education in the United States has, however, received even larger gifts, for Mr. George Eastman and the General Education Board of the U.S.A. (which is financed by Mr. Rockefeller) have together given 9,000,000 dollars to the University of Rochester for a school of medicine, surgery, and dentistry; and the medical departments of Columbia, Harvard, and Johns Hopkins Universities receive between them about 5,500,000 dollars from the estate of the late Capt. J. R. de Lamar. The trustees of the late Sir William Dunn have offered £165,000 to the University of Cambridge to found a school of Biochemistry, and Mr. Edward Whitley has offered £10,000 to the University of Oxford as a contribution towards the endowment of a professorship in the same subject. Oxford has also received £5,000 from the British Dye Stuffs Corporation towards the cost of extending the organic chemistry laboratory. The shareholders of Messrs. Brunner Mond & Co. have consented to the directors' proposal that £100,000 shall be distributed among the various Universities for the benefit of their chemical departments. Sir Jesse Boot has given £50,000 to University College, Nottingham, in aid of a scheme to form a University in that town: £30,000 to go to the building fund, and £20,000 for the chair of chemistry. Messrs. Alfred Holt & Co. have contributed £15,000 to the Liverpool University

Appeal Fund, and merchants interested in the African trade have promised £12,000 for a chair of colonial commerce, administration, and history, and to increase the endowment of the Liverpool School of Tropical Medicine. Dr. Rudolf Messel, among other bequests, left £5,000 to the Royal Institution, and £1,000 to the Chemical Society. The residue of his estate goes as to four parts to the Royal Society, and as to the other, to the Society of Chemical Industry, the income in each case to be devoted to scientific research.

At the time of writing, the decision of the Senate of the University of London concerning the Government offer of a site in Bloomsbury for the central offices of the University and the new buildings for King's College hangs still in the balance. The question is a difficult one in itself, and it is complicated by the refusal of the Government to bear the very heavy cost entailed by the construction of new buildings on the land they offer. It is clear that a University already too impoverished to pay its staff decently cannot lightly accept the responsibility for so huge a burden. On grounds of sentiment and, perhaps, of prestige, it would be a fine thing to establish a definite University quarter in a central position in the County of London; it is by no means certain, however, that the wealthy citizen would show his appreciation of that fact by adequate contribution to the Building Fund. Further, sentiment should not be the controlling factor in a matter of this kind, nor, indeed, the convenience of members of the Faculties or even of the Senate. A University exists for the benefit of its students, and there seems little doubt that, in so large an area as London, these are best served by a wise distribution of the constituent colleges of the University. King's and University Colleges are each large enough to produce an atmosphere of learning; and, placed within a few hundred yards of each other, their organisations and individualities would inevitably, in time, merge into one: the science faculties being concentrated in the one set of buildings, and the Arts in the other, instead of mingling as they do now, to the undoubted advantage of both.

Apparently, however, certain persons are considering favourably the possibility of transferring King's College and the University Headquarters to Kenwood. It is difficult to believe that this suggestion is a serious one, and not merely a means of opposing the Government scheme, for Kenwood is a rather inaccessible locality even to dwellers in the North of London; attendance from other parts of the county would be out of the question. Part would no doubt find excellent use as a Residential College; but the University as a whole cannot be residential, and to suggest that the main University activities of the county should be concentrated in so remote a position is simply ludicrous. There is a third aspect of the matter—that of possibility. The Government can offer the land at Bloomsbury, and has no land to offer in any other reasonably central position. London certainly should have a University quarter, and the surroundings at Bloomsbury are quite suitable for the purpose. If the Senate finds itself unable to accept the offer, it is to be hoped that it will be on the ground of inadequate means, and not with the idea of a remote and inaccessible suburb as alternative.

The views of the London Graduates' Association on this question are expressed by the following resolution, passed unanimously at a recent meeting of the Council:—

"That the University of London Graduates' Association, recalling the undertaking of His Majesty's Government of the removal, at their instance, of the Headquarters of the University from Burlington Gardens to the present buildings at South Kensington, to continue to provide site and buildings rate- and tax-free with maintenance and upkeep, and also to make provision for the full extension and development of the University as reconstituted under the Act of 1898, is of opinion that the renewed offer of land on the Duke of Bedford's estate, accompanied by an undefined maintenance grant, now made by the Government, is in no sense an equivalent

for the accommodation as at present guaranteed by the Government, and does not comply with the stipulations laid down by the Senate, after full consideration of this and other sites, in their resolution of June 17, 1914"; namely:

"That the Vice-Chancellor be requested to inform His Majesty's Government that the Senate, having considered various sites which have been suggested for the Headquarters of the University, are of opinion that it is undesirable to proceed further with such consideration unless and until His Majesty's Treasury intimate their willingness to provide accommodation more suitable in situation, more convenient in character, and on terms not less advantageous as regards tenure, etc., than those attaching to the present occupation at South Kensington."

The Chancellor of the Exchequer proposes to ask Parliament to increase the Treasury Grant in aid of the Universities from £1,000,000 to £1,500,000 in the Estimates for 1921–22, *i.e.* to make the extra non-current sum of £500,000 given this year recurrent. In addition to this absurdly inadequate provision, we understand that a certain amount of money is to be provided to supplement the pensions of the older teachers who, by reason of their early retirement, can benefit but little from the Universities Superannuation scheme. Nothing, apparently, is to be done to make the pensions received by the teachers as a whole more comparable with those received by school teachers.

An inevitable result of the depreciation of money and the insufficiency of Government assistance has been the raising of University fees. The new scales will, in most cases, come into operation next session (*i.e.* in October). They have been agreed upon after consultation among the Universities themselves and the Board of Education, and show an extremely modest increase, *e.g.* at the University of Leeds the change is in no case greater than 17½ per cent. Necessary as this alteration is from the point of view of the University teacher and administrator, it is to be regretted as a definite retrogression from the democratic ideal—no fees at all, but an entrance examination of high standard and a ruthless weeding out of those who subsequently prove themselves unfit for college training.

Finsbury Technical College is to be closed at the end of the session 1920–21.

The American Presbyterian Board has decided to establish a University at Cairo, and has purchased a site for the building. It will include five faculties: Arts, Oriental Languages, Teachers, Commerce, and Agriculture.

The Rubber Growers' Association is offering a sum of £5,000 in prizes for ideas and suggestions for extending the present uses or for encouraging new uses of Rubber. The conditions of the competition (which closes on December 31) state that special value will be attached to suggestions of a thoroughly practical nature accompanied by working details (including diagrams and designs) which would enable the suggestions to be adopted by a manufacturer. Further, it is desirable that the ideas should involve the use of large quantities of Rubber. The designs may not be patented in any country, and the Association reserves itself "the right at any time to publish, test, and otherwise deal" with proposals put forward by any competitor whether he receives a prize or not. Frankly this looks like using a sprat to catch a whale. It is unlikely that anyone, having worked out a really feasible idea, would surrender all rights in it for a reward which presumably would not exceed £1,000, and very possibly might amount to nothing at all.

The Report of the Advisory Committee on Civil Aviation states that, in the opinion of the Committee, the indirect assistance hitherto given to commercial flying is insufficient, and recommends that a sum not exceeding £250,000 should be given, during the next two years, to aviation companies working on approved routes; the subsidy awarded being equal to 25 per cent.

of the total revenue of the companies irrespective of the character of the load they carry.

Vol. xiii of the Contributions from the Jefferson Physical Laboratory (Harvard University) contains an account of Dr. P. W. Bridgman's work on the effects of pressure on the resistance and thermo-electric properties of metals. He finds that, under a pressure of 10,000 kilograms weight per sq. cm., the resistance of cobalt and tungsten wires is 1 per cent. less than at atmospheric pressure, while for lead, tin, and cadmium the decrease is no less than 10 per cent. The law connecting the resistance and pressure is linear, and the temperature coefficient of the resistance between 0° and 100° C. appears to remain unaltered. The effective of increase of pressure on the thermo-electric e.m.f. varies a good deal with different metals, but as a rule the thermo-electric power increases, both Peltier and Kelvin effects becoming greater with larger pressures.

In an article in *Science* (June 18, 1920), Langmuir puts forward arguments favouring quite a new type of structure for the helium atom. He suggests that the two electrons oscillate along arcs of the same ellipse (?) having the positive nucleus as centre, the path of each electron extending 77° 58′ each way from the mid-point, and being so curved that the radius vector at the end of an arc is 1·138 times the radius at the centre. According to his calculations (to be published in the *Physical Review*), this structure gives a value for the ionising potential of helium which accords with the results of recent experiments made by Franck and Knipping, namely 25·5 volts as against the 28·8 volts demanded by Bohr's 1913 model.

Prof. Raymond Pearl, of the Johns Hopkins University, contributed an interesting article on the effect of the war on the populations of France, Prussia, Bavaria, and England and Wales to *Science* (June 4, 1920). During the war there was no migration of the population of these countries, so that the ratio of the deaths to the births serves as an indication of the change of population. If this ratio, expressed as a percentage, exceeds 100, the population is, of course, diminishing. Using data obtained from the quarterly returns of the Registrar-General in the case of England and Wales, and from the *Journal* of the Statistical Society of Paris for the others, he obtains the following results:

Year.	77 Non-invaded Departments of France.	Prussia.	Bavaria.	England and Wales.
	Per Cent.	Per Cent.	Per Cent.	Per Cent.
1913	97	—	58	57
1914	110	66	74	59
1915	169	101	98	69
1916	193	117	131	65
1917	179	140	127	75
1918	198	132[1]	146	92

[1] Based on returns for first nine months of year only.

It will be noticed that the proportionate change of the death-birth ratio is approximately the same for France, Prussia, and Bavaria, in spite of the very different pre-war values and the equally different psychological condition of the peoples during the greater part of the war. The change in the index for England and Wales shows the same general tendency, though to a much smaller extent; and while the index does not show any marked rise as a result of the influenza epidemics of 1918 in any one of the countries considered, though such effect as is shown is greater for England than for any other. Data for the post-war period is not yet available except for England and

Wales, whose 1919 ratio has the value 73 per cent.—a pronounced drop, but still well above the pre-war level.

Mr. John Murray has published, for the Imperial Institute, a Monograph by Mr. R. H. Rastall, M.A., F.G.S., and Mr. W. H. Wilcockson, M.A., F.G.S., on the world's resources of Tungsten Ores with special reference to the British Empire (price 3*s*. 6*d*. net). From this it appears that the chief producing countries are the United States and Burma, which between them accounted for more than 50 per cent. of the world's supply in 1917. The British Empire as a whole has very large resources—more than enough to supply its own needs; and in other parts of the world, for example in China, there still appear to be vast supplies, sufficient to meet all demands for many years to come. The chief ores are iron tungstate (ferberite), manganese tungstate (hübnerite), wolframite, which commonly contains a mixture of the first two and calcium tungstate known as scheelite, which usually shows, on analysis, 2 or 3 per cent. of molybdenum. During the war the price was fixed by the British Government at about £140 per ton—roughly twice the pre-war value. In the United States it was not controlled and rose to ten times this figure. The metal is used for the manufacture of high speed steels (which contain from 13 to 30 per cent. of tungsten), for electric lamps and X-ray bulbs, and as substitute for platinum—for example, in spark coil contacts and the manufacture of acid-resisting alloys. The output from the United Kingdom is quite small—about 300 tons per annum; and that from Germany and Austria combined is estimated to be even less. The chief producing countries, in addition to Burma (4,500 tons) and the United States (5,000), are Portugal (1,600), Japan (1,500), China (1,200), Argentine (1,000), Bolivia (1,600), and Peru (1,000), the figures referring to the estimated output in 1917. In England tungsten ores are mined in various parts of Devon and Cornwall, and at Carrock Fell in Cumberland.

We have received a most interesting paper on "The Nature, Scope, and Difficulties of Industrial Research," prepared for the Tenth International Cotton Congress, at Zurich (June 1920), by Dr. W. Lawrence Balls, Chairman of the Joint Standing Committee of the British Cotton Research Association and the Empire Cotton Growing Committee. Discussing the need for research in the cotton industry, the author remarks that no new appliance has been invented for handling cotton, during the spinning process, for over a century, excepting only Heilmann's Comber, which was devised nearly seventy years ago. While there is no doubt much forgotten knowledge buried under conventional practice, yet the business of the cotton-spinner is a thing in itself; scarcely related to general physical knowledge at all. In fact, Dr. Balls considers the present position such that the first ten years of scientific work will have to be spent merely in defining what the spinner knows! He says: "We begin an attempt to connect the properties of raw cotton with the properties of yarn, only to find that no one possesses any definable knowledge of the properties of yarn, except its breaking strain in the lea test; and when we take this as a more modest starting-point, we find that no one knows what the lea-break means." The whole industry seems to be permeated with "last-my-timers" content to leave improvement to posterity. This is fatal to successful scientific work, and in order that benefit may be obtained from the research now commencing, it will be necessary for all classes to develop a faculty of curiosity, to exercise imagination, and to take up a critical attitude towards supposed perfection of things as they happen to be.

The most interesting part of the paper deals with the question of publication. How to reconcile the publicity without which research can make but slow progress with the secrecy which the business man considers an essential factor for his success. As a compromise, it is suggested that the publication of any discoveries should be deferred for five years. One thing is quite certain: if industrial research involves scientific obscurity, a very valuable stimulus

will be lost, and only an inferior type of man will be attracted to industrial work.

Bulletin No. 4 of the Department of Scientific Industrial Research deals with Solid Lubricants. Of these graphite is by far the most important, used either dry or in colloidal solution with water or a neutral oil. It possesses in a marked degree ability to adhere to metallic surfaces, and there to produce a smooth unctuous surface. The other solid lubricants—talc and mica—are not so good in either of these respects, and are, moreover, not nearly as soft, the best quality talc excepted. The most important artificial graphites are those produced by Dr. Acheson in the U.S.A., and by the Graphite Products Ltd., of Battersea. Acheson No. 1,340 contains over 99 per cent. pure carbon, 98 per cent. of which is in the form of particles less than $\frac{1}{500}$ inch in diameter. When converted into colloidal form by suitable means, it is sold as Aquadag (with water, containing 98·7 per cent. graphite) and Oildag (with oil, containing 97·6 per cent. graphite). Foliac Flake Graphite No. 101 (Graphite Products Ltd.) contains 99·95 per cent. graphite, but is comparatively coarse. When ground finer, it is known as Foliac No. 100 and contains 99·6 per cent. graphite, the chief impurity being silica.

The great advantage of graphite as a lubricant arises from the fact that it fills up pits, etc., in the metal surfaces, and by coating and impregnating them makes it difficult for them to seize. Dry it is used, for example, in lace-making machines and chocolate machinery, to avoid the spoiling of the material by ordinary oil, and in bottle-making machines, where the temperature is so high that the oil would burn away. Colloidal graphite mixed with neutral oil finds wide uses, especially with heavy bearings, worm gear, chain drives, and under suitable conditions with steam-engines. The chief disadvantage is the tendency of the graphite to flocculate in the presence of electrolytes; this makes it undesirable for use with internal combustion engines having an oil-circulation system, unless, indeed, it is mixed with highly purified neutral oils, on account of the danger of the choking up of oil-pipes, oil-grooves, etc., with the precipitated graphite. With splash-oiling systems this danger does not arise, the precipitated graphite merely accumulating in the bottom of the engine. Aquadag is used in wire-drawing metal filaments for electric lamps, and is apparently the only non-oily lubricant which has given satisfaction for the purpose. The whole of this Bulletin (which can be obtained through any bookseller, price 6*d.*) is most interesting, and it is recommended to the notice of all who have occasion to use lubricating materials.

The world-wide scarcity of fuel and the great and apparently permanent increase in the cost of coal have once more revived interest in the possibilities of peat as a fuel. In order that some of the fundamental facts concerning this matter might be brought before the public, the Fuel Research Board has issued, as a separate report, a lecture on "The Peat Resources of Ireland," delivered to the Royal Dublin Society by Prof. Pierce F. Purcell, Assoc. M.Inst.C.E., on March 5. It appears that the chief obstacle preventing the use of peat on a large scale is its watery character in the natural state. To drain a bog properly requires from three to five years, and even then it contains less solid matter than cow's milk! The figures are:—undrained down to 5 per cent. solid, drained 9–12 per cent., cow's milk 12–15 per cent. To obtain one ton of normal air-dried peat (which contains 25 per cent. of water), it is thus necessary to remove and dry from 6½ to 15 tons of the raw material, the exact weight depending on the thoroughness with which the bog has been drained. The drying problem is a very serious one. To use heat is most uneconomical, because so large a proportion of the final product is required to evaporate the water. Thus to obtain 100 lb. of 33⅓ per cent. wet peat from 70 per cent. wet peat requires no less than 41 lb. of the final product—even supposing the drying apparatus to have an efficiency as great as 60 per cent. Pressure alone, in the absence of heat, will only reduce the moisture

content to 70 per cent.—*i.e.*, will only remove three-quarters of the water associated with a given mass of anhydrous peat, the remaining part being held very firmly by the colloidal matter present. Air-drying thus remains at present the only commercially successful method. It is cheap but very slow, and limits the peat-winning period to the five or six months of the year during which climatic conditions are favourable. For the manufacture of produced gas 33⅓ per cent. water content is desirable, while for domestic purposes or for use in a steam boiler the content must be reduced to 25 per cent. It is in any case useless to reduce the moisture below 16 per cent., since anhydrous peat rapidly absorbs this amount when exposed to the air. The successful development of mechanical cutters and spreaders is an essential factor in the large-scale use of peat fuel. In Ireland each worker per day will cut and spread the raw material for only one ton of standard air-dried peat; in Holland the figure is three tons instead of one, but with the best mechanical cutters now available, this becomes 15 tons per worker per day, a figure which could no doubt be much improved. The Irish output for the whole of the year 1913 was as follows: 130 tons of 25 per cent. wet peat per worker with hand-cutting, and 230 tons by machine. The calorific value of this peat is only half that of average coal, so that the 230 tons are only equivalent to 115 tons of coal—a figure which compares badly with the 259 tons of coal won per man in Great Britain (in 1913 !) and the 681 tons similarly obtained in the U.S.A.

It is interesting to note that the largest user of peat fuel in Ireland is the Marconi Co., which burns 5,000 to 6,000 tons per annum for steam-raising purposes at the Clifden wireless station. In Germany, however, the large Central Power Station at Wiesmoor Friesland consumed 30,000 tons in 1913 for the generation of the electric current used to operate the Ems-Jade Canal, and the naval yards at Emden and Wilhelmshaven. However, even this consumption was exceeded by Bogerodzk Power Station, near Moscow, where plant having a capacity of 10,000 kilowatts has been laid down to supply the weaving factories in the neighbourhood, the surplus being transmitted to Moscow, forty-three miles away.

We have received the first few numbers of a new Indian fortnightly journal, the *Scientific World*, published at Lahore by L. Chaman Lal, B.Sc., from March 1, 1920, and give a warm welcome to the venture, which appears to be the first of its kind in India. Under British inspiration, education there has hitherto been almost entirely of a literary character, and journalism has therefore been mostly devoted to the usual futile and unproductive politics. We wish success to the new effort.

ESSAYS

CAUSALITY AND MEMORY IN LOSSKY'S EPISTEMOLOGY

(Joshua C. Gregory, B.Sc.)

THE root of scepticism, wrote Berkeley, is in supposing a twofold existence of objects of sense—one intelligible or in the mind, the other real and without it.[1] Hylas was in one stage of the journey to final scepticism when he deduced, from the original assumption, a distinction between real things or external objects and immediately perceived ideas, which are their images or representations.[2] He soon took the final step: "I tell you that colour, figure, and hardness, which you perceive, are not the real natures of these things, or in the least like them." Yellowness, weight, etc., he adds, are only relative to the senses, and we are ignorant of things—knowing neither their true nature nor even their existence.[3]

Hylas prefigured the course to be run by thought on its way from Descartes to Hume. About 200 years after Berkeley, Lossky again complains that "most epistemologists are inclined to maintain that immediate experience consists entirely of the individual mental states of the knowing subject."[4] A perceived object is still assigned a "twofold existence," in the outer world and in conscious representation. Thought still blunders in and out of scepticism, misled by the notion of mental copies of things or of ideas symbolising reality.

Berkeley dealt summarily with this "twofold existence" by conferring upon sensible objects the status of "ideas." "To me it is evident, for the reasons you allow of, that sensible things cannot exist otherwise than in a mind or spirit."[5] The ordinary distinction between things and thoughts was not completely annulled by Berkeley when he thus converted their separate modes of existence into a single one as "idea." He recognised that some form of duality was required to account for the invasive quality of the "ideas" which were his equivalents for the physical objects of common thought. "Whence I conclude," Philonous adds when he has referred "sensible things" to existence in a mind, "not that they have no real existence, but that, seeing they depend not on my thought, and have an existence distinct from being perceived by me, there must be some other mind wherein they exist."[6] This transference of duality from a distinction between physical things independent of minds and mental things, to a distinction between "ideas" originating in a superhuman mind and "ideas" originating in human minds, involved a limitation on the realm of causality. "There is nothing of power or agency included" in "all our ideas, sensations, notions, or the things which we perceive." Incorporeal active substance or spirit is the cause of all ideas. Here, as elsewhere, and often more explicitly, Berkeley extracts causality completely from the physical world (regarded by him as "idea" in a superhuman mind) and confines it to the

[1] *Principles of Human Knowledge*, § 86.
[2] First Dialogue between Hylas and Philonous.
[3] Third Dialogue between Hylas and Philonous.
[4] *The Intuitive Basis of Knowledge* (Duddington's trans.), p. 76.
[5] Second Dialogue between Hylas and Philonous. [6] *Ibid.*

realm of spiritual substance. The causal link between the sensible object and the idea aroused by it in the human mind is thus severed or annulled: extension, figure or motion, he declares, cannot CAUSE sensations.[1]

This causal connection between perceived objects and mental perceptions of them frequently presents itself as the neck of representationalism to its opponents. Sever it, deny that the mind responds to causal influences from outer reality by producing mental copies or symbolic representations of things, and the representationalistic plunge towards scepticism, promoted by its insistence on the duality implied in the independent existence of physical things outside the mind and their representative existence within it, ceases to trouble thought. Lossky redistributes causality differently from Berkeley: he does not, by banishing it from the external world, support the opinion that one "idea" cannot be the cause of another,[2] which is the Berkleyan equivalent for a non-causal physical world. He affirms that "every part of reality is so constituted that, if some aspects are given, other aspects are necessarily conjoined therewith in organic connection"; that "the explanation of the necessity involved in judgment will be found in the necessity involved in reality itself, in the organic functional relation between the various aspects of reality"; that there are necessary connections between the elements of the world which are causal in form.[3] But, like Berkeley, he so conceives, or attempts to conceive, the process of perception or knowing as to expel causal connection between known and knower. "Relations of causality . . . are already given in each separate act of perception," but "the subject's knowledge of an object is a fact that differs profoundly from other facts . . ."; and it is epistemologically disastrous to refer experience to the "causal action of the not-self upon the self."[4]

The causal nexus between perceived objects and percipient minds cannot be peremptorily severed without any other effect on their plexus of relations. A sharp separation between the self and the not-self, Lossky remarks, is naturally associated with their causal interaction, and contributes to epistemological disaster.[5] The not-self stands in outerness from the self, in a relation to it of transcendence, to direct its causal action upon it. The causal nexus breaks away and dissolves as Berkeley extracts outerness from objects by transforming them into "ideas." A reduced outerness still remains: "the things perceived by sense may be termed external, with regard to their origin—in that they are not generated from within the mind itself, but imprinted by a spirit distinct from that which perceives them."[6] The transcendent relation has, however, become a more immanent relation as the physical outerness is exchanged for the innerness of "idea." "I am not for changing things into ideas," says Philonous, "but rather for ideas into things—immediate objects of perception, not appearances but the real things themselves."[7] The same conjunction of annulment of causal connection between self and not-self with a drawing of the sensible object within the knowing process, is repeated, though it is repeated differently, in Lossky. There is the same motive—to substitute real apprehension for knowledge confined to appearances; there is the same method—to bring the object from the outerness of transcendency within the range of intuition by its immanency in knowledge.

"Identity of knowledge and the object known is only possible if the known object, in all the completeness of its reality, is present in the process

[1] *Principles of Human Knowledge*, §§ 25, 26.
[2] *The Theory of Vision or Visual Language*, § 13.
[3] *The Intuitive Basis of Knowledge* (Duddington's trans.), pp. 263–4.
[4] *Ibid.*, pp. 18, 20, 104. [5] *Ibid.*, p. 20.
[6] *Principles of Human Knowledge*, § 90.
[7] Third Dialogue between Hylas and Philonous.

of knowing, if it is IMMANENT in that process."[1] Does Lossky, when he thus revises the representationalistic conception of an object transcendently acting on a subject, really revise away the causal connection? Berkeley, observing the immanency of the "idea" in knowledge, secured it also for objects of sense by conferring upon them an ideational status. Similarly, Lossky, observing that "the processes of so-called inner perception shows that they are characterised by the presence of the object in knowledge . . ." and that "the structure of such knowledge is entirely determined by this condition," concludes that "our perceptions of the external world must possess the same character, and that here, too, the object apprehended must be immanent in the knowing process."[2] A remnant of outerness remained with Berkeley's "ideas" which were "imprinted on the senses,"[3] since they originated in a superhuman mind; a remnant of outerness also seems to remain in Lossky's "objects," which, though immanent in the knowing process, are still objects. It seems, at least, doubtful whether they do not retain their causal agency with their residual transcendency or outerness.

If knowledge is more complex than its object,[4] if the object apprehended is an experience which is compared,[5] if the knowing subject obtains material from the object and elaborates it into knowledge,[6] if the "objects of knowledge become differentiated,"[7] if knowledge "is a process of differentiating the real world by means of comparison,"[8] the subject still seems to respond to a call from the outer world. The range of causal action has, so to speak, been shortened: the object enters to stir a differentiating process instead of arousing it from afar. The activity of comparing belonging to the knowing subject, if its results are determined by the nature of the objects,[9] seems to be causally related to the reality it compares. If the "law of causality amounts *ex hypothesi* merely to the conviction that any given event necessarily follows upon a complex of certain other events,"[10] if causality, descriptively stated, is events succeeding other events within a system, the comparing, differentiating process which results in knowledge when the object becomes "immanent" appears to be causally related to it. The greater intimacy between self and not-self obtaining through the latter's immanency in knowledge does not appear to annul their causal relation; reconciling subject and object by co-ordination instead of by subordination, and retaining their independence in an indissoluble unity,[11] does not sever their causal bond, nor does it necessarily even reduce it.

Lossky seems to open himself to a suspicion that he has carefully described an alternative method of mental copying. Uncompared reality flows before us, he writes, dark, formless, and unrecognised. When attention is attracted, "the intellectual process of discriminating begins":[12] form succeeds to formlessness, and the vague becomes definite. Reality, through "a process of differentiating the real world by means of comparison" and retaining its real character, "becomes a known reality, a presentation or an idea."[13] The final "differentiated appearance," which never exhausts the richness of the reality, "composed entirely of elements present in the object itself,"[14] strongly suggests an impression made by a seal, or, to keep the metaphor close to the metaphored, a seal imbedded in its impression upon wax. Reality striking in upon attention, discriminating processes

[1] *The Intuitive Basis of Knowledge* (Duddington's trans.), p. 31.
[2] *Ibid.*, pp. 75–6.
[3] *Principles of Human Knowledge*, § 1.
[4] *The Intuitive Basis of Knowledge* (Duddington's trans.), p. 76.
[5] *Ibid.*, p. 82. [6] *Ibid.*, p. 101. [7] *Ibid.*, p. 105.
[8] *Ibid.*, p. 226. [9] *Ibid.*, p. 262. [10] *Ibid.*, p. 39.
[11] *Ibid.*, p. 69. [12] *Ibid.*, p. 225. [13] *Ibid.*, p. 226.
[14] *Ibid.*, p. 227.

stirred in the knower, these discriminating processes guided by the constitution of the reality discriminated, truth acquired by the moulding of differentiating discrimination on the object: there seems to be causal interaction, and there seems to be something very like a "copy" of reality made at close quarters instead of at a distance. If a draughtsman drawing a distant house on his paper represent "copying" through causal action by a "transcendent" not-self upon a self, an engraving stamped from a plate by close contact would seem to represent "copying" in the theory of intuitive knowledge through the immanence of the object in knowing.

Copies are surely causally affected by the copied. False copying indicates a co-operation between copied and copier which is surely causal in character. If the copier introduces error at the instance of the copied, if he is stirred, as his false copy seems to show he is stirred, by the presence of his object, there is causal action. "But since, in the process of knowing the only agent is the knowing subject who makes the comparison, it is only he who can introduce into the object elements foreign to it. In this sense it may be said that truth is the OBJECTIVE and falsity the SUBJECTIVE appearance of the object."[1] Lossky is compelled to admit the efficiency of the knower when he makes mistakes; since his mistaken versions are responses to the reality he falsifies, for it would be irrational to suppose in him a pure spontaneity which is regardless of it, does he not also implicitly admit causal influence of known upon knower? If it is perplexing to deprive the reality immanent in knowledge of its causal bond with the discriminating process which differentiates it, it is still more perplexing to discover that there may be lapses into such causality which provoke error in knowing. It would be hypercritical to pick out the causal terms which constantly appear in Lossky's description of the knowing process, for language is saturated with such causal terms, and he is compelled to use this common instrument of expression; but that description, taken as a whole, seems to require an essential causal action between immanent object and differentiating knower which in no way annuls the causal bond between self and not-self.

The causal relation between known and knower involved in misrepresentative knowing seems to carry the *a fortiori* implication of a similar causal relation when the knower truly knows his object. Theories of knowledge often disclose their weaknesses in their estimates of error, and Lossky seems, in his explanation of falsity, to confess the causal link which he systematically denies in his exposition. This causal link seems to be as immanent in his version of knowledge as he supposes the object to be in the process of knowing, and, imperfectly concealed in his estimate of truth, to be laid bare in his estimate of error. If its estimation of error probes a theory of knowledge, its relations to memory probe it even deeper still. The cumulative effect of experience upon percipiency, the undoubted fact that "experiences differentiated in the past do facilitate the differentiation of present experiences, and also that the process of indirect perception is accompanied by memories"[2] with all the familiar detail of memory, seem to scatter doubts on Lossky's epistemological path. Comparison with memory images suggests, for perceived objects, a looseness of attachment to the knowing process which appears to be curiously incompatible with their immanency in it. With a turn of the heel the eye exchanges a perception of a tree for a perception of a pig; objects pop in and out of perception in agreement with our movements; darkness deprives vision of its objects, and light admits them to it. Memory images, allowing for obliviscence, neither disappear in darkness nor depend in an essential way upon time and place. It may be true that "crude arguments," like the

[1] *The Intuitive Basis of Knowledge* (Duddington's trans.), p. 227.
[2] *Ibid.*, p. 338.

removing of "all extra-organic objects from the field of consciousness" by "the shutting of the eyes and the stopping of ears," do not "at once decide in favour of all experiences being internal," but it does seem unconvincing merely to suggest that "the object thus detached from the knowing subject's body becomes less interesting; it no longer attracts attention, nor does it offer a sufficient number of HABITUAL starting-points for the activity of discriminating and comparing to be directed upon it."[1] Evanescent perpetual objects, like sparks from a fire or coloured stars from a rocket, seem to capture attention, strike in upon the mind and leave upon it, in the form of memory images, effects of their influence. Our own movements, or movements in things themselves, perpetually seem to alter our perceptual fields, and to leave the effects of these perceptual fields on memory because they compel all physical objects to be evanescent for perception. Memory images seem to insist upon causality in perception, because they seem to be obvious effects of the object upon the subject. They also, because they seem more fitted for the rôle of immanent objects than physical things, throw doubts upon the percept's claim to immanency. Lossky is compelled to HOMOGENISE percept and memory image, and this HOMOGENISING is certainly violently opposed to the obvious difference between our mental reactions towards them. He suggests that association by contiguity may rest "upon a relation which holds throughout reality between the past and the present—a relation of ground and consequent which implies that, whenever some elements of a whole are found, its other elements must also really be present in one form or another. In that case, the memories engendered by association would themselves be simply a species of indirect perception, a sort of clairvoyance of the past."[2] The phrase "indirect perception" plainly indicates the homogenising of memory and present perception which is completed when he writes: "Every element of reality, even a fleeting event in the far-off past, remains eternally one and the same, identical with itself."[3] The flight of a shooting star, with its beginning and end, has a continued existence, according to this doctrine, in the memories of observers; it seems also to multiply that continued existence if there be more observers than one. These observers must be placed appropriately both in space and time to see the original event, they can refer to it in their memories virtually at any subsequent time and in any subsequent place. This latter independence of place and time is most simply, as it is most usually, explained by the permanent "immanence" of memory images in their containing minds: these images are methods of reference to the original event impressed by it upon the mind of the knower. If an eagle soars in the air, the event may be immanent in the knowing processes of a hundred knowers: this plurality of immanence lies perplexingly at the very basis of Lossky's theory of perception. Each observer must be suitably placed to see the eagle, and must observe it at the moment of flight. Ten years after, when these hundred knowers meet together in a place far removed from the original site of the event, they may remember their common experience. They may remember it singly in different places and at different times. If the original event is present in each remembrance and in judgments based upon them, Lossky is obviously compelled, since "it seems impossible for an event separated from the knowing subject by an interval of space and time to be present in his acts of judgment," to affirm that "ontology must therefore construct such a theory of space and time as would dissipate the seeming impossibility."[4] Even if ontology does succeed in harmonising the call made by the original event upon the observer to be appropriately placed in time and space, with its subsequent independence of them, there still remain further difficulties.

[1] *The Intuitive Basis of Knowledge* (Duddington's trans.), pp. 99–100.
[2] *Ibid.*, p. 339. [3] *Ibid.*, p. 272. [4] *Ibid.*, p. 274.

Lossky's appeal to ontology for help admits the seriousness of his epistemological trouble. If his difficulty with time and space is resolved for him, he has still to explain one very important difference between remembering a perceived object and reperceiving it. The memory image cannot be further differentiated; the object can be further differentiated if the knower again becomes its percipient. An observer casually perceives a wayside cross without noticing a small red mark at its foot. Repeated inspection of the memory image leaves the mark still unobserved and undifferentiated. Another look at the cross may instantly reveal it. "Reiterated differentiation of objects," to which Lossky ascribes the "clearing away of falsity,"[1] is possible in perceiving and reperceiving objects in a way in which it is not possible by reference in memory.

Our general mental reaction in actual perception is too different from our general mental reaction in memory to allow of Lossky's homogenisation of percept and memory image. Memory images impressed upon minds by objects external to the knower explain, far more simply and consistently than Lossky's intuitive apprehension of immanent objects, the familiar phenomena of perception and memory. One object or event may affect many knowers, as one candle can shine on many mirrors. One object unified with many subjects is a much less intelligible conception: many percipients, it should be remembered, can perceive the same object without any awareness of one another's percipience. The requirement of appropriate situation for percipience is intelligible without requiring Lossky's drastic ontological revision of space and time. The memory image discredits the perceptual object's claim to immanence in knowledge by its own greater claim on that rôle. As a mental method of reference to the original event or object, the memory image impressed by the original perception is naturally independent of locality or time. As a register of differentiation achieved during perception of the object, the memory image is also naturally unpermissive of further differentiation that can only be secured by a repercipience, subject, like the original percipience, to strict conditions for the situation of the knower.

RECENT WORK ON THE INFLUENCE OF THE DUCTLESS GLANDS UPON AMPHIBIAN METAMORPHOSIS (Lancelot T. Hogben, M.A., B.Sc.)

§ 1

While research in many fields has been necessarily curtailed by the events of the past six years, considerable progress has been made in the study of the endocrine organs. One noteworthy advance is due to Kendall (1918), who has isolated thyroxin, the physiologically active iodine compound present in the thyroid gland; and has established its constitution as trihydroxy-triodo-oxy β-indole propionic acid. On the experimental side, several new lines of inquiry have been opened up by the discovery of Gudernatsch (1914) that it is possible to accelerate metamorphosis in tadpoles by means of a diet of mammalian thyroid. The peculiarly favourable conditions of experimentation afforded by the study of amphibian metamorphosis have naturally given an impetus to the investigation of the influence of the ductless glands along these lines.

Gudernatsch's method of inducing a precocious transition from the larval to the adult condition in the case of frog and toad tadpoles has been abundantly confirmed by the work of Morse, West, Barthelemez, Lenhart, and Swingle. In Swingle's experiments the metamorphosis of tadpoles of *Rana catesbiana*, a species which normally requires two or three seasons to attain the adult state, was accomplished, by means of a thyroid diet, within a single

[1] *The Intuitive Basis of Knowledge* (Duddington's trans.), p. 230.

month. Subsequent investigation by the same worker established that similar results could be obtained by the use of iodine free of organic combination, and that, administered in dilute solution added to the culture or in elemental form mixed with the food, iodine itself is more effective for this purpose than any of its compounds.

Such experiments, although suggestive, do not of themselves yield critical data respecting the functional activity of the thyroid gland in the organism to which it belongs. A basis for more extensive inquiry was afforded by Bennet Allen's success (1918) in perfecting the technique of thyroidectomy in very young tadpoles, followed by similar work on the part of Terry and E. R. and M. M. Hoskins. These investigators have demonstrated the complete inhibition of metamorphosis consequent upon the removal of the gland. And it appears that, while total absence of the thyroid results in a cessation of somatic differentiation, it is not found to hinder continued growth in size or to retard the development of the gonads. Swingle has made use of Allen's material to obtain further knowledge of the *modus operandi* of the thyroid diet method of enforcing metamorphosis prematurely. By feeding thyroidless tadpoles of Bufo with a diet containing iodine crystals, he finds that they are capable of completing the process of somatic differentiation. Histological examination, moreover, according to Swingle, reveals that in normal tadpoles fed with iodine the glands are of greater size and the follicular colloidal masses enlarged.

It is possible to distinguish at least two types of endocrine organs: in the first place, such an organ may function by simply setting free hormones secreted by its own activity, as in the case of the suprarenal medulla; secondly, in considering the controlling influence of the gonads on secondary sex characters, Geoffrey Smith's work indicates that there is another type of hormone-producing organ, one which exercises its influence in a less direct and autonomous manner by causing, through its own metabolic activities, the production elsewhere of those substances which supply the characteristic stimulus. In the first case, feeding with the gland or its active principle proves an effective antidote for absence or removal; and it is usually assumed that the thyroid conforms to this category. Swingle's experiments lead to the conclusion that, as far as the thyroid is concerned with Amphibian metamorphosis, it does not function strictly in either of the senses defined above. The relevant facts are—(i) that the thyroid normally contains iodine, and that its iodine content can be increased by including iodine or its compounds in the diet; (ii) that the administration of iodine produces a precocious metamorphosis in normal larvæ; and (iii) that without the administration of iodine thyroidless larvæ are incapable of undergoing a normal metamorphosis. These data, considered collectively, point very strongly to Swingle's conclusion that, in relation to metamorphosis, the thyroid functions as a storage organ, extracting iodine from the blood and retaining it for future use.

§ 2

In accepting this view, it is essential to appreciate clearly that the term "metamorphosis" is used by these authors in a more restricted sense than its customary connotation, to apply specifically to the loss of such larval features as the gills, tail or tail fin. Swingle, at an early stage in his experiments, realised that the development of all the larval organs is not accelerated to the same extent by the methods he employed; in particular, the gonads are unaffected by the procedure of thyroid or iodine feeding. Entirely in unison with his observations are those of Bennet Allen (1918) and of E. R. and M. M. Hoskins (1919), who were able to rear abnormally large and neotenous (i.e. with sexually mature gonads) tadpoles in their thyroidectomised cultures. Swingle has also recorded the relatively shorter length of the gut in his precocious frogs; and he advocates the hypothesis that

Amphibian metamorphosis is due to the interaction of different environmental agencies, such as the iodine content of the medium, with the genetic factors controlling growth. This opinion has been warmly supported by Uhlenhuth, whose work has dealt chiefly with salamanders. Uhlenhuth (1919) finds that the shedding of gills and larval skin are synchronous, and depend upon the presence of iodine: by varying conditions of temperature, the assumption of adult coloration and the onset of sexual maturity can be produced before or after the actual metamorphosis, while the development of tongue and palatal teeth may be checked altogether. He deduces inconclusively that different chemical factors are involved in the development of different series of larval organs; indeed, both Swingle and Uhlenhuth appear to entertain the idea that such factors are independently related to the environment, and not directly co-ordinated by the organism as a whole. But the evidence does not necessarily imply either: adopting the hypothesis of specific activators in somatic differentiation, it is plausible to think of them as individual products of a continuous chain of processes: the amount of any one available at a given moment, and the effect produced in consequence, depend on mass action conditioned by the effect of temperature, among other things, upon the relative velocities of the component reactions. How far such factors are interrelated or independently derived from the environment or the activities of internal organs cannot at present be decided without exceeding the limits of legitimate inference.

In this connection it is instructive to note that apparently similar and normal metamorphic changes can be induced by methods which are not easy at present to correlate with Swingle's results. D. I. Macht (1919) has recently claimed that frogs undergo metamorphosis precociously with prostate feeding. Possibly, in this case, the effect is produced by causing the thyroid to discharge its contents into the vascular system. However that may be, more serious difficulties are raised by the case of the normally neotenous Mexican salamander. Marie de Chauvin and Boulenger's experiments clearly demonstrate that metamorphosis can be enforced in Axolotls, that would not otherwise have transformed, if they are forced to breathe air. That this has any direct connection with thyroid function there is no evidence whatever. Laufberger (1913) and Jensen (1917) succeeded in enforcing the metamorphosis of large Axolotls by thyroid feeding, and this result has been recently confirmed by Huxley (1920) and Hogben, who have found iodine also instrumental in producing metamorphic changes. In such animals no deviation from the normal type of metamorphosis has been observed as the result of thyroid feeding.

§ 3

Side by side with such investigations as those referred to, attention has naturally been directed to the influence of other ductless glands upon metamorphosis in the Amphibia. Concerning the pituitary, there is little matter of interest to record. On the other hand, the researches of Bennet Allen and Uhlenhuth into the activities of the thymus and parathyroids are interesting, and reopen the question of the reputed endocrine function of the former. The author first named confirms Hammar's earlier work, denying that the extirpation of the thymus affects metamorphosis or has any influence on growth and internal differentiation of Bufo larvæ. In accordance with this work, Uhlenhuth submits evidence that the inhibitory effects described by other authors as accompanying thymus feeding are due to a deficiency of diet, which, appropriately balanced, can be rectified so as to give completely negative results. Such larvæ, fed on thymus, frequently exhibit tetanic symptoms, when they reach the stage at which that organ is fully developed: addition of sufficient ordinary diet to induce metamorphosis does not remove the symptoms; but they cease to appear at the

time when the parathyroids attain their functional condition. Since tetany can only be produced by thymus feeding in larvæ, of which the parathyroids are still rudimentary, Uhlenhuth advances the suggestion that there is a reciprocal relation between the parathyroids and thymus activities: that the former absorb a tetany toxin manufactured by the latter. This would explain the tetanic consequences of parathyroid removal in Mammals; but the considerations adduced are not as yet adequate.

Several workers studying Amphibian metamorphosis have supplied minor data dealing with the interrelationship of the endocrine organs. Both hyperpituitarism (Hoskins) and hypertrophy of the parathyroids (Allen) appear to result from thyroidectomy of the larva. One further contribution merits reference in conclusion, that of McCurd and F. Allen, who studied the effect of feeding with pineal gland on the metamorphosis of tadpoles. They supply evidence that the administration of the gland or its extract is accompanied by striking temporary changes in coloration by influencing the melanophores to contract. There is reason to believe that the reaction of the melanophores is partly under the control of stimuli received by the organs of vision; and bearing in mind the archaic function of the pineal as an eye structure, as also the close connection between the physiological effects of the suprarenal cortex and medulla respectively on the organs in association with which they arise in autogeny, it will be exceedingly interesting if this result is confirmed by subsequent work.

VERIFIABLE KNOWLEDGE[1] (George Shann)

THE ultimate function of verifiable knowledge is to afford guidance in action; for which end forecasts of the future must be framed on the basis of knowledge drawn from experience of the past. Such forecasts can be made only on the supposition that certain sequences of change which have occurred previously will recur again in the same order as before.

It is possible to make forecasts about the course of a sequence even when its elements are looked upon as isolated events, without other mental association than the remembrance that they have occurred in a certain order. In the present day, however, such empirical knowledge is felt to be insufficient, and forecasts are hardly considered to be more than probabilities until the sequences from which they are drawn have been hypothetically arranged in a continuous series, or process. Moreover, a prime desideratum of any such mental arrangement is that it should resemble other processes already imagined to be the connections underlying more familiar series of changes, and hence regarded as known processes. When these conditions are fulfilled, forecasts can be made that, if the newly observed sequence does, throughout its course, follow the lines of the known process, then certain events, not hitherto associated with the sequence, may be expected. The verification of a new hypothesis depends upon the success or failure of such forecasts.

Among all processes known to us, the most familiar are change of place (motion) and change of motion (acceleration). Sequences of change which can be satisfactorily arranged in terms of these are held to be thoroughly understood; not because one series is more intelligible than another, but because the analogous series are such as are frequently met with and can be continuously observed, so that very accurate forecasts can be drawn from them. The successive appearances of the planets have been compared with the motion of a heavy body toward the earth, and are therefore said to be explained, although there is no explaining of this latter motion.

So far as verifiable knowledge has been systematised, it consists entirely

[1] A short paper on the subject appeared in the April number of this magazine.

of hypotheses framed and tested in the manner described; it is therefore a knowledge concerned solely with processes of change. Can we, then, be said to know the nature of *things*? The answer is, that we know this in the same sense as we know the processes; for we are unable to form ideas of change sufficiently definite to serve as a basis of forecast, without presupposing some *thing* which undergoes the change, and we are thus constrained to include this as an accessory to our hypothesis of the process of change. To every such *thing* we attribute properties adapting it to the conditions of the hypothesis, and these attributed properties constitute what we know of its nature. Thus our knowledge of *things* is bound up with the same hypotheses as our knowledge of processes; the one is essential to the other, and both are equally certain, or equally uncertain. A logical priority attaches to *things* because their existence is supposed as a condition precedent to their changes; but it should be remembered that in relation to knowledge the order is reversed, since it is experience of changes which gives rise to ideas of something undergoing change.

Now, knowing is in itself a process, and here, as in other cases, if definite forecasts are to be made, a basis must be provided for calculation by supposing something which undergoes the changes in that process. Traditionally this has been regarded as something entirely distinct from matter; it was called the "Mind," and was credited with any properties or "faculties" which would serve for the classification of such changes as were considered to be purely mental.

This hypothesis has, however, two great disadvantages; in the first place, although the mind cannot be supposed to be entirely independent of the functions performed by the body in sensation, yet no satisfactory theory was ever found for the connection between the material body and the non-material Mind; and secondly, since the processes of a non-material entity could not be paralleled with any previously known process, the hypotheses concerning them could not be subjected to verification by the method applicable to other systematised knowledge. These difficulties vanish if we regard both sensation and the other changes involved in the process of knowing as being equally functions of the nervous system; a view which seems to accord with all that I have been able to learn of modern advances in nervous physiology.

Taking, then, the nervous system as the *thing* which undergoes the physical changes whose subjective, or mental, aspect is *knowing*, the following theory may be framed.

The general function of the system is to respond to stimulus by a disturbance of equilibrium and a liberation of energy which affects the whole organism in some degree, but discharges itself mainly along definite lines of least resistance. For the most part these lines follow the paths of previous discharges, since each discharge records its passage by leaving its nerve-track in a condition of reduced resistance. The reduction is a maximum during the actual passage of the discharge; afterwards there is some return toward the original resistance, though this is not always completely reinstated during life.

A nervous track, once marked out, will naturally have its resistance further reduced if the discharges along it be repeated, and this is one way in which tracks become permanent; but the same condition may result from a single strong discharge, especially in the case of immature organisms, since these take the impress of a discharge more deeply than others.

When two stimuli act simultaneously, each liberates energy along its own track, and in so doing must lessen the resistance of the track and thus open a path for energy liberated by the other stimulus. The consequence is that the two become blended in consciousness and are perceived as parts

of a single whole. The result of the connection is that, while its traces last, a repetition of one of the stimuli will liberate energy along the paths of both reactions, and the mental association will be preserved; in other words, the reaction to the stimulus which is repeated will include the memory of the unrepeated stimulus by which it was previously accompanied. The oftener the two stimuli occur together, the more intimate will be the mental association; thus the sight of a familiar surface at once brings to mind the idea of what it feels like.

Again, the response to a stimulus occupies an appreciable interval of time, during the whole of which the maximum reduction of resistance in its track is maintained. If, then, a second stimulus should act before the reaction to the former one is completed, the reduction of resistance due to the first stimulus may allow energy liberated by the second to accompany the uncompleted discharge, making thereby a connection in consciousness between the two reactions. After the connection has been made, a repeated discharge along the track of the first reaction might again carry with it some discharge from the track of the second, and thus the former would revive the memory of the latter. A repetition of the second discharge does not so easily bring back the memory of the first; for instance, it is much easier to recall the spelling of a word than to remember how the letters come in the reverse order.

Since nervous connections are part of, and persist together with, the tracks connected, any one track may be linked up with any other; for if track A be connected on one occasion with track B, and B on another occasion with C, then a link is made between A and C; apparently the association between them may afterwards cease to be dependent on B. As experience widens, the choice among the possible lines of mental association thus becomes practically unlimited; in thought that is concerned with knowing, the selection of a particular line takes the form of framing an hypothesis.

A sensory nerve is directly connected with two distinct parts of the brain, the thalamus and the cortex. There is reason to think that the mental associations here attributed to connections among the different nerve-tracks are effected mainly, if not solely, through the cortex, while the thalamus does little else than receive the immediate sense impressions from the stimuli. The simultaneous reactions of these two parts of the brain are intimately blended in consciousness, but may be mentally discriminated by analysis.

This points to the solution of the dilemma stated by Hume as follows: "There are two principles which I cannot render consistent, neither is it in my power to renounce either of them, viz.: that all our distinct perceptions are distinct existences, and that the mind never perceives any real relation among distinct existences." (Essay 39, § 7.)

Here the "distinct existences" are evidently the immediate sensory impressions—the reactions of the thalamus to stimuli—analytically distinguished from the concomitant response of the rest of the system. The "real connection" is therefore that supplied by the nature of the nervous system, through which the ideas arising from stimuli are presented, not in isolation, but combined with reactions to other simultaneous stimuli and with memories of previous experiences, all blended into an integral whole. On meeting a friend, how little attention do we pay to the sensuous impressions—the response to the visual stimuli of his appearance—in comparison with the complex of sensation and memory which is recognition! The analysis by which the elements originally blended in consciousness are mentally separated is effected by selecting for attention first one, and then another, feature of the complex reaction.

Experience of the capacity for attention suggests that the resistances determining the course of the discharge of energy along particular nerve-

tracks must be more or less subject to voluntary control. Apparently the resistance along any one or two tracks can be temporarily reduced, but, since we are unable to attend to many things at once, it would seem that the resistance of all other tracks must, for the time, be automatically increased.

Everyone is aware that sensuous impressions usually continue to be received even while the attention is engrossed with other ideas; an experience which might be expected if the reactions of the thalamus are not completely controlled by those of the cortex. Their independence would also be a reason for sensations, however faint, being felt to differ in kind from the most vivid recollections; the thalamus co-operating with the cortex only in direct sensation.

It has already been shown that reactions may be almost as intimately blended when the stimuli follow one another closely, as when they are simultaneous. In consequence of this blending of consecutive reactions, changes are not felt as isolated events, but are presented in consciousness as stages in continuous processes; the change from one observed state to the next being supposed to pass through every intervening state. Thus, if a body be observed first in one position and immediately afterwards in another, it is supposed to have occupied successively all the intermediate positions.

The idea of continuity can be extended by means of appropriate hypotheses, and thus applied to sequences of change in which the stimuli are not all closely consecutive; it is then very helpful in the making of forecasts. The method of extension is to suppose that the discontinuously observed sequence exhibits stages in a series which, if its whole course were traced, would be found to follow the same lines as some continuous process already familiar. Astronomy gives the classic example of this method, the intermittent observations of the motions of the heavenly bodies having been paralleled with the more continuously observed motions of terrestrial bodies, and successful forecasts having been framed upon the hypothesis of their similarity. The whole system of science depends on applications of the same method; each newly observed sequence being hypothetically paralleled with some series already regarded as a known process, and the hypothesis being verified by means of forecasts.

In the present theory, comparison is made with more than one known process. The distribution of the discharges of energy is likened to that of discharges of electricity through a system of conductors, where the current is inversely proportional to the resistance of the path. The discharges themselves are supposed to be comparable with those in a pyrotechnic set-piece, where communication is made between the different parts by tubes filled with gunpowder, and the firework is activated by the successive liberation of potential energy at one point after another in a tube. The parallel, however, is not exact, since the single discharge of the firework brings the whole of the gunpowder to a condition of chemical stability, without further power of action; whereas a disturbance of nervous equilibrium, while reducing part of the material along its track to a stable condition, yet renders the equilibrium of the remaining material even less resistant to disturbance than before, and thus facilitates the passage of subsequent discharges of energy. Nevertheless the reserve store of energy is not unlimited; it may be exhausted by a number of rapidly repeated discharges, and the particular track of these discharges, being thus tired out, needs time for recuperation before its activity can be renewed.

The foregoing theory leaves no impassable gap between thought and sensation, between mind and matter; while, by reason of its analogies with known physical processes, the hypotheses involved can be subjected to the usual tests of scientific verification. In both these respects it contrasts favourably with theories constructed on traditional lines.

REVIEWS

MATHEMATICS

The Theory of Plane Curves: Parts I and II. By SURENDRAMOHAN GANGULI, M.Sc., Lecturer in Pure Mathematics, University of Calcutta. [Part I, pp. x + 138; Part II, pp. xiii + 350; with diagrams.] (Calcutta: University Press, 1919.)

THESE two small books comprise a set of lectures on the "Theory of Plane Curves," delivered to post-graduate students in the University of Calcutta. They are intended as an introductory course simply, and consequently little beyond the general theorems of the Calculus and the simpler results in analytical geometry are assumed. Much use has been made of the standard works of Salmon and Scott, but the interesting novelty of the present treatment of plane curves is the introduction of geometrical methods in many cases where those of analysis have formerly been generally used. The author states that this scheme has been introduced to avoid otherwise tedious and lengthy investigations. Various criticisms of this procedure might be made. These books are expressly designed to follow on an elementary course on the theory of plane curves. But even an elementary course must necessarily introduce the fundamental ideas involved in the subject. In general it seems to be better to allow familiarity with the rough outline of a notion to prompt students to a further inquiry into its logical ancestry and relations when interest has been aroused by interesting applications, rather than to endeavour to introduce each logical conception such as the length of a curve or the circular points in a logically unobjectionable way. If the notions are to be intelligently investigated—and this is obviously a different kind of inquiry from the study of the properties of the notion—this must be done at some stage after the introductory one. A book, therefore, which is designed to follow on an elementary course should make some attempt to deal with this aspect, or give good reason why it is to be further postponed. In these lectures no attempt is made, and it is clear, from the *mélange* of geometrical methods and analytical methods, that it was not considered necessary by the author to make such an attempt. As the book is designed for post-graduate students of mathematics, this, on the whole, seems a mistake. But, important as this side is, it is only one part of any branch of mathematics, and it would be a great error to overestimate its value. At some point or other concepts must be used which we cannot further analyse.

The second volume applies the general treatment of curves in vol. i to cubics and quartics. An exhaustive treatise was obviously impossible in the limits of a small book, and the author has singled out various prominent characteristics of the curves to discuss.

DOROTHY WRINCH.

Differential Equations. By H. T. H. PIAGGIO, M.A., D.Sc., Professor of Mathematics, University College, Nottingham. [Pp. xvi + 216 + xxv.] (London: G. Bell & Sons, 1920. Price 12*s*. net.)

THIS book on Differential Equations will be very welcome for teaching purposes. It assumes no previous knowledge of the subject, and covers a

large field. In the introductory chapter use is made of Brodetsky's graphical method, which was expounded last year at the January meeting of the Mathematical Association. In the chapter dealing with linear equations with constant coefficients, the use of operators is explained in a clear and interesting way.

In the chapter on partial differential equations, Fourier's sines and cosines series are introduced to show how certain initial and boundary conditions may be satisfied. The conditions under which the expansion of a function into a set of sines, and into a set of cosines, is valid within the range $(0, \pi)$ are stated. In the chapter dealing with Lagrange's linear partial differential equations, two examples are given from a recent paper of Prof. M. J. M. Hill to illustrate his methods in obtaining special integrals. The method of Frobenius is given great prominence. It is first exhibited in use: then the assumptions involved are investigated. There are a large number of examples in the book, and in many cases notes are appended to the differential equations, mentioning the physical problems in which they occur.

DOROTHY WRINCH

Introductory Mathematical Analysis. By W. P. WEBBER, Ph.D., Assistant Professor of Mathematics in the University of Pittsburgh, and L. C. PLANT, M.Sc., Professor of Mathematics in Michigan Agricultural College. [Pp. v + 300.] (New York: John Wiley & Sons. Price 9*s*. 6*d*. net.)

THIS book is another of the 1919 harvest of American primers for the use of pass students of mathematics. It deals with elementary algebra and trigonometry, and gives as well a certain amount of the theory of conic sections and calculus. It is not to be differentiated in any important respect from the other primers, since it retains that view of complex numbers which relies upon an undiscriminating mixture of geometry and algebra, and is without distinction in its treatment of other topics.

DOROTHY WRINCH.

ASTRONOMY

Report on the Quantum Theory of Spectra. By L. SILBERSTEIN, Ph.D. [Pp. iv + 42.] (London: Adam Hilger, 1920. Price 5*s*. net.)

THE quantum theory of spectra was first put forward by Niels Bohr in 1913, and at once attracted considerable attention owing to the facility with which it led to the formula giving the wave-lengths of the lines in the Balmer series of hydrogen, and to a value of the Rydberg constant occurring in that formula which agreed very closely with the value obtained experimentally. The simple theory advanced by Bohr in his first paper has since been considerably elaborated in a series of papers by Bohr, Epstein, Paschen, Sommerfeld, and others. These developments have provided further triumphs for the theory; in particular, they have enabled an explanation of the fine structure of spectral lines to be given and results to be predicted which subsequently experiment has verified. The papers of Bohr himself have appeared in the *Phil. Mag.*, but the majority of the other papers are contained in the *Annalen der Physik*, and have not been readily accessible to readers in this country. Dr. Silberstein's concise report will therefore be found of great value by spectroscopists and others interested in the theory, presenting as it does a clear and readable account of its present state. The various assumptions which underlie it are plainly stated, and it is carefully pointed out that their only justification at present is to be found in the remarkable agreement with experiment.

The report was written originally for the private use of the optical firm of Messrs. Adam Hilger, Ltd., of 75A, Camden Road, N.W.1, in whose re-

search department Dr. Silberstein has been working. They are the publishers of the present volume. To both author and publishers the thanks of the scientific world are due, and it is to be hoped that this interesting innovation in publishing will meet with success. The quantum theory of spectra is still being rapidly developed, and it is inevitable that before long this report will be out of date. It will remain, however, as a guide to the early development of the theory. We venture to express the hope that it will be found possible, at some subsequent date, to bring out a second edition in which further developments of the theory will be summarised.

H. S. J.

Petit Atlas Céleste. Par G. Bigourdan, Membre du Bureau des Longitudes. [Pp. 54, with 5 star-maps in two colours.] (Paris: Gauthier-Villars & Cie, 1920. Price 2 fr. net + 50 per cent.)

This small volume contains five star-maps, covering the whole sky and showing all stars down to and including the 5th magnitude, and a proportion of those fainter than 5^m but brighter than 5^m5, where the inclusion of these does not produce confusion. The two polar caps down to declivations $\pm 50°$ are presented in two of the maps, whilst the equatorial zone between declinations $\pm 50°$ is presented in three maps, each covering 8 hours of right ascension. They thus contain the brighter naked-eye stars. Unfortunately the maps, which are reprinted from the *Annuaire* of the Bureau des Longitudes, are anything but clear, and are on too small a scale to prove of much value. The figures representing the names of the constellations could have been omitted with some gain in clearness, and, apart from this, they are by no means artistic.

The maps are preceded by a brief but useful introduction, giving a list of the constellations, with their Latin names and French equivalents, and of those whose names are no longer in use; the positions and brightness of the brighter stars in each constellation; and other matter. If better maps were provided, the volume would prove a useful and compact reference-book for beginners in the study of the heavens. H. S. J.

The Sumner Line or Line of Position as an Aid to Navigation. By G. C. Comstock. [Pp. vi + 70, with 6 figures and 2 charts.] (New York: John Wiley & Sons. London: Chapman & Hall, 1919. Price 6*s.* net.)

Blank Reduction Forms for Line of Position Observations, Marcq St. Hilaire Method, with Explanations of Use. By G. C. Comstock. (New York: John Wiley & Sons. London: Chapman & Hall. Price 2*s.* 6*d.* net.)

This small volume is intended to explain the Sumner line method of fixing the position of a ship at sea to readers who already possess some knowledge of navigation. The Sumner line method possesses such great advantages over the older methods that it is surprising that it has not as yet completely displaced them. We believe, in fact, that its use is far from being general. Any volume which will help further to popularise the method is therefore to be welcomed. But we are doubtful whether the present volume will serve this purpose, at any rate on this side of the Atlantic.

The author has not, as might have been inferred from the title, dealt with the general problem of the line of position, but only with the particular method due to Marcq St. Hilaire. Inasmuch, however, as the St. Hilaire method is the best and the one most generally adopted, this cannot be considered a disadvantage. The treatment is straightforward, but is not the simplest that we have seen. Moreover, from the point of view of English

readers, it suffers from two disadvantages. Frequent reference is made throughout to Bowditch (*American Practical Navigator*, published by the U.S. Hydrographic Office); and, although this is doubtless an advantage to American readers, it is the reverse for English readers. Further, the symbols used are not those which have been generally adopted in this country, and their unfamiliarity will undoubtedly be found a drawback.

For these reasons the book can hardly be recommended to students in this country. In addition, a price of 6*s*. net for a book containing only about 70 small pages seems unduly high.

Those readers who decide to follow the author's methods and notation will find the book of blank reduction forms, which is published as a companion to it, of use. The calculations are performed on the front of a printed form, which is adapted for use with either logarithms or spherical traverse table, whilst on the back is a diagram for plotting the position of the Sumner line.

H. S. J.

Space, Time, and Gravitation : an Outline of the General Relativity Theory. By A. S. Eddington, M.A., M.Sc., F.R.S., Plumian Professor of Astronomy and Experimental Philosophy, Cambridge. [Pp. vii + 218, with frontispiece and 18 figures in text.] (Cambridge: at the University Press, 1920. Price 15*s*. net.)

The generalised relativity theory of Einstein has been objected to by its opponents because of the radical revolution which it necessitates in our conceptions of space and time, and in other physical ideas, and because of the difficulty of comprehending the nature of the new " law of gravitation." The advocates of the theory have been challenged to come out into the open and, instead of taking refuge behind a maze of differential invariants, to explain, in a manner which the man in the street can understand, what is involved in the theory. This may not have been the reason which induced Prof. Eddington to write this book, but he has, at any rate, provided an admirable answer to the challenge. If the opponents of generalised relativity will but read the volume with an open mind, it is difficult to believe that they will not be convinced.

The book commences with a prologue entitled " What is Geometry ? " which takes the form of a conversation between an experimental physicist, a pure mathematician, and a relativist: the dependence of the bases of geometry upon physical ideas is made clear, as well as the possibility of conceiving a space with properties differing from those of a Euclidean space. The first four chapters then deal with the foundations of the special relativity theory, the relative significance of the terms " space " and " time," and the simplicity which results from considering time as a fourth dimension. The following three chapters deal with the newer modifications and expound the principles of Einstein's argument without introducing mathematics. Then follow two chapters devoted to the tests of the theory; a careful discussion is given of the evidence afforded by the displacement of the sun's spectral lines, and possible ways of escape, should experiment definitely disprove the displacement, are indicated. Chapter IX deals with the questions of momentum and energy, and it is shown how the principles of conservation of energy and momentum follow from the theory.

The remaining chapters deal with some of the more speculative developments of the theory: such as the significance of absolute rotation, the nature of infinity, and the relationship between electricity and gravitation. The theory of the latter is not yet in a final state, but it holds out remarkable promise of including electrical fields also in the natural geometry in which gravitation finds its explanation.

It may be mentioned that the author does not consider it necessary to abandon the conception of the existence of an æther, although it is difficult to ascertain what rôle is now left for the æther to perform. This attitude may afford some comfort to those to whom the necessity of abandoning the æther is a bar to acceptance of Einstein's theory.

The book is excellently written, and although primarily intended for readers without technical knowledge, it will also be of value to those who are able to go into the mathematics of the subject. To them, Prof. Eddington's commentary on the argument cannot fail to prove illuminating. The style is admirable throughout, except for the final paragraph in the book. ("We have found a strange footprint on the shores of the unknown. We have devised profound theories, one after another, to account for its origin. At last, we have succeeded in reconstructing the creature that made the footprint. And lo! it is our own.") We hope that this last sentence will be altered in a future edition.

H. S. J.

PHYSICS

A Handbook of Physics Measurements. By ERVIN S. FERRY, in collaboration with O. W. SILVEY, G. W. SHERMAN jr., and D. C. DUNCAN. [Vol. i, pp. xi + 251, with 146 figures. Vol. ii, pp. x + 233, with 128 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1918. Price 9*s.* 6*d.* net each volume.)

OF these two volumes, the first deals with fundamental measurements, properties of matter, and optics; and the second with vibratory motion, sound, heat, electricity, and magnetism; and in the two volumes 108 experiments in all are described.

There are several features of value. The work is self-contained; each section commences with a general account of the instruments and methods utilised in the subsequent experiments, and each individual experiment consists of an explanation of its theory followed by an account of the method of performing the experiment. This insures the student thoroughly understanding from the beginning what the experiment is designed to prove, and how it does so. The most important sources of error are pointed out, and means are indicated by which the errors can be eliminated, reduced to a minimum, or allowed for. It is very necessary that students should realise why, *e.g.*, Joule's determination of the mechanical equivalence of heat must possess a much greater weight than their own.

The experiments have been well selected on the whole: both in the selection and the method of treatment, the authors' extensive teaching experience is evidenced. Apparatus of standard type has, in general, been described, and where improvised apparatus is used, it will be good for the student to construct it himself. More can be learnt from self-constructed rough apparatus, which can necessarily only give a relatively inaccurate result, than from expensive and delicate apparatus the facility of using which to its full advantage the student has not acquired.

The two volumes should prove very valuable to those responsible for preparing courses in practical physics.

H. S. J.

Relativity: The Special and the General Theory. A Popular Exposition. By ALBERT EINSTEIN, Ph.D., Professor of Physics in the University of Berlin. Authorised translation by R. W. LAWSON, D.Sc. [Pp. xiii + 138, with 5 diagrams and a portrait of the author.] (London: Methuen & Co., 1920. Price 5*s.* net.)

THIS account of the special and general theories of relativity by Prof. Einstein himself can be recommended, particularly to those who, whilst not having had

a mathematical or scientific training, are desirous of obtaining an insight into the principles upon which the theory is based, and of understanding its bearings in the realms of science and philosophy. The book has been written for the average reader, and the author has spared no pains to put the ideas into a simple form, many homely illustrations being used to make the point of an argument clear.

About one-half of the book is occupied with the special theory, and the modifications which it necessitated in our ideas of space and time. Then follows an account of the general theory, involving as a consequence the solution of the problem of gravitation. A brief account is also given of the bearing of the theory on the general question of the structure of space.

Prof. Einstein is careful to point out in his preface that the "work presumes . . . a fair amount of patience and force of will on the part of the reader." The average reader must be prepared to find some of the conceptions difficult to grasp at first, and will be advised to reread the book in order thoroughly to understand it. There is certainly no other work available covering the same extent of ground in such an elementary manner. Dr. Lawson is to be congratulated on the service he has rendered by making Prof. Einstein's account available in English in this excellent translation.

H. S. J.

CHEMISTRY

Cours de Chimie. Par R. de Forcrand, Professeur de la Faculté des Sciences ; Directeur de l'Institut de Chimie de l'Université de Montpelier. [Vol. i, pp. viii + 437 ; vol. ii, pp. 527.] (Paris : Gauthiers-Villars & Cie, 1918–19.)

In its scope and general arrangement, this textbook of chemistry for students differs considerably from the more usual type of textbook with which we are familiar on this side of the Channel.

Vol. i is divided into two main sections, "Generalités" and "Chimie Minérale," whilst vol. ii deals with "Chimie Organique," "Chimie Analytique," and "Applications Numériques," so that a student who has been through the entire course will have a very considerable knowledge of the fundamental principles of the subject. It is, of course, somewhat unusual to include both the theoretical and practical sides of Chemistry within the limits of the same book, but it has the advantage of compactness.

The arrangement also of the section on Organic Chemistry does not follow the usual plan of dividing the subject into the aliphatic and the aromatic divisions and so on, but the whole section is split up into some nineteen chapters divided according to the "Fonctions" dealt with ; thus the "Septième fonction, Cétones," deals with acetone, camphor, sugars, anthraquinone, cellulose, and alizarin, a fairly mixed batch !

This method has, of course, certain obvious advantages, but probably English readers will prefer the more usual arrangement of dividing into aliphatic and aromatic groups, and then further subdividing according to the number of carbon atoms.

The textbook suffers from the usual failing of French works in having no index other than the "Table des Matières" at the end, but is worth examination as a variant from the usual run of books on chemistry.

F. A. M.

Histoire de la Chimie. Par Maurice Delacre, Membre de l'Académie Royale de Belgique, Professeur a l'Université de Gand. Ouvrage couronne par l'Institut de France. (Prix Binoux.) [Pp. xvi + 632.] (Paris : Gauthier-Villars & Cie, 1920.)

We have not a few histories of chemistry from Thomson's *History* to Kopp's *Geschichte* and onwards, but Prof. Delacre has written a book which pos-

sesses certain very definite features of its own which distinguishes it from most of its predecessors. In passing, it may be noted that the work was completed at Ghent in October 1916, during the German occupation, and only the good services of Cardinal Mercier, which are duly acknowledged, enabled the manuscript to find its way to Paris, where in due course it was awarded the Prix Binoux of the Institut de France.

The object aimed at by the author has been to write a more or less objective history of Chemistry—or, as he terms it, "Histoire de la Chimie positiv"—not dealing so much with a mere chronological statement of chemists, discoveries, and the rise and fall of theories, as endeavouring to trace the historical development of various matters which may now be regarded as more or less definitely settled; in particular our present table of atomic weights and the constitution of organic compounds.

For this reason the work may appear to be rather one-sided in certain respects, and the zeal of the author is reflected in the copious—perhaps too copious—extracts from original documents which fill the pages of the book.

One has the feeling, whilst reading, that one is perusing some technical legal work requiring documentary proof at every step, and there is also the suspicion that Prof. Delacre has sometimes overwhelmed himself in a flood of extracts and quotations, so that at the end it is not always clear just what conclusions he has drawn from his evidence, or whether, indeed, he has reached any definite conclusions at all.

In drawing attention to this obvious defect due to the general treatment adopted, it is not intended to minimise in any way the value of what is obviously a careful and painstaking work, which should prove a most valuable source of information for those chemists who may desire to study in detail the historical development of their science, though the absence of a subject index diminishes somewhat its value as a book of reference.

F. A. M.

Chemistry in Everyday Life (Opportunities in Chemistry). By ELLWOOD HENDRICK. [Pp. xii + 102.] (London: Hodder & Stoughton, 1919. Price 3*s*. 6*d*. net.)

CHEMISTRY is fortunate enough in having so delightful an interpreter as Mr. Ellwood Hendrick, who, besides being the author of *Everyman's Chemistry*, is President of the American Chemists' Club amongst his other activities, and is thus in the centre of things, and able to keep his eye both on the latest developments of chemical science and on the trend of popular interest.

In his earlier book he leapt into fame largely owing to the fresh, untrammelled view he took of Chemistry in its relation to everyday life, and in the present little work he endeavours to show how closely chemistry touches all our human activities at a hundred points. A glance through the book makes even a hardened chemist look at things from a new point of view. Whilst Mr. Hendrick does his best to show the man in the street how much he owes to chemistry, he does not place the chemist on a pedestal; for instance, at the end of Chapter IV, on "The Great Cycle of Nature," dealing with fuel and plant growth, he writes: "Whenever we get to fancying that we are as clever, able, or as wise as anybody need be, it is worth while to take a glance at a tree, at any old tree, even the scraggly backyard runt with the wash hung out on it to dry, and say to ourselves that, when we can turn the little trick that that tree does daily, it will be plenty of time to perk up and grow chesty!"

There is such a cheerful fresh atmosphere about Mr. Hendrick's writing that one thinks of him as a young man of an inquiring turn of mind rather than as "grey-headed and fat," as he describes himself. Anyway, a man

is as young as he feels, and certainly the author of *Chemistry in Everyday Life* must feel somewhere in the early twenties. The book shows some signs of similarity to *Everyman's Chemistry*, but is none the worse for that.

F. A. M.

A Textbook of Organic Chemistry. By E. DE BARRY BARNETT, B.Sc., A.I.C. [Pp. xii + 380, with 15 illustrations.] (London: J. and A. Churchill, 1920. Price 15*s*. net.)

YET another short textbook on organic chemistry following more or less the lines of most other similar books on the subject. The chief claim to originality appears to be the inclusion of a "short account of the Richter system of indexing, and a very brief mention of a few of the standard works of reference," with a view to providing chemists with "the knowledge of where to find information," since, in the author's experience, "many chemists are woefully ignorant of the literature of their science."

In the ten pages devoted to carbohydrates, we look in vain for any mention of the lactone formula for glucose, or of the α and β modifications of this substance; similarly, the subject of mutarotation is not mentioned, and glucosides likewise are omitted from the book. The short chapter devoted to Purins and Alkaloids is also disappointing.

P. H.

BOTANY AND AGRICULTURE

The Life and Work of Sir Jagadis C. Bose, M.A., F.R.S. By PATRICK GEDDES. [Pp. xii + 259, with portraits and illustrations.] (London: Longmans, Green & Co. Price 16*s*. net.)

PROFESSOR BOSE was born in Eastern Bengal and educated at St. Xavier's College, Calcutta. From there he came to London, with the intention of entering the medical profession. His health not allowing of the fulfilment of the scheme, he went on to Cambridge, and was trained there in natural science. He began teaching Physics in an Indian University, and his first researches, very interesting and important ones, are in connection with this subject.

The last of these investigations led him to note certain peculiarities of inanimate objects, such as the metals, which suggested comparisons with the behaviour of living beings on stimulation. The intrinsic interest of these discoveries, and probably partly the difficulty which Bose found in getting a ready acceptance for the explanations which he put forward, gradually brought his studies more and more from the physical to the physiological. His physical training, and the fact that he was accustomed to measuring various constants with accuracy, showed him the need, as indeed it had showed others, of tackling physiological phenomena with more delicate instruments than had hitherto been the case, instruments not subject to such gross limitations as the human senses, that were so commonly used as recorders. Sir Jagadis not only perceived this need, but he possessed what is indeed a rare gift, the inventive powers necessary to produce such instruments, and the infinite patience which enabled him to wait, for years in some cases, until the inspiration necessary for the completion of some particular instrument, or part of an instrument, came to him.

Professor Geddes describes shortly these products of inventive genius, the optical lever, the resonant recorder, and the various crescographs. He shows that their uses are not necessarily limited to the particular fields of activity in which Bose works, that the principle of the optical lever, for example, has been used in the Cambridge Botanical Laboratory for the elucidation of quite a different problem.

A general account is given of the researches on movement and growth, and the author succeeds in doing what he had intended, namely, effectively, but not exhaustively, covering all the ground. He gives an accurate and clear account of the results achieved, but does not seem so happy in his attempts to connect these interpretations with those of other workers in the field. For example, the results of observations on the "Praying Palm" are given, and we are told that similar movements have been observed by Professor Bose "in all trees and their branches and leaves"; but no reference is made to the large amount of scattered literature, dating from about 1812, on the subject of the connection between temperature and the movements of branches; and on p. 176 there is confusion of thought between perception and response, both in the case of gravity and of light.

The importance of Sir Jagadis Bose's work is now generally admitted, and as recently as May of this year he was elected to a Fellowship of the Royal Society. In 1917 he saw the dream of his life realised on the opening of the Bose Institute. Two important volumes of botanical researches have already been published, and we are told that in the near future other branches of learning will receive attention from the workers in the institute. The biography, if not a great one, is at least very competent and reliable.

E. M. C.

New Zealand Plants and Their Story. By PROF. L. COCKAYNE, Ph.D., F.L.S., F.R.S. New Zealand Board of Science and Art Manual, No. 1. [Pp. xv + 248, with 99 photographic illustrations and 14 text-figures.] (Wellington, 1919.)

THE Flora of New Zealand is one of the most interesting of the island Floras of the world, of which no one is more competent to tell the story than Prof. Cockayne. Like all areas isolated by natural barriers, that prevent or retard the gradual process of dispersal, New Zealand possesses a large proportion of endemic species, some of which—as, for example, *Pittosporum obcordatum*—are amongst the world's rarest plants. Of the vascular plants, the endemics comprise 74 per cent., whilst amongst the Dicotyledons and Conifers together their proportion rises to 85 per cent.

Much has been said and written with regard to the success of the introduced species, of which no less than 520 have become securely established. But the author strongly emphasises the fact that it is only where the interference of man has modified the conditions of the habitat that there is any evidence of the success in competition with the native Flora to which so much prominence has been given. "With but one trifling exception, no truly primitive plant-formation is desecrated by a single foreign invader."

It is the human factor which has been responsible for so much change, and in particular for the deterioration of the mountain pasture, which now only supports one sheep for every four acres.

The successive chapters describe the various types of vegetation in an attractive manner, unusually free from the assumption of any but the most elementary technical knowledge. Much of the information thus conveyed is of a general character, and equally applicable to our antipodean Flora. The Dune Vegetation, for instance, is essentially similar to our own in its physiognomic character, and even in the specific identity of some of its constituents. *Spinifea* serves as the New Zealand *Psamma*, whilst *Convolvulus soldanella* plays the same rôle as with us. So too *Zostera nana* covers the mud of shallow estuaries along the edge of which occurs the tidal scrub dominated by *Avicennia officinalis*, and salt marshes with *Scirpus americanus*, *Leptocarpus simplex*, *Juncus maritimus*, *V. australiensis*, *Salicomia australis*, etc.

The forests contain a large number of diverse trees, but nevertheless

belong to two main types, viz.: the *Nothofagus* forests of the South with, often pure, stands of one or other species of Evergreen Beech, whilst the Rain forest of the North exhibits a vast assemblage of mostly evergreen trees, in which *Agathis australis*, *Beilschmiedia* spp., *Dacrydium* spp., and *Podocarpus* spp. are prominent. But though so different in composition from our own, they exhibit much the same architecture, built up of trees, shrubs, herbs, climbers, and epiphytes, and the individual species mostly possess dull-coloured and inconspicuous flowers.

Each type of vegetation is made to tell its story, from each there is much of interest to be learnt, and we cannot too highly commend the action of the Minister of Internal Affairs in publishing this volume as a manual of the New Zealand Board of Science and Art at a price within the means of all.

E. J. SALISBURY.

Hydration and Growth. By D. T. MACDOUGAL, Ph.D., LL.D. [Pp. vi + 176, with 52 figures and 124 tables.] (Washington: Carnegie Institution, 1920.)

INCREASE in size, the visible component of growth, is mainly the outcome of absorption and adsorption of water consequent upon the osmotic and imbibitional capacity of the cell units of the organism. As the author of these pages justly observes, our knowledge of the imbibition of colloidal substances is largely based on the behaviour of gelatin, which, being a nitrogenous substance, is comparable rather to the protoplasm of animals rich in nitrogen than to that of plants in which, normally, colloidal carbohydrates preponderate.

Bearing these facts in mind, the author utilised the carbohydrate Agar for the purpose of many of his experiments either alone or with a small proportion of added protein, salts, etc., so as to simulate as far as possible the conditions in a phytocolloid.

The results show that, whereas the maximum imbibition of gelatin and colloids rich in nitrogen is attained in an acid medium, that of colloids in which the proportion of carbohydrates is relatively high is attained in a neutral relatively salt-free condition.

The effect of dilute monobasic amino-compounds was to increase the hydration capacity of pentosan colloids, although, in higher concentrations than those encountered in plants, they produce the reverse effect.

Prepared colloidal plates and plant sections, alike, showed a marked decrease of imbibitional capacity in swamp-water, whilst in bog-water with a higher acidity but lower proportion of salts the swelling was nearly equal to that in distilled water. With colloidal plates containing a high proportion of nitrogen, the swelling in the swamp-water was greater even than in distilled water, and it is suggested that possibly variation, in the relative proportions of carbohydrate and protein colloids, may be one means adopted by plants to meet the specialised conditions of particular types of habitat.

Not the least interesting sections are those in which the author treats of the varying imbibition capacity of sections of *Opuntia* under different conditions of growth and at different times of the day, changes ascribed to the variations in acidity, protein content, the proportion of mineral salts and pentosans. Experiments are described on *Opuntia, Mesembryanthemum, Helianthus*, and *Phaseolus*, from which the author concludes that the fluctuations in growth bear a direct relation to the varying hydration capacity of the growing cells.

These pages provide a valuable addition to our knowledge of imbibition, none the less important even if we feel that the author somewhat overemphasises the rôle of this factor in relation to growth.

E. J. SALISBURY

Forest Products, their Manufacture and Use. By NELSON COURTLANDT BROWN, B.A., M.F., Professor of Forest Utilisation, The New York State College of Forestry at Syracuse University, Syracuse, New York; Trade Commissioner, United States Lumber Trade Commission to Europe, Department of Commerce, Washington, D.C. [Pp. xix+471.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 21*s*. net.)

THIS book gives an introductory account of the principles and practice underlying a number of industries dependent on the use of timber and other products of forest trees. As it was found necessary to limit the scope of the work, certain industries, such as furniture, shipbuilding, and allied industries, are omitted, as they belong to a distinctly different category from those considered. These latter are as follows: wood pulp and paper, tanning materials, veneers, cooperage, and naval stores, the distillation of hardwood and softwood, charcoal, such industries as pole and post and shingle making, maple syrup and sugar, rubber, dye woods, cork, etc.

Although this work deals chiefly with industries in America, where, in spite of much depletion, the forests available for timber are still very large, and where the consumption of timber per head of the population is about fourteen times as much as that consumed per head in this country, yet the book will be read with appreciation by all in this country interested in forestry and economic botany, and the application of science to industry. It is clear that the forests of the world in general, and of this country in particular, are rapidly diminishing, while more and more industries are dependent upon them. That the time cannot be far distant when the importance of forest production will force itself into recognition becomes clear from the large number of industries, described by Prof. Brown, which are dependent on the maintenance of forests.

In spite of its somewhat encyclopædic character, the book retains its interest from the beginning to the end.

W. S.

The Nature Study of Plants in Theory and Practice for the Hobby-Botanist. By THOMAS ALFRED DYMES, F.L.S, with an Introduction by PROF. F. E. WEISS, F.R.S. [Pp. xviii + 173, with frontispiece and 53 illustrations (21 photographs).] (London: Society for Promoting Christian Knowledge, 1920. Price 6*s*. net.)

THIS is an admirable little book, written, as the title-page informs us, for the hobby-botanist, and probably also for teachers of nature study, and children. In the first portion of the book the general facts of Nature Study are reviewed; in the second, special part, a much more detailed account is given of the life-history of the Herb Robert, of which the author has made an intensive study. This is followed by a description of the relatives of this plant and by a comparison with the Storksbill. As Prof. Weiss says, in his introduction, such a detailed study will no doubt prove to many "an incentive to make a personal investigation of the fascinating processes which make for the preservation of the individual and the race." Our knowledge of such facts in regard to even the commoner British plants is all too meagre; the illustrations are almost invariably good, and the style clear and simple.

E. M. C.

Cocoa and Chocolate: their History from Plantation to Consumer. By A. W. KNAPP, B.Sc., F.I.C. [Pp. xii + 210, with numerous illustrations.] (London: Chapman & Hall, 1920. Price 12*s*. 6*d*. net.)

THIS is an attempt to cover, with accuracy, but not with completeness, the whole ground of this important subject, Cocoa, from the preparation of

the ground for planting out to the output of the various manufactured articles and the by-products. A short history is given of the introduction of cocoa, followed by a description of the plant and a short account of the methods of cultivation, which are comparatively simple. It is interesting to note that in Grenada, for example, close planting and a careful attention to the condition of the land enable the cultivator to do without the shade-plants considered, in most other places, to be essential. Wind-screens, in the form of hills and belts of trees, are of course still necessary, and on comparatively flat land, such as the island of Barbados, it is not possible to grow cocoa successfully. The desirability or otherwise of suckers, or "chupons," as they are called locally, is also discussed. The cauliflory, or formation of flowers on the old bark, and the presence of red pigment in the young leaves, noted by the author in the case of this plant, are comparatively common phenomena amongst tropical plants. In Trinidad, experiments have now been some years in progress with the view of finding out whether grafting and budding will enable the planter to get quicker returns than is customary, and also to obtain a surer growth of the delicate Criollo variety by grafting it on the hardy Forastero. The first-named variety gives a better grade cocoa than the commoner Forastero, and is fermented more easily and quickly. So far the experiments have been very successful, and are of importance also in another connection. The plants often vary greatly in their "cropping powers," and this character shows early in the life of the plant and is fixed. It is possible that grafting shoots from prolific plants on to comparatively bad stock would improve the character of the plantation.

The various diseases to which the plant is subject are not treated of, as to have done so adequately would have unduly increased the size of the book. The actual manufacture of the bean into cocoa powder, chocolate, milk chocolate, and creams, is considered more fully than any other part of the subject, and the author, who is Research Chemist to Messrs. Cadbury Bros., is in a position here to give an authoritative account of what proves a very interesting story. The remaining portion of the book is taken up with the economic problems connected with cocoa and with its dietetic value; a full bibliography is appended. The book is addressed to the general reader, is simply written, very well and fully illustrated, and can be recommended to all who wish to obtain a general idea of the problems that the cocoa manufacturer has to face.

E. M. C.

A Course of Practical Chemistry for Agricultural Students, Vol. I. By L. F. Newman, M.A., F.I.C., Fellow of St. Catherine's College, Cambridge, and H. A. D. Neville, M.A., B.Sc., F.I.C., Professor of Agricultural Chemistry, University College, Reading. [Pp. 235.] (Cambridge: at the University Press, 1920. Price 10s. 6d. net.)

The scope of the laboratory manual of which the volume under review forms the first part was described in the pages of this journal when Vol. II, Part I, which appeared some months ago, was noticed. Vol. I includes laboratory exercises which are introductory to the study of chemistry, these exercises being concerned with air, hydrogen, water, carbon dioxide, ammonia, hydrochloric acid and chlorine, acids, bases, and salts. These introductory exercises are followed by sections in which the student is made acquainted with the principles of volumetric analysis. The remainder of the book deals chiefly with the qualitative and quantitative analysis of soils, manures, and plants, and with the physical properties and mechanical analysis of soil. The book concludes with some exercises in elementary physics. Throughout

the authors have borne in mind the difficulties experienced in providing complicated apparatus for large elementary classes, and for this reason the work will be of great use in all practical classes in agricultural chemistry, as the possession of unusual apparatus or apparatus difficult to obtain is never assumed. W. S.

PHYSIOLOGY

Physiology of Farm Animals. By T. B. Wood, C.B.E., M.A., F.R.S., and F. H. A. Marshall, Sc.D. Part 1: General, by F. H. A. Marshall, Sc.D. [Pp. xii + 204, with 105 figures.] (Cambridge: at the University Press, 1920. Price 16*s*. net.)

The old story of the arising of science, from man's early efforts to master the processes of nature, should make us give this book a hearty welcome on the strength of its title, even if a welcome was not assured by the names of the authors. The immediate and obvious benefits to be gained by the practical application of a science bring in a driving force which assists the progress of the general theory; a gain for which the grounds of hope are particularly good when the practical application is an extension of the field, rather than an intensification of the study of some particular branch which has long been put to a useful purpose. A survey of the work already accomplished, and the fields where more work is most urgently required, when presented to fresh minds, such a survey, we can say, may lead to no small contribution to the science of Physiology.

The first volume of the book before us, by Dr. Marshall, treats of the general part of the physiology of domestic animals. It is to be followed by a second volume on animal nutrition, by Prof. Wood. In the introduction to the present volume, the author states that the physiology of farm animals has been much neglected, a statement with which the agriculturalist would probably concur, since he would see that the author, in the discussion of the elementary properties of protoplasm, has to resort to inhabitants of the horse-pond more regularly than the horse. It would be a dangerous experiment to ask a farmer if he was Hydra's keeper. Turning to details, the first three-quarters of the book are pretty general, while the last quarter deals with the generative organs. Of this latter portion all we need say is that the author is Dr. Marshall, so praise would be superfluous, criticism dangerous. The earlier portion, however, requries closer examination. The impression left on the mind, after reading the book, is that the author has given far too much consideration to the man who wishes to add two letters to his name for the minimum expenditure of effort. The elements of the descriptive aspect of physiology receive the lion's share of the space. This information is compact and well arranged, but the chapter on Nerve would have been much improved by a short account of the sympathetic system. The experimental branches of physiology have been cut down very severely. The nerve muscle preparation receives very inadequate treatment. On p. 71, the dissociation curve of oxyhæmoglobin is figured, but surely it would have been lawful to state that it was a rectangular hyperbola—even to give its equation.

The statement that the general form of the curve for carbon dioxide in the blood is similar to that of oxygen puts a brutal strain on the word "general," a strain which it ought not to be called upon to bear. The selection of material is particularly difficult in physiology: no two men would make the same choice; but in the opinion of the reviewer the book would have been improved if the subject had been treated as a growing and not as a finished product.

It is an excellent thing to get a number of people through the examination mill, the "practical application" of most textbooks, but if the gaps

in our knowledge are to be filled, more attention should be drawn to them, and the industrial applications should be kept more clearly in the sight of the reader, as a powerful stimulus in bringing this about. Work which should be done is left, not because it is too difficult, but because no one thinks of doing it. The enthusiasm with which a new subject is begun may easily be wasted unless the student feels there is something for him to do—and the writer of a textbook has a responsibility in directing thought into fields which will bear fruit in due season. Though we admire the many good qualities of the book, we think that, in this respect, it has fallen short of the ideal.

G. S. ADAIR.

What Bird is That? By FRANK M. CHAPMAN, Curator of Birds in the American Museum of Natural History. [Pp. xxvi + 144, with 8 coloured plates and 9 text illustrations.] (New York: D. Appleton & Co. Price $1.25 net)

THIS book is written for beginners in the study of United States birds. It is produced on new lines and is, as far as it goes, extremely useful. The chief feature of the publication lies in the coloured plates, eight in all. Each plate represents a case of museum specimens, each case including all the birds that occur together at certain times of the year. Thus one case shows permanent residents, another winter visitants, and others late and early spring migrants. The student can thus see at a glance all the birds that should be present during any given month by turning up the appropriate cases. As all the birds are drawn to the same scale, the comparative size of a bird as observed by the student can be made accurate use of—a very valuable feature. Unfortunately the book is restricted to land birds found east of the Rockies, and so deals with only a part of the American Avifauna.

The coloured plates are well reproduced from excellent originals. It is a pity, perhaps, that they are so extremely small, but as the author claims, owing to the care in production, no essential details have been lost thereby. The pictures differ from most bird illustrations in the extraordinarily good and characteristic poses given to each species. If the paintings are indeed copies of "stuffed" birds, the originals must be veritable gems of the art of taxidermy. Indeed, considering the restricted scope of the volume, this appears to me to be its most valuable feature, for it converts it from a beginner's book to one that should be in the hands of all bird artists and taxidermists.

The letterpress consists of "labels" for all the birds depicted. Each label gives brief details of colour, distribution, migratory dates, etc. The last-named is a particularly useful feature, as it enables the student to put exact dates to the cases to suit his particular locality, accurate migratory dates being given for at least five divergent centres with each "label."

The few text figures are so poorly printed that they might well have been omitted. The "map" of a bird, giving the correct descriptive terms to the various components of its surface anatomy, should be of value. There is also a good index.

WM. ROWAN.

Action de la Chaleur et du Froid sur l'Activité des êtres Vivants. Par GEORGES MATISSE. [Pp. ii + 556.] (Paris: Emile Larose, 1919.)

THIS is a comprehensive discussion of the effect of different temperatures on living organisms. From the crude test of the extremes of temperatures which may be survived, the author passes on to consider the effect of variation in temperature at moderate temperatures.

There is a marked difference between warm- and cold-blooded animals

The former react to changes in the temperature of their surroundings by altering their heat production and heat loss so as to attempt to maintain a temperature within a narrow range. The latter tend to a temperature approximately that of their surroundings. When the temperature of the surroundings is altered, there is a period of warming or cooling which must be neglected if one is studying the effect of steady temperatures. Further, if the animals are kept at a high temperature for some time, there is a falling off in their activity.

The rates of movement of two species of worms and of a snail were studied. The results were treated from the physico-chemical point of view.

With rising temperature there is an increase in activity up to an optimum above which the activity rapidly decreases. The author's results, as well as those of previous observers, are discussed. The rate of movement probably depends on the rate of a chemical reaction. Therefore it depends on the concentration of reacting substances, on the change of rate with change in temperature, and on any physical modifications of the system caused by the temperature. The first of these is independent of the temperature, but may be affected by the duration of the experiment. The second is expressed by the Law of Van't Hoff and Arrhenius, whilst the last has been studied by examining the tissue fluids by means of the ultramicroscope.

The results show that the optimum is due to the crossing of the curves of increase in activity with rise of temperature and that of the physico-chemical changes in the colloids at high temperatures. The longer an animal is kept at a high temperature, the greater is the latter effect. The three variables—rate, temperature, and time—are expressed geometrically by a surface from which one can read either the variation of rate at the same temperature (isothermal lines) or after equal durations (isochronic lines).

The motor reactions show evidence of four rhythms: (1) A gradual decrease during several days due to a cumulative effect of the temperature; (2) a daily rhythm due to inactivity during darkness, with an accumulation of reserves of material for activity; (3) an hourly or multi-hourly rhythm; (4) a rhythm of short duration probably of the nature of an autocatalytic reaction.

The action of drugs is modified by the temperature. At high temperatures the reaction may be so violent as to kill the animal, but at a low temperature the same dose may be harmless because the reaction is less violent, so that the drug may be excreted without fatal effects.

This book is intended as a first approximation to the intimate mathematical analysis of the effect of temperature on living organisms, and it should be read by all interested in the subject.

H. E. R.

Altitude and Health. By F. F. Roget. [Pp. xii + 186.] (London: Constable & Co., 1919. Price 12*s*.)

This book is concerned with the effect of altitude in relation to the maintenance of health and to the cure of respiratory diseases such as pulmonary tuberculosis. It is divided into three parts: namely, I, Climate (Alpine and Northern); II, The Air at Altitudes; III, Sunlight and Sunheat. These three represent three physical conditions associated with high altitudes, which are a fall in temperature, a decrease in atmospheric pressure, and an increase in the radiant energy of the sun.

Low temperatures can be reached by going to high latitudes, but this fall in temperature is not associated with a decrease in atmospheric pressure. In addition, at high latitudes the radiant energy of the sun is less because the rays pass through a thicker layer of atmosphere, which absorbs heat and ultraviolet rays.

The cold at high latitudes is more uniform, there being very little sun in winter; and even in summer, although the sun shines nearly all the time, it has not great heating power. At high altitudes the heating power of the sun is great so that any object exposed to its rays is rapidly heated ; there being thus marked variation in temperature between the shade and objects exposed to the rays of the sun.

Low temperatures kill or paralyse the activities of micro-organisms; therefore food remains without putrefaction, and the danger of infection by pathogenic organisms is decreased.

The amount of water that can be present in the form of vapour is decreased by low temperatures; therefore a low absolute humidity is one of the characteristic features at low temperatures.

Clouds are infrequent because the moisture in the warmer air is condensed at lower altitudes; therefore in addition to greater heating power, the sun shines for longer periods than at low altitudes. The result is that at high altitudes one experiences cold nights with low absolute humidity, and warm, sunny days.

The decrease in atmospheric pressure means a proportional decrease in the pressure of oxygen: this decrease in oxygen pressure is the limiting factor to human life at high altitudes. Communities do not naturally live near the limit imposed by lack of oxygen because of the difficulty in obtaining a supply of food.

There is a difference between going to a high altitude slowly, as in ascending a range of mountains, and in ascending quickly, as in going up in a balloon. In the former case changes take place in the blood producing acclimatisation, whilst in the latter the blood changes consist in an incerase in the number of red blood-corpuscles without a corresponding increase in the amount of hæmoglobin.

Life at moderately high altitudes is not injurious to health, as shown in vital statistics of monks at the Great St. Bernard Pass and of the inhabitants at Canton des Grisons.

The healthiness at high altitudes depends on the coldness, dryness, and stimulant effect of radiant heat.

In places one feels that the book would be improved by condensation. There are one or two erroneous statements, such as on p. 67: "dark pigmentation of the skin . . . is a preventive against radiation of blood-heat into circumambient air." Physically dark surfaces radiate more than light ones. Again, on pp. 96 and 99, there are some statements about carbon dioxide which are not understandable. In spite of these defects, the book contains much that is worth reading.

H. E. R.

The Principles of Ante-natal and Post-natal Child Physiology Pure and Applied. By W. M. FELDMAN, M.B., B.S. [Pp. xxvii + 694.] (London: Longmans, Green & Co., 1920. Price 30s. net.)

THE development of a new individual involves a number of special adaptations which are not found in the non-developmental period of adult life. The physiology of these adaptations requires special treatment. No course of physiology is complete without an outline of the period of development, but the special problems of infant life cannot be treated fully in an elementary course. The importance of this subject is especially great for the medical practitioner whose practice is often largely concerned with children and their development.

The arrangement of the material in this book is chronological. Starting with the preconceptional study of germinal cells, they are traced through fertilisation, growth, and development until puberty, when the individual becomes an adult.

The scope of the subject is tremendous, as it involves the study of all the usual physiological processes in addition to the special conditions due to fertilisation, intra-uterine growth, birth, and extra-uterine growth.

The treatment of the subject is adequate. One is surprised to find not only an explanation of the laws of heredity, but a physico-mathematical treatment of the cleavage planes in the dividing egg. Such a discussion is useful for anyone studying the subject, as it gives a starting-point. On the other hand, those who look for mere technical information will find it in the numerous tables and diagrams. Thus the weight and height of children at different ages and mathematical formulæ for expressing these relationships are given. These standards are useful for the purely practical purpose of determining whether the nutrition of a given child is what it ought to be.

All branches of the subject are treated: internal secretions, accessory food substances, digestion during childhood, metabolism of children, etc., all have a place in this book.

The data on which the book is founded have been collected from many sources. The collection of the material must have required great industry and perseverance. The putting together of the material in a readable form has been well accomplished. The book has something beyond these merely desirable attributes, as it is characterised by an enthusiasm and a freshness of outlook. It is original because it is the first book which attempts to cover the field of child physiology.

This book can be thoroughly recommended to anyone interested in physiology or in child development. To obtain the greatest value from it, the reader must be able to follow mathematical formulæ, such as the differential calculus.

The numerous references by themselves are of great use to students of the subject.

H. E. R.

The Link between the Practitioner and the Laboratory. By Cavendish Fletcher, M.B., B.S., M.R.C.S., L.R.C.P., and Hugh McLean, B.A., B.C. Cantab., D.P.H. Camb., M.R.C.S., L.R.C.P. [Pp. 91, with 7 illustrations.] (London: H. K. Lewis & Co., 1920. Price 4*s*. 6*d*. net.)

The selection of material to be sent to the pathological laboratory, and the technique of its collection and preservation by the medical practitioner, form the subject-matter of this little book. Attention to these details makes all the difference between a haphazard and an accurate diagnosis, and nowadays, when the practitioner so often bases his future treatment upon the pathological report, it is of the utmost importance that the rules laid down as to the securing and transmission of uncontaminated specimens should be strictly adhered to.

The advice given as to the preliminary preservation of histological material is good; it is, unfortunately, the experience of many pathologists to receive material for histological examination which has been badly selected, and placed straight away into a strong solution of formalin or methylated spirit. The authors recommend 50 per cent. alcohol, which, although only a slow fixative, yet allows of the subsequent use of more rapid fixing and hardening agents, without detriment to the final result.

In connection with the paragraphs dealing with collection and transmission of pathological material obtained from cases of cerebro-spinal meningitis, the stress laid upon rapidity of transit of cerebro-spinal fluid and infected naso-pharyngeal swabs is to be commended. So many failures have occurred from culturing swabs which have been allowed to become cold, where success so essentially depends upon keeping swabs and cerebro-spinal fluid at body-temperature right up to the time of culturing.

The diagrams illustrating points of technique are clear, and the text is concise and to the point. As an aid to the practitioner who, whilst appreciating the value of accurate pathological diagnosis, cannot find the time to carry out the technique himself, this guide should prove very useful.

H. A. HAIG.

The Physiology of Vision, with Special Reference to Colour-Blindness. By F. W. EDRIDGE-GREEN. [Pp. xii + 280, with 23 figures.] (London: G. Bell & Sons. Price 12*s*. net.)

Card Test for Colour-Blindness. By F. W. EDRIDGE-GREEN. (G. Bell & Sons. 25*s*. net.)

THE subject of colour-vision has engaged the attention of physicists and physiologists from the time of Boyle to the present day. Newton himself, who paved the way for all subsequent researches in colour from the physicist's point of view, was not very keenly interested in the question of colour mixing: "For I could never yet by mixing only two primary Colours produce a perfect white. Whether it may be compounded of a mixture of three . . . I do not know, but of four or five I do not much question but it may. But these are curiosities of little or no moment to the understanding the Phænomena of nature." In other words, the way in which the eye interprets the physical facts was for him a minor matter. Two physicists only less great than him, however, Thomas Young and Helmholtz, both men of wide medical and physiological knowledge, devoted much study to the mechanism of vision, and the theory of colour perception known by their joint names, suggested by the former and elaborated by the latter, obtained much support. For the physicist the spectrum of white light is a band of colour in which every line element corresponds to a perfectly definite and measurable wave-length, the distinguishing of any portion of which from its neighbour is merely a question of the resolving power of the instrument. It is the task of the physiologist to solve the very complicated problem of the relationship between wave-length—and mixture of wave-lengths—and colour perceived, and to analyse the mechanism by which the physically infinite number of colours is reduced to the few simple sensations known as the primary colours. The phenomena which he has to investigate are extremely varied, and besides ordinary colour-perception, the complex subjects of simultaneous and successive colour contrast, positive and negative after images, colour adaptation and other similar effects, have to be studied.

The book which is before us covers the whole field of physiological optics, in nearly every department of which Dr. Edridge-Green has made striking advances. His theory of the functioning of the retina is now generally held to furnish the most comprehensive and suggestive explanation that has yet been offered of the peculiarities of foveal and peripheral vision, the movement of after-images, and various other phenomena. On this theory the cones—and it will be remembered that the fovea contains cones only—are the perceptive elements, but they are in a condition to convert the incident light into nervous impulses only when the retinal fluid in which they are bathed contains the visual purple. It is the function of the rods to liberate this visual purple when stimulated by light A whole series of entoptic phenomena which are in closest agreement with this view are now known, and the features of foveal vision, such as the disappearance of a small bright object, find in it an exact representation.

The importance of Dr. Edridge-Green's work on colour vision is, in spite of a somewhat unscrupulous opposition in the past, at length receiving adequate recognition, and a wide circle of readers will welcome the summary of his views on the subject contained in this book, especially as hitherto

there has been no easily accessible general statement of them. The three-colour theory of Young, Maxwell, and Helmholtz has naturally received such prestige from the distinguished names connected with it that it has been slow to weaken its hold, even though no anatomist has ever found a trace of the three different retinal or nervous elements which it would seem to require. The experimental evidence against it brought forward by Dr. Edridge-Green goes far to render it quite untenable, or to demand such modifications as would render it too complicated to be of any use, just as the modifications which were introduced into the emission theory of light, to make it accord with the facts, destroyed all its force. For details we must refer the reader to the book: we can only mention the convincing proofs of the simplicity of the yellow sensation, and the various cases of colour-blindness detailed—such as the shortening of the red end of the spectrum as a *distinct* defect—which cannot be reconciled with the hypothesis of a missing, or diminished, green, red, or blue sensation. The author's own theory, based on a study of colour-blindness unrivalled in the number of cases examined, is that there are certain colour-perceiving centres in the brain which have been gradually evolved from a state in which only a sensation of light could be perceived. It is a diminished power of distinguishing between neighbouring colours in the spectrum which constitutes colour-blindness. The optic nerve can convey any mixed type of vibration, but the analytical centres differ in the power of discrimination in different individuals. Dr. Edridge-Green classifies observers as heptachromics, who see seven distinct colours in the spectrum; hexachromics, who see six; and so on, down to the dichromics, who see only two. (The totally colour-blind see, of course, only a grey band varying in luminosity from point to point). He himself has not attempted any physical explanation of the nature of the nervous impulses, but recently Dr. Houston has written some very suggestive papers on a mathematical theory of colour-perception, in which he strongly supports Dr. Edridge-Green's views.

Testing people for colour-blindness has a great practical importance in the case of signalmen and lookout-men of all kinds, to mention only the most obvious instance. Dr. Edridge-Green, having shown that the old "wool test" is worthless, has devoted many years to the perfection of other tests, and has had the satisfaction of seeing his methods adopted by the Admiralty, the Board of Trade, and other bodies of similar status. He has hitherto relied chiefly on his lantern, which is an expensive piece of apparatus, but has now issued a card test which can be carried in the pocket, and which promises to become the standard test where portability and simplicity in operation are demanded. The test is fully described in the book. The many ingenious features which it embodies are the fruit of a prolonged experience—only those who have tried the older tests can realise how defective they were—and have successfully eliminated all possibility of the examinee using differences of form, luminosity, or other aids in reading them. He must rely on his colour-perception alone. We have found it a convincing and rapid test, which readily eliminates tetrachromics and those of inferior grades of colour-vision, and we can confidently recommend it to all interested in testing for colour-blindness.

E. N. da C. A.

ENGINEERING

The Propagation of Electric Currents in Telegraph and Telephone Conductors. By Prof. J. A. Fleming, M.A., D.Sc., F.R.S. Third Edition. [Pp. xiv + 370, with 97 diagrams and illustrations.] (London: Constable & Co., 1919. Price 21*s.* net.)

The appearance of the third edition of Dr. Fleming's book on the *Propagation of Electric Currents in Telephone Conductors* is a welcome addition to the class

of literature which sets out to present a difficult and often mathematical subject in a manner easily comprehended by the student. The general outline of the work follows the same plan as that adopted for the earlier editions, the first of which appeared in 1911, but a number of additions have been made which enhance the value of the volume. These additions include recent progress in the development of the theory of the subject, with some references to practical work in the same direction. Amongst them may be mentioned in particular a useful table of circular and hyperbolic functions, and an extended chart of such functions, which did not appear in the original layout. These tables and chart are given in terms not of the usual degree, but of the more useful radian.

Extensive additions have been made to the chapter on the theory of wave propagation along wires; and also to Chapter IV, on Telephony and Telephonic Cables, where the instructive mechanical analogy of the vibrations of loaded strings has been added. The arrangement described is the one developed by Dr. Fleming in his laboratories, using a small motor to apply a sustained series of simple harmonic or circular impulses to the end of a stretched cord along which various loads may be arranged. The method forms an extremely pretty demonstration of wave propagation, and in particular the special properties of non-uniformly loaded lines may be rendered visible. Photographic examples of some of the results obtainable have been included in the book.

Chapter VII, on the Measurement and Determination of Cable Constants, has also been revised, but there would appear to be room for still further additions here, with the inclusion of more recent practical work. No measurements made on or with triode vacuum tubes, used either as telephone repeaters or for laboratory work, are here mentioned, while their use for repeating is, in fact, merely referred to in a later chapter. Possibly, however, there is insufficient data of this nature available in this country to render a useful summary practicable.

It is to Chapter IX that the most extensive additions have been made, and interesting particulars have been incorporated relative to the New York–San Francisco and other long loaded telephones lines, while a considerable amount of new theoretical matter on the predetermination of the constants of loaded cables has also been included.

The revision of the book for this edition has enabled the author to correct most of the misprints that occurred in the earlier copies, but a few numerical errors in the table on p. 114 have apparently escaped attention.

The book is written more for the student than for the telephone engineer, who is often by necessity forced to employ some empirical solution other than the theoretically best one, but as such it forms an excellent introduction to the theoretical side of the subject.

P. R. C.

Physical Laboratory Experiments for Engineering Students. By SAMUEL SHELDON, Ph.D., D.Sc., and ERICH HAUSMANN, E.E., Sc.D. [Pp. vi + 134, with 40 figures and 16 tables.] (London: Constable & Co., 1919. Price 6*s*. net.)

"THE material in this volume was prepared for the use of sophomore students in the Polytechnic Institute of Brooklyn. All of these students are candidates for engineering degrees."

This extract from the Preface fully explains the title, otherwise it would be hard to understand why these experiments could not be used by all and sundry studying mechanics, sound, heat, and light. As the engineer has to deal with problems involving space, time, matter entropy, and electricity, it is a little hard at first to follow the order in which the experiments

are built, but evidently the whole arrangement has been carefully thought out. Thus the first experiment—"radius of curvature of lenses by spherometer"—presupposes a knowledge of elementary physics, and introduces the spherometer, micrometer caliper, vernier caliper, plane surface, and concave and convex lenses—the latter being useful objects for exact measurement of space and its application.

The book—regardless of its title—may be used with advantage in the higher forms of our public or secondary schools.

J. Wemyss Anderson.

Thermodynamics for Engineers. By J. A. Ewing, K.C.B., M.A., LL.D., D.Sc., F.R.S., M.Inst.C.E., M.I.Mech.E. [Pp. xiv + 381, with 99 figures and 6 tables.] (Cambridge: at the University Press, 1920. Price 30*s*. net.)

The scope of this volume is best understood by saying it is divided into eight chapters and three appendices. The chapters taken in order are: First Principles; Properties of Fluids; Theory of the Steam Engine; Theory of Refrigeration; Jets and Turbines; Internal Combustion Engines; General Thermodynamic Relations; and Applications to Particular Fluids. The appendices: Effects of Surface Tension on Condensation and Ebullition; Molecular Theory of Gases; and, lastly, Tables of the Properties of Steam.

Those who are able to accept, without question, the author's opening words—"The Science of Thermodynamics treats of the relation of heat to mechanical work. In its engineering aspect it is chiefly concerned with the process of getting work done through the agency of heat"—will, equally without question, vote the volume a complete success. Indeed, within the limits defined, it is a success, but the trained engineer of to-day and of the future must be prepared to deal with all natural phenomena and processes involving questions of energy, and therefore a better definition of thermodynamics would be that it dealt with the physical theory of energy in relation to matter.

It is true that such a definition opens up a large field of physics and chemistry, or, perhaps, to be more correct, physical chemistry, yet the author has to resort to one appendix to treat on the "Effects of Surface Tension on Condensation and Ebullition," and to another appendix to draw particular attention to the "Molecular Theory of Gases."

The present volume, then, as the author defines thermodynamics, is, we would repeat, an excellent treatise on the thermodynamics of heat engines and refrigerators, and would serve as an introduction for those who wish to specialise in these branches of engineering; but we still require a volume that will deal with thermodynamics for engineers, and lay the right up-to-date scientific foundation which will enable the subsequent man, as an experienced engineer, to act up to his charter and "direct the forces of Nature to the use and convenience of man." In such a book, questions of surface tension and capillary effects generally, and the molecular theory of gases, will find a place in the body of the book, while "energy," "work," and "power," will be clearly defined for the benefit of those who subsequently study heat engines.

J. Wemyss Anderson.

Applied Aerodynamics. By L. Bairstow, F.R.S., C.B.E. [Pp. x + 566, with 255 figures and 22 plates.] (London: Longmans, Green & Co., 1920. Price 32*s*. net.)

It is quite impossible, within the limits imposed for this review, to do justice to this important volume.

The author is an acknowledged authority on aerodynamics and we feel this throughout—from the point where he deals with the "Principles of Flight" (p. 18) to "The Stability of the Motions of Aircraft," which forms the subject-matter of chapter x (and last). Further than this, Mr. Bairstow knows and appreciates the limitations of those engaged in practice, and interprets the results of research, gives approximate solutions to awkward equations, and illustrates results graphically wherever possible. Explanations and actual weights, dimensions, etc., are freely given, and in many places, after what, to many, will be severe mathematical flights, the reader finds himself brought gently to earth by a practical application.

Every engineering graduate of the future should have a good knowledge of aerodynamics, but it would be impossible to expect other than those who specialise in aircraft to assimilate the material in the present book. Is it too much to ask the author to issue a further volume based on the present one of about one-fifth the size and one-quarter the price? It would be as great a boon to the engineering student as the present volume is to those interested or engaged in the industry.

J. Wemyss Anderson.

The Theory and Practice of Aeroplane Design. By S. T. G. Andrews, B.Sc. (Engineering), London, and S. F. Benson, B.Sc. (Engineering), London. [Pp. viii + 454, with 308 figures and 74 tables.] (London: Chapman & Hall, 1920. Price 15*s*. 6*d*. net.)

This volume is issued under what is known as "The Directly Useful D.U. Technical Series," and deals, not only with the design of the various parts—wings, fuselage, chassis, air-screw and control surfaces—but treats of the stability, performance, and lay-out of machines, together with a chapter on "The General Trend of Aeroplane Design."

The work throughout is reliable and of the utmost value to engineers engaged in, and for students training to enter, the industry, and the following suggestions are meant "for improvements in future editions" rather than critical of the present volume. In the first place the title should be "Aeroplane Design." The words "Theory and Practice" might have been in order ten years ago—to-day they are out of place.

Then, again, a book on design must not only deal with up-to-date work, but must be suggestive from the first to the last page. A chapter on the general trend is not sufficient. For instance, Fig. 155 and Table XXIX deal with actual stream-line strut sections—and they are very varied. What is the reader to do? Which is he to use? Is the "Beta" section (p. 213) the last word? Mention should have been made and results given of the Hele-Shaw method of investigation, which would have given the designer plenty of food for reflection, with possible development. Finally, the work on air-screw design is marred by the statement on p. 7 that "the function of the air-screw is to transform the torque on the engine crank-shaft into a propulsive thrust by discharging backwards the air through which it moves; and whose resultant reaction enables the necessary forward momentum to be secured." This sentence requires drastic rewording—the "discharging of air backwards" is not a function of the air-screw.

J. Wemyss Anderson.

Aircraft in Peace and the Law. By J. M. Spaight, O.B.E., LL.D. [Pp. viii + 234.] (London: Macmillan & Co., 1919. Price 8*s*. 6*d*. net.)

The author, in a very able manner, has dealt with the more important questions—chiefly of law—that must arise with the development of civil aerial transport both at home and abroad.

Questions of sovereignty of the air, frontiers, difference in the laws of various lands, registration of machine, efficiency of the pilot (including medical examination), difference between crimes and torts as applied to the air, customs, collisions and wrecks, are all discussed in a most impartial manner—but the author does not fail to give a lead where one seems desirable.

Notwithstanding the Convention relating to International Air Navigation in 1919 (full details and findings of which are given in Appendix I), the author clearly shows the necessity for continued international activities on the part of each and every Government.

The possibilities of an Englishman flying a German machine from Belgium to England and coming to grief, with damage to property, in France, are, at the moment, truly Gilbertian.

An excellent bibliography adds greatly to the volume, and while, as already indicated, it is of particular value to lawyers, it could be and should be read by all interested in aircraft.

J. WEMYSS ANDERSON.

Aviation. By BENJAMIN M. CARMINA. [Pp. xii + 172, with 92 figures.] (New York: The Macmillan Company, 1919. Price 11*s*. net.)

THE author of this interesting little book is Assistant Chief Instructor at the Y.M.C.A. Airplane Mechanics' School, somewhere in the United States of America. It should be of great value to air artificers and mechanics, and to those who wish to know something of the general principles of flight and of the construction of aeroplanes, without the aid of mathematics.

J. WEMYSS ANDERSON.

A Primer of Air Navigation. By H. E. WIMPERIS, M.A., late Major R.A.F., Head of Air Navigation Research Section, Air Ministry. [Pp. xiv + 128, with 57 figures.] (London: Constable & Co., 1920. Price 8*s*. 6*d*. net.)

THIS volume is an elementary treatise on a subject of rapidly growing importance, by an acknowledged authority on the subject. Whilst the fundamental principles of air navigation are the same as those of sea navigation, the differences in detail are sufficiently great to render a separate treatment of the methods of air navigation desirable. The two chief differences are: (i) Observations are more difficult to make in an aeroplane than on a ship, owing to the absence of a horizon; (ii) the reduction of the observations is simpler in air navigation, as the position of the aeroplane does not require to be known with the accuracy required for a ship at sea. As regards the first difference, it is true that a cloud horizon may sometimes be utilised, but unless the cloud layer is at a uniform height, the results so obtained are liable to a considerable error, even if the precaution has been taken to verify that the dip of the horizon is the same at two diametrically opposite points. This necessitates measuring *true* altitudes, and not altitudes above the apparent horizon. The difficulty of determining the true vertical in an aeroplane here enters, owing to the acceleration of the plane combining with gravity to produce a false vertical. The disturbing effects of acceleration are best avoided by using a bubble sextant, but in order to obtain reliable results, the plane must be flown as steadily as possible in a fixed direction during the observations. It requires a very skilled pilot to do this. The author has not laid sufficient emphasis upon this fundamental fact.

As regards the second difference, the range of vision from an aeroplane being much greater than from a ship, and dangers from rocks and shoals

being absent, it is not necessary that the position should be determined with an accuracy exceeding ten miles. This enables the observations to be reduced with the aid of such appliances as the Veater diagram or the Bygrave slide rule, so avoiding the use of logarithms.

The author explains in some detail the determination of position from astronomical observations, using the Sumner line method and these mechanical methods of reduction. The principles and methods of dead reckoning navigation—in which two inventions by the author, the wind gauge bearing plate and the course-setting sight, are of considerable use—are dealt with, and also the determination of position by the use of directional wireless, with the method of plotting the directions on a Mercator chart.

The book is written in a straightforward, simple style, and can be thoroughly recommended.

H. S. J.

The Principles of Aerography. By ALEXANDER McADIE, Professor of Meteorology, Harvard University, and Director of the Blue Hill Observatory. [Pp. xii + 318, with 112 figures.] (London: George G. Harrap & Co., 1917. Price 21*s*. net.)

THE author, in his preface, makes a distinction between meteorology and what he terms aerography, the former being the science of recording diverse atmospheric conditions, the latter resembling geography in its widest sense, and being concerned with utilising the knowledge gained by meteorology in insuring human safety and expediting progress. The book, therefore, aims at giving prominence to recent work in exploration of the air, an account of much of which has hitherto only been available in scientific publications. Reference is made to the work of Shaw, Dines, Cave, and other modern investigators, and Simpson's theory of thunderstorms is given in considerable detail. It is unfortunate that the important work of G. I. Taylor on turbulence in the atmosphere, Rayleigh's investigation of the dynamics of revolving fluid in the atmosphere, and much of Shaw's work on cyclones, are just too recent to be included, so that in some respects the present state of knowledge is not well represented. Wherever possible, the practical application of results is emphasised, a feature which is, unfortunately, too often lacking in textbooks on meteorology. Thus the methods of forecasting storms and frosts are dealt with in detail, and in the discussion of thunderstorms are included details of methods of lightning protection.

Much attention is given to cloud forms and the thermodynamics of their formation, and an attempt is made to classify them according to their origin rather than according to their appearance. The older plan has much to recommend it, and the proposed changes are not likely to meet with general acceptance. In connection with clouds, the author rightly emphasises the unsatisfactoriness of cloud observations being made at most observatories only twice in twenty-four hours—say, 8 a.m. and 8 p.m. Such records have little value when information as to the average cloudiness at, say, midday is required. More than once, eclipse expeditions have gone to unsuitable stations owing to the available cloud observations giving misleading information.

The author makes a special point of using absolute units throughout. This step is to be welcomed. Their general use in meteorological textbooks will greatly help to displace the old arbitrary units.

The book is very well indexed and illustrated, and, in spite of somewhat ambiguous phrasing in several places, can be thoroughly recommended. It may be noted that at the top of p. 32 the reciprocal of flattening of the earth is given as 1/29·4 instead of 1/294.

H. S. J.

Airplane Photography. By HERBERT E. IVES, Major in Charge of Experimental Department, Photographic Branch, Air Service, U.S.A. [Pp. 422, with 210 illustrations.] (Philadelphia and London: J. B. Lippincott Co., 1920. Price 18*s*. net.)

AEROPLANE photography practically originated in the war, and under the stress of military and naval requirements, it underwent an extraordinarily rapid development and reached a relatively high pitch of perfection. The author of this volume, a scientist who became head of the photographic experimental department of the U.S. Air Service, is convinced that it has equally great possibilities and practical uses in times of peace. The semi-popular treatise under review contains an account of the methods employed in aeroplane photography during the war, the uses to which it was put, and its possibilities for the future. The book is extremely well illustrated, and many of the illustrations, particularly those of war subjects, are of fascinating interest. To some extent the requirements during the war were extreme: in order not to become too easy a target for anti-aircraft gunners, aeroplanes had to fly at a great speed and generally at a great height; this necessitated the length of exposures being reduced to a minimum and, generally, successive photographs being taken at as short intervals as possible. These requirements to some extent conditioned the types of camera and the brands of plates used, and the methods of development employed. But, although peace conditions differ somewhat, it is desirable that those who would contribute to future peace-time development should be acquainted with what has already been done. Major Ives' volume will serve this purpose; in addition, it will be found of great interest by the large body of amateur photographers.

The nature of the contents of the volume can be tolerably well indicated by the titles of the sections into which it is divided: Introductory; The Airplane Camera; The Suspension and Installation of Airplane Cameras; Sensitised Materials and Chemicals; Methods of Developing Plates, Films, and Papers; Practical Problems and Data; The Future of Aerial Photography.

The last section will be found in some ways the most interesting; the author's views, based on a wide practical experience, must receive great weight, and it appears certain that peace-time aerial photography has a great future before it. Apart from purely pictorial uses, it has undoubted advantages in preliminary surveys for engineering purposes in undeveloped country; in locating submerged vessels, with a view to estimating the chances of salvage; in city mapping; in revealing shifts in channels and shallows; in making coast and harbour surveys, and in other directions too numerous to mention.

The most important omission is an account of the method employed in the French Air Service of obtaining a photograph true to scale from an oblique (distorted) photograph: for accurate mapping purposes, the use of such a method of restitution is essential.

H. S. J.

MISCELLANEOUS

Food Inspection and Analysis, for the Use of Public Analysts, Health Officers, Sanitary Chemists, and Food Economists. By ALBERT E. LEACH, S.B. Fourth Edition, revised and enlarged by ANDREW L. WINTON, Ph.D. [Pp. xix + 1090, with 120 figures and 40 plates.] (New York: John Wiley & Sons, 1920. Price 45*s*. net.)

ALL who are interested in the important subject of food inspection and analysis will welcome the appearance of the fourth edition of this standard work, which represents an advance on the previous editions in more than

one way. The lists of references at the end of the chapters have been dropped, and have been substituted by more complete footnote references, whilst the size of the book has increased by ninety pages, nineteen of which are devoted to a brief but concise consideration of the determination of acidity by means of the hydrogen electrode, from the pen of Dr. Gerald L. Wendt.

It is probable that this important addition will require even more space in subsequent editions, when the value of the electrometric method of titration becomes more familiar to food analysts.

The book appears to be well up to date, and is produced in the usual excellent manner that one associates with the publishers.

J. C. D.

The Falkland Islands and Dependencies. An Illustrated Handbook. By T. R. St.-J. [Pp. 100, with 13 illustrations.] (Stanley, Falkland Islands: The Government Printing Office, 1920.)

This book, written by Lieut.-Col. T. R. St.-Johnston, recently Acting Governor of the Falkland Islands, and published by the Government Printing Office there, contains much interesting matter combined with many good photographs, and maps dealing chiefly with the whaling industry for which those islands are a centre. The scenery of the islands reminds the writer of a Scottish moorland: "Deep bays open up into land-locked harbours, and beneath the blue sky the rugged grey hills have a beauty and grandeur all their own. Occasionally a seal can be seen disporting himself in the water, while the swimming sea-birds, penguins, and 'logger' ducks barely trouble themselves to move out of the ship's way." Here was, of course, the scene of the great Falkland Islands battle between the British and German navies, where the latter met with such a disaster. The chief town, Stanley, has straight and regular streets, stores, and well-kept, comfortable houses, each with its flower-filled porch or glass conservatory. The surrounding country can be best described as consisting of mile after mile of springy turf, dotted about with grey stone boulders and scattered clusters of browsing sheep, contentedly grazing on the succulent grasses that abound everywhere. Farm-houses and shepherds' homesteads are seen scattered here and there, the farms being great tracts of country ranging from some two thousand acres up to perhaps one hundred thousand acres. The land is of a gentle undulating nature, but there are some mountain ranges which offer the excitement of Alpine climbing. The best-known range is that of the Wickham Heights, while Mount Adam reaches 3,000 feet. Trout abound in the streams. The colony is full of historical associations; but the discovery of the islands is unknown. They were called the Ascension Islands in an old Spanish chart of 1527. The history of the islands is full of many adventurers and navigators, British, French, and Spanish. In 1842 the total population was only 49,000 people. The whole story is briefly but well given by the author. There is much sheep-farming, though there are occasional severe blizzards and sand-storms in the early spring. The author says that the climate has been improving during the last eighty years or so. There are some 700,000 sheep in the colony now. The history of the whaling industry reads like a romance. In 1905 the whole of the sea south of the Islands was a silent waste of waters dotted about with uninhabited islands. In ten years it has become the site of the largest whaling industry the world has ever seen. Great factories, alive with mechanical power, are preparing countless thousands of barrels of oil for the European markets. The seas are traversed in all directions by steam whalers; and a permanent population of a thousand busy people has been settled in South Georgia. This island was originally let by Government at a rental of £1 per annum. The species of whales caught are *Megaptera nodosa*, *Balænoptera physalus*, and *B. musculus*.

The total value of the oil has risen from £251,077 in 1909, to £1,218,316 in 1918. The amount of oil obtained from the various whales has been estimated to be roughly forty barrels from *M. nodosa*, fifty or more from *B. physalus*, and anything up to one hundred barrels from *B. musculus*, the blue whale. There should also be about a ton of guano to every five barrels of oil, but the whalebone is now of less value than it used to be. Seals, sea elephants, etc., are also hunted for oil. Col. St.-Johnston describes the constitution and laws of the colony, the amusements of the people, and gives some brief notes on the natural history and geology. Altogether an excellent little book. R. R.

The Rising Tide of Colour against White World-Supremacy. By Lothrop Stoddard, A.M., Ph.D., with an Introduction by Madison Grant, Chairman of the New York Zoological Society. [Pp. xxxii + 310.] (New York: Charles Scribner's Sons, 1920.)

If one was to tell the ordinary man in the street that, to-day, the coloured races were threatening White world-supremacy, one would in all probability be laughed at for one's pains. Nevertheless the author of this most interesting book, interesting whether you believe in it or not, seems firmly to believe that the relations between the primary races of mankind is the first political problem of the twentieth century. Nor does his belief appear to be unfounded, as he is prepared to prove.

An admirable summary of the biological and historical aspects of the case is furnished in the Introduction by Mr. Madison Grant, in which he points out how very nearly, in times gone past, has Europe been dominated by Asiatics, whether Mongols under Attila, Jenghiz, or Bajazet, or Arabs under Othman. In all these invasions it seems only that the Teutonic, or Nordic, peoples of Europe were able to turn the tide of Asiatic invasion at Chalons, Wahlstatt, Tours, or Vienna. The siege of Vienna was only two hundred and forty years ago, and Asiatic irruptions into Europe had lasted for more than twelve hundred years. Why, then, should we believe Asia to be incapable of fresh aggrandisement if Europe were torn, as she is now, by fratricidal strife? In the first chapter of the book Mr. Stoddard points out that, although the greater portion of the world is under White control, nevertheless the coloured races outnumber the whites by two to one, and are increasing more rapidly. He goes on to describe the habitat of the Yellow Races, their aspirations, and their desire for expansion, the last of which is necessary for their very existence. He points out that, while the surplus Chinese population could be allowed to flow to the East Indies, Central Asia, and Indo-China, the Japanese colonist cannot stand tropical heat, or even moderate cold, so that the natural outlets for Japanese migration lie in White-men's lands—California, temperate Latin America, and Australia; and bitter feeling is therefore caused in Japan by the laws excluding the Japanese from entrance into those regions.

Brown-man's land is Southern Asia and Northern Africa, and their great bond is Islam, though Mohammedanism is only professed by one-fifth of the inhabitants of India. There are, of course, immense numbers of anti-Europeans in every Brown country; but the author seems to consider that, once White pressure on the Brown World was relaxed, there would be no need to fear Brown aggression, as the various races would fly at one another's throats, as of yore.

On the other hand, Mr. Stoddard regards the Black races as being an entirely passive factor unless converted to Islam, when they would be used by the heads of that religion for their own ends.

The Red race is the least numerous of the coloured peoples. It forms the greater part of the population of "Latin America" with the exception

of Chile, Argentina, Uruguay, and Southern Brazil, but most American Indians are saturated with white blood. There seems to be little or no danger from them, racially. The danger is that their lands may become settled by Chinese or Japanese coolies.

In the second part of the book the author shows how for four hundred years—up to the beginning of this century, indeed—the expansion of the White world has been going on. The Russo-Japanese War marked the beginning of the ebb, while the late War, a veritable White civil war, which Mr. Stoddard compares with the Peloponnesian War, has weakened Europe to such an extent that only strong measures can save her civilisation from oblivion.

The author divides the White defences against the coloured tide into what he terms the "outer" and the "inner" dikes. He is of the opinion that White control of Asia should be abandoned, as he thinks that, once the nationalist aspirations of India and the rest of Brown Asia are satisfied, all fear of a Brown-Yellow alliance would be at an end. On the other hand, he considers that the retention of Africa and tropical America is absolutely vital to the White races.

C. Ross.

Hindu Achievement in Exact Science. A Study in the History of Scientific Development. By Benoy Kumar Sarkar. [Pp. xiii + 82.] (London: Longmans, Green & Co., 1918. Price 5s. net.)

Whatever the actual Hindu achievement in exact science may have been, this book is not one of them. It has recently been severely handled in an American mathematical magazine - with which we quite concur. Apparently the author thinks that mere mention of a subject in ancient history proves that the person who has mentioned it discovered all about it. For example, we doubt much whether Bhaskar-Charya anticipated Newton by over five hundred years in the discovery of the principles of the differential calculus and Newton's application to astronomical problems and complications. A statement like this could only be proved by giving very detailed quotations from that pundit's work, which the author does not attempt to do. We do not mean to imply that the Hindus discovered nothing in mathematics: perhaps they did; but a very judicious and critical inquiry is necessary before all their claims can be admitted.

We see that, in the June number of *Discovery*, Mr. Ikbal Ali Shah would have us believe that the ancient Indians were able to make and use flying machines, simply on the strength of a few vague quotations from ancient writings. We would suggest that these allusions were rather flights of poetical fancy than anything else—akin to the flying carpet in the *Arabian Nights*, and to the achievements of Daedalus and his Icarus. In short, this book and the article referred to show, to our mind, a considerable deficiency of the power of accurate scientific judgment, which requires practical evidence in the first place.

O. A. Craggs.

The End of the World. By Joseph McCabe. [Pp. vii + 267, with numerous illustrations.] (London: George Routledge & Sons, 1920. Price 6s. net.)

The subject-matter of this volume is wider than is suggested by the title. The author's main purpose is to discuss the evidence which modern astronomy can bring to bear upon the ultimate end of a star, or in particular of the earth. This question is intimately bound up with theories of stellar evolution and of stellar cosmogony in general. The author, therefore, brings under review many recent discoveries and theories, and introduces his readers

to most of the newer developments of astronomy. He writes for the inexpert reader with a pleasant, easy style, and, for a book of this type, with a remarkable freedom from serious inaccuracies. The volume will serve a very useful purpose in bringing the reader in touch with those developments which have so greatly widened our outlook upon the universe since the beginning of the present century.

A few of the inaccuracies and omissions may be mentioned: if a further edition should be required, perhaps these will be rectified. On p. 10, the ring nebula in Lyra is stated to be a spiral seen edgeways: this is not correct—it is a planetary. On p. 20 the *weight* of the earth is referred to: it is to be regretted that this incorrect and misleading terminology should be perpetuated. On p. 33 there is a confusion between W. H. and E. C. Pickering. On p. 80 the probable existence of dark stars is overlooked, and in the same paragraph (p. 81) the assumption is tacitly made that *α* Centauri (*not* a Centauri) has no motion perpendicular to the line of sight. On p. 84 the representation of a star as a crust which confines a molten interior is inaccurate. On p. 125, the evidence afforded by Shapley's recent work on clusters on the general question of cosmic dust has been overlooked; in view of this important evidence, the paragraph requires rewriting. On p. 134 occurs a very bad sentence: "the photographic plates used in the telescope are such tiny squares of glass that the figure of Mars on them . . . is about the size of a pin's head"—the size of the plate apparently conditions the size of the image! The final chapter in the book is the poorest: the giant and dwarf theory of stellar evolution is represented as not being generally accepted by astronomers; and when the author touches on the relationship between æther and matter he is quite out of his depth: he refers to "the material atoms which make up our worlds being formed out of æther," and so on. It is to be regretted that the last chapter falls so far below the general level of the book. H. S. J.

John Murray III, 1808–1892. A Brief Memoir by John Murray IV. [Pp. viii + 106, with portrait and illustration.] (London: John Murray, Albemarle Street, W., 1919. Price 3*s*. 6*d*. net.)

The name of John Murray is a household word, and the title of this little work indicates that this old and celebrated firm has now been carried on by four generations of the same name. Many will therefore be interested in this memoir of the last John Murray, written by his son, which carries forward that most entertaining book, the *Life of John Murray II*, by Samuel Smiles, published in 1891—a book which contained numerous letters from most of the famous writers of the earlier part of last century, and ably pictured the character of a publisher who not only built up the greatest publishing business in the world, but who was noted as a Mæcenas of the time. The first part of this book, also, expounds for the reader the character of a good, learned, and studious man, looked up to and loved by a large and varied society of people, which included such notable names as Gladstone, Bright, and Livingstone; and the second part contains extracts of his letters home during his travels. These not only formed an important part of his own life, but subsequently bore fruit in the shape of the famous Guide Books. Although some travellers were latterly in the habit of consulting Baedeker's book, Baedeker himself acknowledged that the idea, and to a great extent the matter, were drawn from Murray's Guide Books, the original conception of which was due entirely to John Murray II (if we remember a right). Previous to the publication of them nothing existed in print to help the traveller on his way. In fact, Baedeker states distinctly in his first edition that he copied, extracted, and even translated freely from Murray's books.

We learn from the present Mr. Murray that his father's chief study was that of geology, to which he devoted considerable time in the earlier part of his career; but, in addition, he took a deep interest in arboriculture and horticulture. One is accustomed to picture a student sitting alone in his study in perfect quiet; but Mr. Murray says that "he never had a study of his own, but preferred to work"- and he worked almost every evening —"in the midst of a family party, working, talking, or playing games, which, however, never seemed to disturb him." These few words recall to one's mind the method of composing of Sebastian Bach, and leads one to deplore afresh the modern conditions which bring in their train such a stress on the nervous system. In the following passages the present Mr. Murray bears witness to the worth of his father's character: "My father had an unusual faculty for seeing the good in people from whom he differed absolutely on politics, religion, and other subjects, and being on the most friendly terms with them." "His generosity was shown, not only in gifts of books, but in lavish help to all sorts of good causes. He served from the first on the committee for the decoration of St. Paul's; and when Bishop Thorold started his great scheme for the evangelisation of the slums of his diocese (now the South London Church Fund), it was my father who suggested to him to include in it the poor district of South Wimbledon, which had sprung up like a mushroom under his eyes. He had noticed with dismay the growth of its mean streets and population in the course of his work as a J.P. The formation of a new parish, the salaries of workers, and the building of the Church of All Saints, were all abundantly helped by him, as an inscription in the church now testifies. The amount given by him to private cases of distress, whether poor authors or others, will never be known." The following extract will interest scientific readers: "When the MS. of the *Origin of Species* was submitted, my father showed it to his intimate friend, George Pollock (a son of the old Chief Baron), who strongly urged him to publish it. He took the advice, though in those days it required some courage to act upon it. Charles Darwin was one of the most courteous and modest of authors. I was present when he called, in 1887, with a MS. in his hands, and said, 'Here is a work which has occupied me for many years and interested me much. I fear the subject of it will not attract the public, but will you publish it for me?' My father replied, 'It always gives me pleasure and hope to hear an author speak of his work thus. What is the subject?' 'Earth-worms,' said Darwin. The book was duly published, and six editions were called for in less than a year."

Peetickay, an Essay towards the Abolition of Spelling. By WILFRED PERRETT. [Pp. 97.] (Cambridge: W. Heffer & Sons, 1920. Price 6s. net.)

SPELLING reform has now been before us for some three centuries—without success. One reason for the failure is that no decision has yet been reached as to which of the many systems of writing proposed in place of the existing system is the best one; and, naturally, the public is not likely to make any change until this point has been settled. The chief difficulties are that we have some eleven elementary vowel-sounds, but only five vowel-symbols wherewith to write them; that our homophones are numerous; that our dialects are many; and that it would be a loss to the world to change the original form of our Greek and Latin words, which we now retain, though we mispronounce these words themselves. The courses open to us are: (1) to adopt an entirely new and scientific writing, say some shorthand or Persian character based on the principle of Bell's "Visible Speech"; (2) to use Roman or Greek characters with modifications where required, such as (*a*) new letters, or (*b*) diacritics, or (*c*) digraphs, or (*d*) a positional system, like that of our present spelling, but rendered regular.

Mr. Perrett's system lies between (1) and (2*a*). He proposes to retain the Roman consonants, with the addition of the Anglo-Saxon symbols for *dh* and *th*, but to use a physiological notation for the vowels, based on the pitch of them—strokes drawn upwards, rightwards, and downwards, and at intervening angles, for the vowels, in the order *i, e, a, o, u*. The retention of the old consonant-symbols makes the script easily decipherable; and the vowels are designed upon a logical scientific principle, which is easily mastered. On the other hand, I gather from the examples that the vowel-strokes are intended to be discontinuous from the consonants—which renders a running script impossible, adds greatly to the labour of handwriting, and tends to illegibility.

Moreover, one asks at once, why should only the vowels be drawn upon scientific principles? Why not all the letters? Why not adopt at once a complete physiological system as referred to in (1) above—which should have been designed long ago? Again, if we retain the Roman consonants, why should we not retain the Roman vowels also, distinguished by one or more of the methods referred to in (2 *a*, *b*, *c*, *d*) above? The author objects to diacritics, because he says, "From the typographer's point of view, a diacritic means a new letter." This, in fact, is not the case, because almost every fount of type contains all the vowels marked with the three French accents, and also with the diæresis, giving, with the five unmarked vowels, no less than twenty-five vowel-symbols altogether; and I showed long ago that, if used with intelligence, these suffice to give us a complete scientific vowel system (for English at least), without the introduction of a single new letter. On the other hand, as the author states, diacritics require what I call back-hand marks in script; but so do *i* and *t* and the Anglo-Saxon symbol for *dh* (which the author supports); and it is easy to show that arrangements can be made by which a complete diacritical system (giving stress as well as vowel) can be constructed which will actually require fewer back-hand marks than are required by our present spelling. Many good systems under (2*c*) and (2*d*) may also be designed; and there are also mixed methods, and the late Dr. Larrison's system under (2*a*)—all of which are preferable in some respects to Mr. Perrett's, though they may be inferior to it in others.

It is certainly good to devise as many systems as possible, for comparison; but the problem before us is to determine the best one. This requires a very wide and deep survey of all the difficulties and requirements; and this book hardly attempts to answer the question, but merely insists on the author's particular solution. We can suggest at least half a dozen other solutions which are as good. Of course, the public can always and now adopt one reform—the dropping of useless letters—which would by itself constitute a great advance; or, by the introduction of an accent without any other change (as pointed out by me in SCIENCE PROGRESS for 1913–14, p. 367), it might amend our spelling immensely—and still more so by adopting both these alterations. But even these, simple as they are, would be a cause of much expense in printing, and would leave the final constructive problem unsolved.

The book is written in an irregular and garrulous manner; but it is often witty, always amusing, and contains information which will be of interest even to those who have given some study to the subject.

R. ROSS.

BOOKS RECEIVED

(Publishers are requested to notify prices)

The Concept of Nature: Tarner Lectures delivered in Trinity College, November 1919. By A. N. Whitehead, Sc.D., F.R.S., Fellow of Trinity College, Cambridge, and Professor of Applied Mathematics in the Imperial College of Science and Technology. Cambridge: at the University Press, 1920. (Pp. ix + 202.) Price 14*s*. net.

Geodesy, including Astronomical Observations, Gravity Measurements, and Method of Least Squares. By George L. Hosmer, Associate Professor of Topographical Engineering, Massachusetts Institute of Technology. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. xi + 368.) Price 18*s*. 6*d*. net.

A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse. By Florian Cajori, Ph.D., Professor of History of Mathematics in the University of California. Chicago and London: The Open Court Publishing Company, 1919. (Pp. viii + 299, with portraits of Berkeley and Maclaurin.) Price 8*s*. 6*d*. net.

Traité de la Lumière. Par Christian Huyghens. Paris: Gauthier-Villars, Quai des Grands-Augustins, 55, 1920. (Pp. x + 153.) Price 3.60 frs.

Ozone. By E. K. Rideal, M.B.E., M.A. (Cantab.), Ph.D., Professor of Physical Chemistry, University of Illinois. London: Constable & Co., Orange Street, W.C., 1920. (Pp. viii + 198.) Price 12*s*. net.

The Physical Chemistry of the Metals. By Rudolph Schenck, Professor of Physical Chemistry in the Technischen Hochschule in Aachen. Translated and annotated by Reginald Scott Dean, Research Metallurgist, American Zinc, Lead, and Smelting Co., St. Louis, Mo. New York: John Wiley & Sons; London: Chapman & Hall, 1919. (Pp. viii + 239.) Price 17*s*. 6*d*. net.

Everyday Chemistry. By. W. Robinson, B.Sc., Instructor in Chemistry and Physics, Officers' School of Education, Oxford. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 130). Price 3*s*. 6*d*. net.

The Nomenclature of Petrology, with References to Selected Literature. By Arthur Holmes, D.Sc., A.R.C.S., D.I.C., F.G.S., F.R.G.S., Demonstrator of Geology and Petrology in the Imperial College of Science and Technology, London. Technical Geologist to the British Fire Prevention Committee. London: Thomas Murby & Co., 1 Fleet Lane, E.C.4. (Pp. 284.) Price 12*s*. 6*d*. net.

Oil-Finding: An Introduction to the Geological Study of Petroleum. By E. H. Cunningham Craig, B.A., F.R.S.E., F.G.S., Member of Council of Institution of Petroleum Technologists, late of H.M. Geological Survey. Second Edition. London: Edward Arnold, 1920. (Pp. xi + 324, with 13 plates.) Price 16*s*. net.

A Manual of the Timbers of the World; Their Characteristics and Uses. By Alexander L. Howard, to which is appended an account, by S. Fitzgerald, of the Artificial Seasoning of Timber. London: Macmillan & Co., St. Martin's Street, 1920. (Pp. xvi + 446, with 109 illustrations.) Price 30s. net.

General Botany for Universities and Colleges. By Hiram D. Densmore, M.A., Professor of Botany at Beloit College, Beloit, Wisc., with original illustrations by the author and by M. Louise Sawyer, M.S., formerly Instructor in Botany at Beloit College. New York and London: Ginn & Company. (Pp. xii + 459, with 289 figures.) Price 12*s*. 6*d*. net.

An Introduction to Entomology. By John Henry Comstock, Professor of Entomology and General Invertebrate Zoology, Emeritus, in Cornell University. Second Edition, entirely rewritten. Ithaca, New York: The Comstock Publishing Co., 1920. (Pp. xviii + 220.) Price $2.50.

The Seashore. By W. P. Pycraft, Zoological Department, British Museum (Natural History), Member of the Royal Anthropological Institute, etc. London: Society for Promoting Christian Knowledge; New York: The Macmillan Company, 1920. (Pp. vi + 156, with 11 plates, 12 figures, and 2 maps.) Price 4*s*. 6*d*. net.

Vertebrate Zoology. By Horatio Hackett Newman, Ph.D., Professor of Zoology and Embryology in the University of Chicago. New York: The Macmillan Company, 1920. (Pp. xiii + 432, with 217 figures in the text.)

Principles of Animal Biology. By A. Franklin Shull, Associate Professor of Zoology in the University of Michigan, with the collaboration of George R. Larne and Alexander G. Ruthven. New York: McGraw-hill Book Company, Inc., 239 West 39th Street; and London: 6 and 8 Bouverie Street, E.C.4., 1920. (Pp. xv + 441, with 244 figures in the text.) Price 21*s*. net.

Symbiosis: A Socio-Physiological Study of Evolution. By H. Reinheimer. London: Headley Bros., 18 Devonshire Street, E.C.2., 1920. (Pp. xii + 295.)

A Manual of Practical Anatomy. A Guide to the Dissection of the Human Body. By Thomas Walmsley, with a Preface by Thomas B. Bryce, M.D. In Three Parts: Part I, The Upper and Lower Limbs. London: Longmans, Green & Co., 1920. (Pp. viii + 176.) Price 9s. net.

The drawings are very simple, but at the same time very effective, and, above all, do not attempt to crowd too much information into a small space. They are not coloured. The letterpress is of two sizes of type, with very convenient black-lettering. A convenient book for the dissector.

Extra Pharmacopœia. Revised by W. Harrison Martindale, Ph.D., Ph.Ch., F.C.S., and W. Wynn Wescott, M.B., D.P.H. In Two Volumes. Seventeenth Edition. Vol. I. London: H. K. Lewis, 1920. (Pp. xxxix + 1115.) Price 27*s*. net.

The successive editions of this book, so useful to medical men, seem to be endless, and every edition contains some changes owing to the rapid development of medical science. The last edition was issued in January 1915, and much water has run under the bridge since then.

A Synopsis of Medicine. By Henry Letheby Tidy, M.A., M.D., B.Ch. (Oxon), F.R.C.P. (Lond.). Bristol: John Wright & Sons; London: Simpkin, Marshall, Hamilton, Kent & Co.; Toronto: The Macmillan Company of Canada, 1920. (Pp. xv + 952.) Price 26*s*. net.

It is doubtful whether books of this kind are really very useful, because one cannot correctly concentrate into one brief volume all our present knowledge of diseases. Authors of such books are often compelled to describe diseases with which they are not familiar from information given to them by other experts—real or alleged. On looking through these books, we always pick out subjects which we ourselves know about, and I have consequently glanced through the chapter on Malaria. This, I fear, contains several errors. For instance, Grassi and Bastianelli did not prove that the mosquito cycle of the human parasites is limited to Anophelines, because this has not been proved yet, and it was I who showed that these parasites do not develop in a number of kinds of *Culex*. Nor did Sambon and Low's experiments in Italy prove anything of importance; nor did infection of Thurburn Manson in England complete the proof of transmission by mosquitoes; nor can it be said that Marchiafava and Celli described the parasites fully in 1885. The passage on treatment is equally unsatisfactory, and five grains of quinine twice a week is not sufficient to keep off relapses. Probably, however, other subjects are dealt with more correctly. R. R.

War against Tropical Disease: being Seven Sanitary Sermons addressed to all Interested in Tropical Hygiene and Administration. By Andrew Balfour, C.B., C.M.G., M.D., B.Sc. (Public Health), F.R.C.P. (Edin.), D.P.H. (Camb.), Director-in-Chief Wellcome Bureau of Scientific Research. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1920. (Pp. 220.) Price 12*s*. 6*d*. net.

Mémoires sur la Respiration et la Transpiration des Animaux. Par Antoine-Laurent Lavoisier. Paris: Gauthier-Villars et Cie, Quai des Grands-Augustins, 55, 1920. (Pp. viii + 67.)

Observations et Expériences Faites sur les Animalcules des Infusions. Par Lazare Spallanzani. Paris: Gauthier-Villars et Cie, Quai des Grands-Augustins, 55, 1920. (Vol. I, pp. viii + 105, and Vol. II, pp. 122.)

Eugenics, Civics, and Ethics. A Lecture delivered to the Summer School of Eugenics, Civics, and Ethics, on August 8, 1919, in the Arts School, Cambridge. By Sir Charles Walston (Waldstein), M.A., Litt.D. Cantab., M.A., L.H.D., Col. Univ., New York, Ph.D. Heidelberg, etc. Cambridge: at the University Press, 1920. (Pp. 56.) Price 4*s*. net.

A Manual of Psychiatry. Edited by Aaron J. Rosanoff, M.D., Clinical Director, King's Park State Hospital, N.Y.; Lieutenant-Colonel Officers' Section, Medical Reserve Corps, U.S. Army. Fifth Edition, Revised and Enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. xv + 634.) Price 22*s*. net.

The Organisation of Industrial Scientific Research. By C. E. Kenneth Mees, D.Sc., Director of the Research Laboratory, Eastman Kodak Company, Rochester, N.Y. New York: McGraw-Hill Book Company, Inc., 239 West 39th Street; and London: 6 and 8 Bouverie Street, E.C.4., 1920. (Pp. vii + 175.) Price 12*s*. net.

The Year-Book of the Scientific and Learned Societies of Great Britain and Ireland: A Record of the Work done in Science, Literature, and Art, during the Session 1918-19, by numerous Societies and Government Institutions. Compiled from Official Sources. Thirty-Sixth Annual Issue. London: Charles Griffin & Co., Exeter Street, Strand, W.C.2., 1919. (Pp. 336.)

The Group Mind. A Sketch of the Principles of Collective Psychology, with some attempt to apply them to the Interpretation of National Life and Character. By William McDougall, F.R.S., Late Fellow of St. John's College, Cambridge, Fellow of Corpus Christi College, and Wilde Reader in Mental Philosophy in the University of Oxford. Cambridge: at the University Press, 1920. (Pp. xvi + 304.) Price 21*s.* net.

Human Efficiency and Levels of Intelligence. By Henry Herbert Goddard, Director of the Bureau of Juvenile Research of Ohio. Lectures delivered at Princeton University, April 7, 8, 10, 11, 1919. Princeton: Princeton University Press; London: Oxford University Press, 1920. (Pp. 128.) Price 6*s.* 6*d.* net.

Archimedes. By Sir Thomas Heath, K.C.B., K.C.V.O., F.R.S., Sc.D. Camb., Hon. D.Sc. Oxford. Men of Science Series. Edited by S. Chapman, M.A., D.Sc., F.R.S. London: Society for Promoting Christian Knowledge; New York: The Macmillan Company, 1920. (Pp. 58.) Price 1*s.* net paper; 2*s.* net cloth.)

Internal Combustion Engines: Their Principles and Application to Automobile, Aircraft, and Marine Purposes. By Wallace L. Lind, M.S. (Columbia University), Lieutenant-Commander U.S. Navy. New York and London: Ginn & Company. (Pp. 225.) Price 10*s.* net.

A Constitution for the Socialist Commonwealth of Great Britain. By Sidney and Beatrice Webb. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. xviii + 364.) Price 12*s.* 6*d.* net.

Grosses Wörterbuch Deutsch-Ido. In Uebereinstimmung mit den Beschlüssen der Ido-Akademie, und revidiert von Kurt Feder. Paris: Imprimerie Chaix, 11 Boulevard Saint-Michel, 1920. (Pp. xviii + 823.) Price 17.50 frs.

The Idea of Progress. An Inquiry into its Origin and Growth. By J. B. Bury, Regius Professor of Modern History, and Fellow of King's College, University of Cambridge. London: Macmillan & Co., 1920. (Pp. xiv + 377.) Price 14*s.* net.

The Idea of Progress. By W. R. Inge, C.V.O., D.D., Hon. Fellow of Hertford College. Delivered in the Sheldonian Theatre, May 27, 1920. The Romanes Lecture, 1920. Oxford: at the Clarendon Press, 1920. (Pp. 34). Price 2*s.* net.

ANNOUNCEMENT

The Second International Congress of Comparative Pathology is holding its meetings in Rome during April 1921, and the Secretary-General is prepared to receive titles and abstracts of papers dealing with Comparative Pathology of Man, Animals, and Plants, provided that the communications do not reach him later than December 15, 1920. Those desiring to attend the Congress are requested to communicate either with M. le Prof. Mario Levi della Vida, Secrétaire-Général du Comité d'Organisation, 58 Via Palermo, Rome, or with Prof. George H. F. Nuttall, Sc.D., F.R.S., Longfield, Madingley Road, Cambridge, England.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By DOROTHY M. WRINCH, Fellow of Girton College, Cambridge, Member of the Research Staff, University College, London.

IN the algebra of propositions elaborated by Boole and the earlier symbolic logicians, the three operations of disjunction "not p and q," conjunction "p and q" and negation "not p" were taken as undefined. Sheffer (*Trans. Amer. Math. Soc.*, **14**, 1913), however, showed that the three operations are not independent, and can be obtained from one operation, which may be written p/q, which turns out to have the properties of "not p or not q." Nicod (*Proc. Camb. Phil. Soc.*, **19**, 1916), using the one fundamental operation p/q, substantially reduced the number of primitive propositions elaborated by Russell and Whitehead for the construction of the logical calculus.

In the field of ordinary algebra Huntington (*Trans. Amer. Math. Soc.*, **6**, 1905) developed a set of thirteen postulates for fields making use of multiplication and addition as undefined notions. Norbert Wiener (*Trans. Amer. Math. Soc.*, **21**, 1920) performs the same task for these undefined notions as Sheffer performed in 1913 for the undefined notions of the algebra of logic. The one undefined operation Wiener uses is represented by the symbol x @ y. In terms of @ multiplication and addition can be defined. This operation turns out to have the formal properties associated with x/y in algebra.

It was established by L. L. Dines in 1915 (*Bull. Amer. Math. Soc.*, **21**) that Sheffer's five postulates for Boolean algebras (in terms, of course, of *one* undefined operation) are independent in the ordinary sense that no one of the postulates is implied by the other four. E. H. Moore, in the New Haven Mathematical Colloquium (*Yale University Press*, 1910), gave a new definition for the independence of a set of postulates, as follows: a set of m postulates is completely independent if, and only if, there are no implicational relations existing between the properties defined, either by the postulates or by the negatives of the postulates. For, if the truth or falsity of one postulate implies either that another postulate is true, or that it is false, it would seem either that the two postulates are concerned with two

aspects of the same fundamental property, or that there are two fundamental properties involved in such a manner that one of the postulates, at least, deals with both properties. J. S. Taylor (*Bull. Amer. Math. Soc.*, **26**, 1920) modifies the first of Sheffer's five postulates, and proves the complete independence of the five postulates in their emended form. As Sheffer stated them the negative of the first implies the third, fourth and fifth postulates of the set.

In the *Proc. Roy. Soc. Edin*,, **40**, 1920, F. L. Hitchcock, in a paper entitled "An Identical Relation connecting Seven Vectors," develops an identical equation satisfied by any seven vectors, and also satisfied by an arbitrary quadratic function, and explains methods by which new identities can be derived from any vector identity.

A. D. Pitcher (*Bull. Amer. Math. Soc.*, **26**, 1920) discusses how far the property of *coherence* (introduced and defined in a paper by A. D. Pitcher and E. W. Chittenden, *ibid.*, **19**, 1913) belongs to the systems introduced by E. H. Moore (*ibid.*) in 1910.

W. E. Milne (*Bull. Amer. Math. Soc.*, **26**, 1920) approaches the study of infinite systems of function from an elementary point of view, and derives results of considerable generality. The discussion is limited to real functions of a single real variable, but the methods used can be extended to more general systems.

Norbert Wiener (*Ann. Math.*, **21**, 1920), develops the necessary and sufficient condition that a bilinear operation in two variables should generate by iteration all rational operations with rational coefficients. This is a particular case of the general problem of determining what operations any given bilinear operation will generate by iteration. In the *Bull. Amer. Math. Soc.*, **27**, 1920, he develops methods of attack which yield, in particular, an important necessary condition that each of two operations generate the other by iteration. The complete solution of the problem is still to be accomplished.

J. Mercer, in a paper entitled "Symmetrisable Functions and their Expansions in Terms of Biorthogonal Functions" (*Proc. Roy. Soc.*, **93**, 1920), announces certain results relative to the expansion of a symmetrisable function $k(s, t)$ in terms of a complete biorthogonal system of fundamental functions which belong to $k(s, t)$ regarded as the kernel of a linear integral equation.

E. Borel, in a paper entitled, "Sur la classification des ensembles de mesure nulle" (*Bull. Math. Soc. France*, **47**, 1919), studies and classifies by means of the elementary properties of decimal fractions certain classes of zero measure.

Frédéric Riesz contributes a paper on Lebesgue's integral to *Acta Math.*, **42**, 1920. His object is to make the notion of an integral independent of the theory of measure. His point of view, therefore, differs in an important respect from that of Borel, who begins by introducing his own notion of measure, and so is limited to less general sets than those which can be dealt with by the methods of this paper.

P. J. Daniell (*Ann. Math.*, **21**, 1920) gives further properties of the general integral defined in 1918 (*ibid.*, **19**, 1918). This definition covers the Lebesgue integral and the Radon-Young integral as particular cases.

R. L. Moore (*Trans. Amer. Math. Soc.*, **21**, 1920) gives a definition of a simple continuous arc joining A to B, which does not stipulate that the set of points M should be bounded, nor that it should contain no proper connected subset, containing both A and B. He shows that in a Euclidean space of two dimensions every point-set that satisfies this definition is a Jordan curve. The advantages of the proposed definition are explained, and other definitions introduced.

J. W. Alexander (*Ann. Math.*, **21**, 1920) gives a proof of Jordan's Theorem, that a simple closed curve divides the plane into two regions, which was first proved by O. Veblen in 1906 (*Trans. Amer. Math. Soc.*, **66**). The argument is based on elementary combinatorial properties of chains or systems of polygons.

M. Jaiuna (*Tôhuku Math. Journ.*, **17**, 1920), gives an elementary proof of the theorem about Bertrand curves proved by Briosche in *Bull. Soc. Math. France*, **17**.

In a paper entitled "On the Term by Term Integration of an Infinite Series over an Infinite Range and the Inversion of the Order of Integration in Repeated Infinite Integrals" (*Proc. Camb. Phil. Soc.*, **20**, 1920), S. Pollard investigates first under what conditions on the Lebesgue Theory of integration term by term integration of an infinite series is permissible. He obtains much wider conditions than can be obtained on the Riemann Theory, when the condition of uniform convergence is almost always involved. The problem for infinite integrals is dealt with on similar lines.

In a paper entitled "Sur les fonctions de lignes implicites" (*Bull. Soc. France*, **48**, 1920), Paul Lévy considers the problem of the correspondence between two functions $u(s)$ and $v(s)$ both defined for a certain continuous series of values of s. He studies the general characteristics that the relation between v and v must have, if v is uniquely determined by u, making use of Hadamard's results (*Bull. Soc. France*, **34**, 1908).

G. Mittag-Leffler continues, in *Acta Math.*, **42**, 1920, in a sixth note his series of important papers entitled "Sur la

représentation analytique d'une branche uniforme d'une fonction monogène," of which the fifth was published in 1904.

R. D. Carmichael, in a paper "On the Convergence of Certain Classes of Series of Functions" in *Amer. Journ. Math.*, **42**, 1920, attacks the central convergence problem for series $S(x)$ of the form

$$S(x) = \sum_{n=0}^{\infty} c_n e^{v_n(x)}$$

and also for series $T(n)$ of the form

$$T(x) = c_o + \sum_{n=1}^{\infty} c_n P_1(x) P_2(x) - P_n(x)$$

where c c_1 . . . are constants and $v_o(x)$, $v_2(x)$ . . ., $P_1(x)$, $P_2(x)$ . . . are a given sequence of functions. The functions $v_r(x)$ are defined in such a way that a considerable variety of important classes of series are included under the general form $S(x)$. We may cite, as examples, an ascending or descending power series of the generalised Direchlet form

$$S(x) = \sum_{n=o}^{\infty} c_n e^{-\lambda_n(x)}$$

where λ_o λ_1 λ_2 . . . is an increasing sequence of real numbers tending to infinity.

Alfred Kienast (*Proc. Camb. Phil. Soc.*, **20**, 1920) proves certain results with respect to the relation between the nature of Hölder's mean of a sequence of complex numbers a_1 a_2 . . . a_n . . ., viz. $\lim_{n \to \infty} h_n(k)$,

$$\text{where } h_n^{(o)} = a_1 + \ldots + a_n$$

$$h_n^{(1)} = \frac{1}{n}(h_1^o + \ldots + h_n^o)$$

$$\ldots\ldots\ldots\ldots\ldots\ldots$$

$$h_n^{(k)} = \frac{1}{n}(h_1^{k-1} + \ldots + h_n^{k-1}),$$

and that of $\lim_{n \to \infty} s_n^k$,

$$\text{where } s_n^{(o)} = a_1 + \ldots + a_n$$

$$s_n^{(1)} = \frac{1}{n}(s_1^o + \ldots + s_{n-1}^o)$$

$$\ldots\ldots\ldots\ldots\ldots\ldots$$

$$s_n^{(k)} = \frac{1}{n}(s_k^{k-1} + \ldots + s_{n-1}^{k-1}),$$

proving among other results that the existence of either limit

involves the existence of the other limit, and that when the function s^k_{n+k} oscillates finitely the function h_n^k oscillates between the same limits.

G. H. Hardy, *Math. Zeitschrift*, **6**, 1920, gives a simple proof of Hilbert's Theorem that the series already proved by Hilbert, Weyl, Weiner and Schur

$$\sum_{m,n}^{\infty} \frac{a_m a_n}{m+n} \qquad a_m > 0$$

is convergent whenever Σa_m^2 is convergent.

Practically nothing has as yet been done in the application of methods of summation of divergent series to double series.

Lloyd L. Smail, *Ann. Math.*, **21**, 1920, gives a general theorem about a method of summation of double series analogous to his general method of summation for simple series given in 1918 (*ibid.* 20).

C. A. Fischer, *Bull. Amer. Math. Soc.*, **27**, 1920, gives the necessary and sufficient conditions that a linear transformation may be completely continuous. These results complete the results of the same author published in 1919 (*ibid.*, 25), and are important in that there it is thus proved that a certain part of the theory of integral equation due to Fredholm is applicable to Stieltjes's integral equations of certain types.

In a paper entitled, "Sur les développements en série suivant les inverses de polynomes donnés" (*Bull. Soc. Math. France*, **48**, 1920), P. Appell collects the principal results given in his papers of 1913 (*Comptes Rendus*, **37**, *Bull. Sci. Math.*, **37**) and adds new matter.

R. D. Carmichael, *Ann. Math.*, **22**, 1920, discusses the expansion of certain analytic functions in series.

C. N. Moore, in a paper entitled, "On the Summability of the Developments in Bessel's Functions" (*Trans. Amer. Math. Soc.*, **21**, 1920), establishes sufficient conditions for the summability (*Cesàro*) at the origin and the uniform summability near the origin of the development in Bessel functions of an arbitrary function. This paper forms an important supplement to the paper by W. H. Young (*Proc. Lond. Math. Soc.*, **2**, **18**, 1919), in which the convergence and the summability are considered, but not the behaviour of the series near the origin. Many of the lemmas obtained incidentally in the process of proving the main theorems have an interest of their own and applications to Fourier series.

Abanibhushan Dattor (*Bull. Calcutta Math. Soc.*, **11**, 1920) gives a generalisation of Neumann's Expansion in a series of Bessel functions in the forms :

$$fz = \Sigma b_n J_{n+a}(kz) \quad : \quad fz = \Sigma b_n J_{n+a}(kz) z^{-a}$$

where the coefficients b_n, etc., are independent of z and a is any positive integer or fraction.

L. J. Mordell (*Quart. Journ.*, 48, 1920) investigates the value of the definite integral

$$\int_{-\infty}^{+\infty} \frac{e^{at^2+bt}}{e^{ct}+d}\,dt$$

in the general case when a, b, c, d are real or complex, and when path of integration is the real axis or if the integrand has any singularities on that path the real axis suitably indented.

J. W. Nicholson (*Quart. Journ.*, 48, 1920), in a paper entitled " A Generalisation of a Theorem due to Sonine," proves that if $m > -\frac{1}{2}$

$$\int_0^\infty J_m(a_1x)J_m(a_2x)\ \ldots\ J_m(a_nx)x^{-mn+2m+1}\,dx = 0,$$

where $(a_1\, a_2 \ldots a_n)$ cannot form the sides of a polygon of n sides. The Theorem of Sonine given in 1880 (*Ann. Math.*, 16) was of

the form ; if $m > -\frac{1}{2}$

$$\int_0^\infty J_m(ax)\,J_m(bx)J_m(cx)x^{-m+1}dx = 0$$

unless (a, b, c) can form the sides of a triangle, in which case its value is

$$\frac{[(a+b+c)(a+b-c)(c+a-b)(b+c-a)]^{m-\frac{1}{2}}}{\left[\sqrt{\pi 2^{3m-1}}\left(m-\frac{1}{2}\right)a^m b^m c^m\right]}$$

The value of the integral with n Bessel functions in the integrand when $(a_1\, a_2\ \ldots\ a_n)$ can form the sides of a polygon of n sides cannot in general be obtained in a simple manner. The evaluation is performed in this paper for the case when the functions are of zero order, in terms of the complete elliptic integral k.

This theorem is also the subject of a short communication to the *Edin. Math. Soc.* (*Proc.*, 37, 1919) by John Dougall. A proof is given of Sonine's Theorem based on the Theory of the Potential.

G. H. Hardy and J. E. Littlewood, in the *Quart. Journ.*, 48,

1919, in a paper entitled, "A New Solution of Waring's Problem," give a short account of a solution of Waring's Problem of the *existence* of the function $g(k)$ [solved by Hilbert in 1909 (*Gött. Nachrichten*, 1909, and *Ann. Math.*, **67**, 1909)] which they have recently discovered, which is important in that it brings Waring's Problem, which is one of many similar problems of Combinatory Analysis, into relation with the transcendental side of the Analytic Theory of Numbers. This method yields a great deal more than can be obtained by more elementary methods. The paper opens with a short account of the main features of this method, which is applicable to almost any problem concerning the decomposition of integers into parts of a particular kind.

L. Tschakaloff (*Ann. Math.*, **80**, 1919) investigates the arithmetical properties of the infinite series

$$\sum^{\infty} a^{-\frac{\nu(\nu-1)}{2}} x^{\nu}.$$

In a paper entitled "An American Tournament treated by the Calculus of Symmetric Functions" (*Quart. Journ.*, **49**, 1920) P. A. MacMahon analyses by means of the powerful calculus of symmetric functions the events in a tournament of n players, where each player plays every other player, and when the players are or are not in an assigned order.

H. W. Richmond (*Proc. Camb. Phil. Soc.*, **22**, **19**, 1920) discusses the classical problem of the determination of the formulæ for sets of four integers, such that the sum of their cubes is zero.

P. A. MacMahon, in a memoir entitled "Congruences with respect to Composite Moduli," in the *Trans. Camb. Phil. Soc.*, **22**, **21**, 1920, puts together certain results in the Theory of the Residues of Powers with respect to composite moduli.

G. A. Miller (*Trans. Amer. Math. Soc.*, **21**, 1920) investigates the properties of the subgroup of an abelian prime power group which are conjugate under its group of isomorphisms.

Louis C. Mathewson, in a paper entitled "On the Groups of Isomorphisms of a System of Abelian Groups of Order p^m and Type $(n_1 1, 1, \ldots, 1)$, in the *Amer. Journ. Math.*, **22**, 1920, studies the groups of isomorphisms of the system of abelian groups of order p_m, type $(n_1 1 \ldots 1)$, $n > 1$, and shows that these groups may be built upon the group of isomorphisms of an abelian group which contains no operations of order greater than p.

G. A. Miller, in a paper on Abelian Groups (*Mess. Math.*, **49**, 1920), reduces the determination of the properties of the group of isomorphisms of G for a general abelian prime power

group to the determination of these properties when G is of type (1, 1, 1, . . .).

P. Faton contributes two memoirs on functional equations to the *Bull. Math. Soc. France*, **47**, 1919; **48**, 1920.

G. A. Bliss (*Trans. Amer. Math. Soc.*, **21**, 1920) gives results of a general character with respect to Differential Equations containing arbitrary functions. The investigations were suggested by the problems which arise in ballistics in the computation of trajectories disturbed by the wind and other factors. The results given cover these problems, but are of a more general character than the ballistic problem requires.

Applications of these results are given in a second paper by the same author (*ibid.*, **21**, 1920).

P. Boutroux (*Ann. Math.*, **22**, 1920) suggests a method by which in the case of a number of differential equations of the first order the general solution in its whole field of its existence is represented to any required degree of approximation, the representation displaying the fundamental properties of the equation, showing how the internal conditions are involved. The method is also of interest as introducing certain multiform functions (associated with the solutions of the differential equations) which have remarkable automorphic properties.

J. L. Walsh, in a paper "On the Solution of Linear Equations in infinitely many Variables by Successive Approximations" in the *Amer. Journ. Math.*, **22**, 1920, gives a number of new conditions under which a system of equations of the type

$$a_{r1}x_1 + a_{r2}x_2 + a_{r3}x_3 + \dots = C_r. \quad (r = 1, 2, \dots)$$

can be solved by successive approximations. This method had previously been used chiefly for Hilbert Space [*i.e.*, the space of points (x_k) for which $\sum_{k=1}^{\infty} (x_k)^2$ converges] with corresponding restrictions on a_{rs} and c_r.

R. D. Carmichael, in a paper entitled "On Sequences of Integers defined by Recurrence Relations" (*Quart. Journ.*, **48**, 1920) develops certain general properties of the sequence of integers $u_0; u_1, u_2 \dots$ defined uniquely in terms of the given initial numbers $u_0\, u_1 \dots u_{k-1}$ by the recurrence relation

$$u_{x+k} + a_1 u_{x+k-1} + a_2 u_{x+k-2} + \dots + a_k u_x = a$$

in which $a, a_1 \dots a_k$ are given integers. The theory of such sequences affords an extensive generalisation of the theorems of Fermat and Wilson.

Sir T. Muir, in a note on compound determinants expressible as simple determinants (*Quart. Journ.*, **48**, 1920) works on the fact recently published in the *Proc. Math. Soc. Edin.*, **36**, by Professor Whittaker that certain compound determinants are expressible as simple determinants, and obtain some more general results.

Sir T. Muir contributes a seventh list of writings on determinants to the *Quart. Journ.*, **49**, 1920. This list covers the writings on the subject up to the end of 1919.

Frank Morley (*Ann. Math.*, **22**, 1920) discusses the relations between the centroids, symmedian points and other points attached to a triangle, in the case of four triangles whose vertices are formed by the omission one at a time of four given points which lie on a circle.

An addition has been made to the series of *Cambridge Tracts in Mathematics and Mathematical Physics*. R. H. Fowler, in "The Elementary Differential Geometry of Plane Curves" (Tract number 20), presents a precise account of the elementary differential properties of plane curves. No suitable connected treatment of the subject has hitherto been available in English.

Feuerbach's Theorem states that the existence of a circle (the nine point circle) which touches the inscribed circle of a triangle internally and each of the three described circles externally. This theorem is extended to apply to triangles formed by arcs of circles.

T. C. Lewis (*Amer. Journ. Math.*, **42**, 1920), in a paper entitled "Is there an Analogue in Solid Geometry to Feuerbach's Theorem?" considers this question put by L. Coolidge in his *Treatise on the Circle and the Sphere*, and shows that the analogue for three dimensions does not in general hold.

L. E. Wear, in a paper on "Self-dual Plane Curves of the Fourth Order" in *Amer. Journ. Math.*, **42**, 1920, discusses, in the case of quartic curves, those curves called by Appel autopolaire, which are identical with their reciprocals. They turn out to be the limaçon and the degenerate case of two conics.

R. W. Winger, in a paper "On the Satellite Line of the Cubic" in the *Amer. Journ. Math.*, **42**, 1920, gives the explicit equation of the satellite, for the general cubic in canonical form.

E. Cartan (*Bull. Math. Soc. France*, **47**, 1919) in a paper entitled "Sur les variétés de courbure constante d'un espace euclidien ou non-euclidien," discusses the general case of p dimensions. His results with respect to 3-dimensional Euclidean space were given in a series of communications to the *Comptes Rendus* (**167**, 1918).

G. H. Hallett, Jr. (*Ann. Math.*, **21**, 1920) deals with Euclidean

and double elliptic geometries of three dimensions, the notions of point and order being undefined.

J. W. Alexander (*Bull. Amer. Math. Soc.*, **26**, 1920) proves for the case $n = 3$, that every closed orientable n-dimensional manifold can be represented on an n-dimensional hypersphere as a Riemann space or generalised Riemann surface. The extension to higher dimensions is perfectly automatic.

The sixth of a set of memoirs by P. Koebe on conformal representation appears in *Math. Zeitschrift*, **7**, 1920.

W. Burnside, in a memoir entitled "On Cyclical Octosection" in the *Camb. Phil. Soc.*, **22**, **20**, 1920, establishes independently the formulæ of the problem of cyclical quartisection, first given (but not passed) by V. A. Le Besgne in 1860, so far as they are necessary for the problem of octosection.

W. F. Sheppard in a memoir entitled "Reduction of Error by Linear Compounding" proves certain general theorems by means of which the analysis required in two problems in the theory of error can be considerably shortened. These problems are as follows. The data are a set of quantities, $u_0\ u_1\ u_2\ \ldots$, which are regarded as representing certain true values, $u_0\ u_1\ u_2\ \ldots$, with errors $e_0\ e_1\ e_2\ \ldots$, so that $u_r = u_r + e_r$. In the first problem it is assumed that the sequence of u's is fairly regular. In the second u_r is a polynomial in r of degree j, and the problem is to find the coefficients in this polynomial by the method of least squares. The general theorems referred to allow other problems in which the assumptions are not so narrow as in these two problems to be dealt with.

In a paper entitled "On a Parabolic Equation of the rth Degree for any Graphically Faired Curve" (*Phil. Mag.*, vol. xl., No. 238) T. C. Tobin gives a convenient and rapid method of obtaining the values of the constants $a_0\ a_1\ \ldots$ in the equation

$$y = a_0 + a_1x + a_2x^2\ \ldots + a_rx^r$$

satisfied by a curve plotted with respect to a set of rectangular axes.

F. H. Safford (*Bull. Amer. Math. Soc.*, **26**, 1920) gives the parametric equations of the path of a projectile when the air resistance varies as the nth power of the velocity.

In *Acta Math.*, **42**, 1920, David Hilbert publishes his address to the Academy of Sciences of Göttingen delivered in 1917 and L. P. Eisenhart his address to the joint meeting of the American Mathematical Society and the Mathematical Association of

America delivered in 1917 on Gaston Darboux (1842–1917). Both papers contain valuable sketches of Darboux's work.

G. A. Bliss (*Bull. Amer. Math. Soc.*, **26**, 1920), in a presidential address, gives, in addition to an interesting account of the recent history of the Calculus of Variations, a new method of treating the second variation.

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RITT, J. F., On the Iteration of Rational Functions, *Trans. Amer. Math. Soc.*, **21**, 1920.

STORLOW, S., Remarques sur les ensembles de mesure nulle à plusieurs dimensions, *Comptes Rendus*, **171**, Sept. 20, 1920.

BRAY, H. E., A Green's Theorem in Terms of Lebesgue Integrals, *Ann. Math.*, **21**, 1920.

DANIELL, P. J., Stieltjes' Derivatives, *Bull. Amer. Math. Soc.*, **26**, 1920.

NEDER, L., Konvergenzbesitztum der Potenzreihen Stetiger Funktionen auf dem Rande des Konvergenzkreises, *Math. Zeitschrift*, **6**, 1920.

NEDER, L., Über die Fourier Koeffizienten der Funktionen von Beschränkter Schwankung, *Math. Zeitschrift*, **6**, 1920.

BOHR, H., Zur Theorie der allgemeinen Dirichletschen Reihen, *Math. Ann.*, **79**, 1919.

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WILSON, W. H., On Certain Related Functional Equations, *Bull. Amer. Math. Soc.*, **26**, 1920.

SMIRNOFF, V., Sur quelques points de la théorie des équations différentielles linéaires du second ordre et des fonctions automorphes, *Comptes Rendus*, **171**, Sept. 20, 1920.

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RÉMOUNDOS, J., Les singularités des équations différentielles et les séries sommables, *Bull. Soc. Math. France*, **48**, 1920.
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GODEAUX, LUCIEN, Sur les surface algébriques possédant un système simple dont les courbes contiennent une involution, *Bull. Math. Soc., France*, **48**, 1920.
DRACH, J., D'équation differenticlle de la balistique extérieure et son intégration par quadratures, *Ann. Sci. de l'École Norm., Sup.*, **56**, 1920.
GRONWALL, T. H., Qualitative Properties of the Ballistic Trajectory, *Ann. Math.*, **22**, 1920.

ASTRONOMY. By H. SPENCER-JONES, M.A., B.Sc., The Royal Observatory, Greenwich.

The Extension of the Ultra-violet Spectrum.—When the solar spectrum is observed with prisms and lenses of glass, the violet limit appears to the normal eye to occur at a wave-length of about 4,000 angstroms, near the Faunhofer H lines. By the use of the photographic plate and the employment of optical apparatus made of quartz, the spectrum can be extended without difficulty to a wave-length of about 2,000 angstroms. The absorption of the air and of the lenses and prisms then becomes important, and the extension of the spectrum to wave-lengths much shorter than this value becomes difficult. The region of wave-lengths from 4,000 to 2,000 angstroms is commonly referred to as the ultra-violet region.

The pioneer work in the extension of the spectroscopy into the extreme ultra-violet (*i.e.* into wave-lengths shorter than 2,000 angstroms) was performed by Victor Schumann about thirty years ago. Schumann was a successful business man who was over forty years of age before he had sufficient leisure to devote to scientific work. When he took up the pursuit of spectrum analysis, he soon perceived the important parts played by the absorption of the gelatine film on the photographic plate and by the absorption of the air in the apparatus. By employing special photographic plates with an emulsion nearly free from gelatine, using lenses and prisms of fluorite—which is more transparent than quartz to short waves—and devising a spectroscope which could be used in a vacuum, he was able to

extend the ultra-violet spectrum down to λ 1,200, a limit set by the absorption of fluorite.

Schumann's apparatus was not suitable for the accurate measurement of the short waves, because of the complex dispersion curve of fluorite in this region, although admirably adapted for general exploration purposes. This difficulty was removed by T. Lyman, who, by employing a concave grating of speculum metal, paved the way for a further advance. The grating spectroscope was contained in a vacuum chamber containing hydrogen at a low pressure, the light from the discharge tube being admitted through a fluorite window. Special plates, prepared according to Schumann's methods, were used. With a disruptive discharge, an extension of the spectrum down to about λ 900 was secured, and fairly accurate measures of the principal lines of many substances in the new region were made.

The work of Laue, W. H. and W. L. Bragg, and others on the diffraction of X-rays by crystals has shown conclusively that such rays are a form of ether vibration with the extremely short wave-length of about one angstrom. Lyman's work, therefore, left a gap of about nine hundred units between the ultra-violet extension of the visible spectrum and the X-ray region.

A remarkable advance towards bridging this gap has recently been secured by R. A. Millikan in the Ryerson Laboratory of the University of Chicago (*Physical Review*, **12**, 168, 1918; *Science*, **19**, 138, 1919; *Astroph. Journ.*, **52**, 47, 1920). The essentially new features in Millikan's work consist in (1) the employment of a very perfect vacuum, which eliminates all absorption by residual gases; (2) the use of a source consisting of very high potential sparks beween metal electrodes placed very close together, so insuring the production of the highest frequencies; (3) the use of special gratings, so devised as to throw as much light of short wave-length as possible into the first-order spectrum, and with sufficient regularity of ruling to produce good images when the ratio of grating-interval to wave-lengh is as much as 70, instead of having a value of 3 to 6, as employed in common practice, or of about 20, as in Lyman's work. The gratings used were ruled in the Ryerson Laboratory, and are the outcome of long experience in grating ruling; it is probable that the limitations imposed by the use of a grating have been reduced to a minimum. It is considered by Prof. Millikan that all possible question as to the validity of the results has now been removed: a detailed account of the various difficulties encountered and the methods of overcoming them is left for future publication, but the announcement is made that the ultra-violet spectrum has been carried down to a wave-length of 202 angstroms.

The spectra were obtained by intermittent sparking between

close electrodes (0·1 to 2 mm. apart) with a battery of Leyden jars charged to a potential of several hundred thousand volts by a powerful induction coil. For the maintenance of this type of spark the pressure must be kept below 10^{-4} mm.: this was secured by using a mercury diffusion pump, kept in continuous operation. The shortest wave-lengths thus far obtained in the spectra of carbon, zinc, iron, silver, and nickel are 360·5, 317·3, 217·6, 260, and 202 angstroms respectively. The spectrum of carbon was found to be particularly brilliant, more than fifty bright lines with wave-lengths shorter than 1,000 angstroms being obtained; although the other metallic spectra obtained showed a greater number of lines, they were for the most part weak and somewhat diffuse. It seems to be certain that the intense spark employed causes the carbon to volatilise, and that the lines obtained are true gas lines. Prof. Millikan advances reasons for believing that the lines obtained belong to the L-series of the X-ray spectrum of carbon. In favour of this supposition is the fact that the spectrum stops abruptly at a wave-length of 360·5 angstroms, whilst lines of shorter wave-length were obtained with the other elements examined, and this limit is about the wave-length at which the L spectrum of carbon might be expected to commence, if extrapolation is justifiable from Moseley's measurements on the L rays of elements having atomic numbers from 30 to 90. Additional facts in support of this conclusion are (1) that the relative wave-lengths and intensities of the three shortest lines in the spectrum are closely similar to those shown by the three main lines of the L-series; (2) that the strong line corresponding to $L\alpha$ in carbon is double, and it is well known that the alpha line of the L-series is a double in all elements.

This conclusion, if justified as it appears to be, is of great importance, because no X-ray spectra of the L-series have ever been taken with crystal gratings in the case of elements of atomic number less than 30; and for so low an atomic number as carbon the grating-space of a crystal grating would be too small in comparison with the wave-length for sharp images to be obtained. Further, on this hypothesis, no other lines in the carbon spectrum are to be expected until the K-series is reached, the longest wave-length of which should be about 45 angstroms. At present, therefore, Prof. Millikan has not only very considerably extended our knowledge of spectra in the extreme ultra-violet region, but he has also nearly succeeded in bridging the gap between the visible spectrum and the X-rays of shortest frequency.

Stellar Distribution and Motions.—One of the most important problems of stellar statistics is the determination of the laws according to which the stars are distributed throughout

space in regard to numbers and luminosities. The two laws which govern the distribution are the density law—which gives the number of stars per unit volume of space in different parts of the system—and the luminosity law— which gives the proportion of stars between different limits of absolute brightness. It is generally tacitly assumed that the density law is the same for all luminosities, and that the luminosity law is the same at all distances. These assumptions considerably simplify the mathematical discussion, and enable a satisfactory representation of the observational data to be given within the limits of possible error. The data referred to are the counts of the numbers of stars between given limits of magnitude, and the mean parallaxes of stars of given magnitudes.

The validity of these assumptions has been discussed in a recent paper by J. Halm (*M.N., R.A.S.*, **80**, 162, 1919). He combines the two laws into one function, which he calls the distribution function F, defined so that the number of stars in a cone of solid angle $d\omega$, with its apex at the sun, situated between the distances $r \pm \frac{1}{2}dr$, and whose apparent magnitudes are between $m \pm \frac{1}{2}\,dm$, is given by

$$dN = F\ r^2 dr dm d\omega$$

where F is a function of r, m and the galactic latitude and longitude. Since the absolute luminosity, M, is a function only of m and r, F can be regarded also as a function of r, M and the galactic latitude and longitude.

A particular form of the expression F is $F[ar, h(M+A)]$, where a and A are functions of position, and h is a constant. Halm shows that this special functional form is in agreement with observational data. The general assumptions referred to above are equivalent to a still further specialisation, in which F is assumed to have the form

$$F = f(ar)\psi[h(M+A)]$$

where f is a function of r, and the galactic latitude and longitude, but is independent of M, whereas ψ is a function M only, and is independent of the space co-ordinates.

Halm then proceeds to show that this assumption does not lead to the *only* forms of f and ψ which satisfy the statistical data. He considers an alternative possibility, in which the density is constant throughout space, so that f is a constant, whilst the luminosity law is different at different distances; *i.e.* A is a function of the position. These suppositions can be made to satisfy the counts of stars of different magnitudes absolutely, and the mean parallaxes of stars of a given magnitude are the same as those obtained on the more usual assumptions to within the limits of observational error.

To discriminate between the two possibilities, he considers the distributions in distance of stars of given apparent magnitude obtained from the two sets of assumptions, and compares these with the results of the investigations of Dyson and Eddington concerning the distribution in distance of the stars contained in Carrington's and Boss's catalogues respectively. The representation appears to be much better on the assumption of a constant star density than on that of a luminosity law independent of distance.

The counts made by Shapley of the stars in the cluster Messier 13 are utilised to support this conclusion. The stars in a stellar cluster can all be assumed to be at the same distance from the sun, and therefore the absolute magnitudes differ from the apparent magnitudes only by a constant, and the counts can be taken to refer to absolute magnitudes. It is shown to be impossible for the assumption of a uniform luminosity law to represent the counts; but if the star density is assumed to be the same at all points of the cluster, a luminosity law can be found which will enable the observational data to be satisfied. It is, however, doubtful to what extent results obtained in the case of a star cluster can be regarded as valid for our own stellar system. Halm appears to consider the fact that in certain clusters a galactic condensation has been found as affording some justification for the assumption. But it is impossible to suppose that the density in a cluster can be constant at all distances from its centre; there must be a limit beyond which any increase in exposure would only reveal the fainter field stars which do not belong to our system. It is, moreover, doubtful to what extent the law obtained by Dyson and Eddington is valid, as the distances are measured from the sun, which is not at the centre of the galactic system: it is therefore improbable that the distribution is dependent only upon the distance, and not also upon the direction. The assumption of uniform density may, however, be a first approximation in the neighbourhood of the sun, and the most important result brought out by Halm in his discussion is that the available statistical evidence does not seem to support the generally accepted statement that the luminosity law is the same at all points in space.

In another paper, entitled "On the Systematic Motions of Stars derived from Radial Velocities" (*M.N.*, *R.A.S.*, **80**, 682, 1920), Halm analyses the available radial velocity determinations on the assumptions (1) that, apart from their random velocities, the stars move in two interpenetrating drifts; (2) that, in addition to these two drifts, there is a third drift which is at rest in space. Hahn had previously brought forward evidence in favour of such a third drift, derived from an

analysis of proper motions. The discussion of the radial velocities strongly confirms the existence of the third drift. It is further shown that this drift holds a most prominent position in the B-type stars, which show the ordinary star-streaming but feebly, and that it is shown least clearly in the types F and G. These stars are on the average the nearest, and this leads Halm to conclude that the double-drift phenomenon is of a local character, whilst stars at considerable distances from the sun belong pre-eminently to the third drift.

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METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, Air Ministry, London.

Preliminary Steps in the making of Free-air Pressure and Wind Charts, by C. Le Roy Meisinger (*Monthly Weather Review*, May 1920, vol. xlviii, No. 5).

In this paper an attempt is made to find for three American stations the most probable mean temperature up to a given level in the free air, when the temperature and direction of the wind on the ground only are known. Should it be possible to do this with reasonable accuracy, isobars might be drawn for various heights, and the problem of forecasting the wind in the free air might be approached more directly than by the method at present in use, in which isobars drawn for sea-level are made use of, together with a certain amount of direct information about the upper winds. It is true that the wind theoretically required to balance the horizontal gradient of pressure at the surface, though not found at the surface because of friction with the ground, often prevails at 500 metres or 1,000 metres, and sometimes at even higher levels, but this is only because the horizontal pressure gradient frequently changes very little up to moderate altitudes. It would be more satisfactory to begin by drawing the isobars in the free air, and then to proceed to calculate the corresponding wind. A knowledge of the direction of the wind would be of little use in North-West Europe for estimating free-air temperatures, but in America there is a much closer connection between the two.

The three stations employed in this research were Mount Weather (Virginia), Drexel (Nebraska), and Ellendale (North Dakota), 3,000 kite records of free-air temperature being available. The mean temperature of the air-column from the surface up to 2,000 metres height was tabulated for a large number of cases. The results were classified according to the wind direction for each month of the year. It was found that these differences depended to a marked degree upon the direction of the wind, the N.W. winds on the whole bringing cold air, and the S.E. winds warm air, but this effect was much interfered with by another factor, namely the seasonal variation of temperature. For instance, in the case of Ellendale, which is at a great distance from the sea, the differences in January are all positive, so that the mean temperature up to 2,000 metres is greater than the surface temperature for all wind directions.

This is due to the low temperature generally found at the surface at about 8 a.m., when most of the observations were made. The topographical factor further complicates matters, *e.g.* at Mount Weather in January it is the westerly winds which are the coldest, and the easterly the warmest, because of the near neighbourhood of the Atlantic towards the east.

Owing to these various causes, there is no simple relationship between the temperature differences and the wind, and it was found necessary to make a diagram for each station, having the wind direction and time of year as ordinates and abscissæ respectively, on which isopleths (lines of equal temperature difference) could be drawn. As was only to be expected, the form of the isopleths was found to be very different for each station. Similar diagrams were made for a height of 1,000 metres. The method of use is to find the point on the diagram corresponding to the given time of year and direction of the wind, and then observe which isopleth is nearest to this point. The value of this isopleth must then be subtracted from the surface temperature, so as to obtain the best estimate of the mean temperature required.

The author goes rather fully into the question as to how accurate these estimates are, and those of the pressure at the top of the column calculated from them. He uses modern statistical methods to determine the probable error, and finds this to be the order of 1 or 2 millibars. From this he hopes for good results from the method when free-air observations are extended to a larger number of stations.

Winds and Temperature Gradients in the Stratosphere.—G. M. B. Dobson, in the *Quarterly Journal of the Royal Met. Soc.* (January 1920, vol. xlvi, No. 193), discusses the winds of the stratosphere over North-West Europe. The well-established fact that in the neighbourhood of the British Isles the troposphere is colder in cyclones than in anticyclones, whereas the stratosphere is warmer, implies that the wind velocity should diminish above the tropopause, as has often been pointed out. The results of seventy *ballons-sondes* ascents which penetrated well into the stratosphere have been dealt with in a rather novel manner. In addition to finding average values of wind velocity for different heights above the ground, diagrams were also made in which the height of the tropopause was taken as the zero line, and the velocities were plotted for various heights above and below this line. Moreover, the ascents were separated according to the velocity of the wind, light and strong winds being dealt with separately. An interesting fact immediately became evident, namely that at some height between 8 and 14 kilometres nearly all winds above 15 metres/sec. fall off to under 10 metres/sec. This takes place very close to the

tropopause, if not actually at that point, the initial falling off being very rapid. The effect is much less marked in the case of moderate winds, and does not exist in the case of light winds. The average results were shown in a diagram which is here reproduced (Fig. 1). When the direction of the wind was treated in the same manner, it was found that in nearly every case there was no change on entering the stratosphere, the direction being

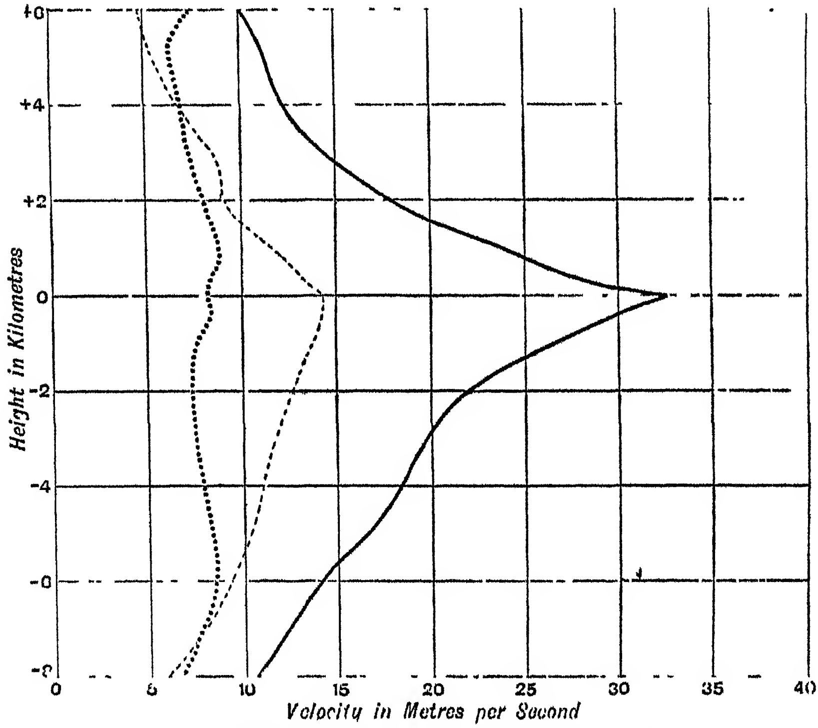

FIG. 1.—Mean variation of wind velocity with height.
(a) Less than 13 metres/sec. in highest 2 kilometres of Troposphere.
(b) Less than 13 to 19 metres/sec. in highest 2 kilometres of Troposphere.
(c) Greater than 19 metres/sec. in highest 2 kilometres of Troposphere.

very steady from a height of 2 kilometres to the highest point reached.

Passing on to the question of horizontal pressure and temperature gradients, the former was found on the average to increase slightly between the ground and the tropopause, and then to diminish rapidly within the stratosphere. The horizontal temperature gradient showed a complete reversal at the tropopause: in the troposphere temperature of course decreases horizontally in the direction of the cyclone, whereas in the stratosphere the reverse is true. The suddenness of the

change found was most remarkable. Above the tropopause the gradient was found to fall off to practically zero at a height of 20 kilometres.

The Structure of the Atmosphere when Rain is Falling (*Q. J. of Royal Met. Soc.*, April 1920, vol. xlvi, No. 194). Prof. V. Bjerknes, in the summer of 1918, initiated the experiment of increasing the number of observing stations in Southern Norway from eight to about ninety, in order to make some attempt at reducing the distance between the points of observation to space-differentials of the streams of air, whose movements and physical changes cause the weather experienced in any place. The experiment has had satisfactory results, and has brought to light some facts that have escaped attention on the less detailed type of synoptic chart in general use. The most important of these is in connection with the bands of rain which frequently sweep across the country in front of travelling depressions, and which are generally replaced by showery weather and a more broken cloud sheet.

A detailed study of the surface wind in these regions makes it clear that there are here two distinct converging currents. Of these, one moves along within the rain-band in the direction of its length, and the other moves almost normally against it. The latter is the warmer of the two, but sometimes the difference of temperature is very slight. We have therefore a cold current lying across the track of a relatively warm current, and the latter, being the most buoyant, steers above the other. The dynamical cooling which takes place in the process results in rain, which falls through the colder current below, and under the pressure of the advancing warm air the colder current yields and the rain-band moves forward. The charts also showed another kind of travelling rain-band, narrower than the other. Here also the lines of flow show two distinct currents, one of which is along the length of the band, and the other roughly perpendicular to it. But in this case the rain is behind the boundary line, and the temperature is higher in front than behind, so that there is a warm current lying across the path of a cold one, and the heavier cold air edges its way under the warm one. The process is naturally more violent in this case, and there are squalls and heavy showers, followed by colder and finer weather. The two lines of separation may be called the "steering" and "squall" lines, and are found to meet at the centre of the depression. The essentials of a depression according to these ideas may be summed up in Prof. Bjerknes' own words, as follows :

"We have before us a struggle between a warm and cold air-current. The warm is victorious to the east of the centre. Here it rises up over the cold air, and approaches in this way a

step towards its goal, the pole. The cold air, which is pressed hard, escapes to the west in order suddenly to make a sharp turn towards the south, and attack the warm air in the flank; it penetrates under it as a cold W. wind. The centre of the cyclone is thereby displaced to the east, along the southern edge of the broad rain-band. This edge shows thus which way the cyclone will take. We have called it the 'steering-line' of the cyclone."

The second part of the paper deals with the local showers that often occur in summer afternoons in otherwise bright sunny weather in Norway, when there is not much wind. At first sight the appearance of these appears to be extremely capricious. That they are the result of local heating is evident, but this was found to be only one condition for their formation. The other is that the inflowing air shall be not merely dry inland air, but fresh vapour-laden air from the sea. The detailed study of a number of cases showed clearly that this was the case, and observations of humidity at a centre of convergence that for several days produced no rain showed the appearance of rain to be correlated with a rapid increase of moisture in the inflowing surface winds. During the period referred to it was found that sea-breezes were the cause of a gradual penetration of damp air into the inland districts.

Thunderstorms.—In Professional Note No. 8, published by the Meteorological Office, Air Ministry, Capt. C. K. M. Douglas summarises many observations of temperature and humidity in the upper air made when flying in France. These cannot well be summarised in a short space, but his classification of conditions favourable for thunderstorm development is worth mentioning. He finds that many summer storms can be placed in one of the following classes:

A. Those due to heated surface air in sunny weather.

B. Those associated with powerful upper currents from south-west, with light variable or south-easterly surface winds.

C. Those with very low upper air temperatures in the south-westerly or north-westerly currents of depressions.

Class A.—Conditions are best in early summer, when the upper air is still cold. The wind is generally light, and the storm seldom travels far before dying out.

Class B.—Instability is due to the relative coolness of the strong south-westerly current high up, and not to heating of the surface layers. They therefore occur as often by night as by day, and they move rapidly with upper current from south-west. The most violent summer line squalls are included in this class.

Class C.—These are often preceded by storms of Class B, and differ from them chiefly in having the westerly wind at

the level of the ground as well as high up. Instability is due to the low temperature of the depression, which is more marked high up than near the surface, but the surface temperature is often below the normal. Such conditions may occur even in winter. The storms are often of only moderate intensity.

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PHYSICS. By F. T. PEIRCE, B.Sc., Barker Graduate Scholar (Sydney).

X-ray Reflection from Crystal Powders.—The now classical researches of M. v. Laue, the Braggs and others on the scattering of X-rays by crystals have, within the last few years, led to a very notable extension in the substitution of a crystal powder for the comparatively large, perfect crystal, hitherto considered necessary. The method is a natural development, and was

worked out independently and almost simultaneously in Germany (Debye and Scherrer, *Physik. Zeitsch.*, xvii, 1916) and in America (A. W. Hull, *Phys. Rev.*, 9, 84, 1917).

The general principles governing the regular scattering or reflection of X-rays by crystals could not be more lucidly explained than in the account given by the original investigators (W. H. and W. L. Bragg, *X-Rays and Crystal Structure*). Many of the results obtained by these earlier methods were necessary, and are assumed in the interpretation of the powder photographs, e.g. the measurement of wave-length and the examination of the characteristic spectra of the metals.

When a parallel, homogeneous beam of X-rays penetrates a crystal, the latter acts as a three-dimensional grating. The regularly distributed scattering points lie in planes regularly spaced throughout the mass, the spacing being greater or less according as more or less points are included in each plane. Each may be considered to act as a mirror reflecting a small fraction of the incident rays. A strong maximum of such reflected energy occurs when the contributions from all the similar planes (the number of such co-operating planes is of the order of several millions) are in phase, which only occurs when

$$n\lambda = 2d \sin \theta. \quad . \quad . \quad . \quad . \quad (1)$$

where n is a small integer, the "order" of the reflections, λ the wave-length, d the spacing of the planes, and θ the angle of incidence.

If a crystal be rotated about an axis in the plane, a fine beam will be reflected as a series of beams, in a plane perpendicular to the axis, corresponding to $n = 1, 2, 3$, etc., the higher orders being progressively fainter and necessarily ceasing when $n\lambda > 2d$. If other axes are taken, the reflections will all lie on cones whose axis is the undeviated beam and semi-vertical angle 2θ given by equation (1). Thus, if a crystal be given every possible orientation, there will be reflected a system of cones corresponding to all possible planes and orders for which equation (1) can hold. This is practically attained in a fine crystalline powder in random distribution, with rotation and stirring if necessary, as with a coarse, lamellated or fibrous powder.

Though an infinite number of planes can be drawn in a crystal, the number capable of reflecting is strictly limited and includes only those in which $d > \frac{\lambda}{2}$. Hence the number of lines, among which the greater part of the scattered energy must be divided, is finite and each has equal reflecting opportunity. In diamond with the tungsten Kα, $\lambda = 0{\cdot}212$Å, this number is 100, while the iron doublet, $\lambda = 1{\cdot}93$, can be reflected by

only three sets of planes. The probability of any reflecting position is rendered finite by the imperfect parallelism of the bundle of rays and the thermal and other irregularities of the atoms which give a finite reflecting range.

A definite fraction of the energy will, therefore, be reflected along each cone depending upon the number of co-operating sets of planes with the same spacing, the position of the nuclei with regard to the reflecting plane and on the position of the scattering electrons within the atom. In elements of high atomic weight, the majority of the electrons will be close to the nucleus so that the position of the nuclei will be the predominant factor. In light atoms, the position of the valency electrons will largely determine the relative intensity of the lines, whose position and number alone can then be relied upon to determine the crystal structure.

The actual intensities of the reflections are very small even under the best of conditions, and necessitate long exposures to intense radiation. Exposures cited by Hull range from 20 to 300 milliampère-hours at constant potential. Every wave-length in the beam is scattered into a different system of cones which in a continuous spectrum would produce a general blackening of the photographic film. Use is made of the characteristic radiation of the anticathode metal, whose properties have been determined by the earlier crystal methods. The system of lines due to these particular wave-lengths stands out from the general effect and can be separately determined.

Hull obtains a still greater homogeneity by making use of the sudden increase in the absorption, by a metal, of rays whose frequency exceeds that of its k_r line. By using a thin filter of a metal whose k_r wave-length is intermediate between the k_a doublet and the k_β of the anticathode, the former, which fortunately is the longest and most intense of the series, is practically isolated. The optimum potential for exciting the tube, the material and the thickness of the filter may be calculated for any particular combination of anticathode, filter and crystal powder from previous work on intensity, wave-length and absorption.

The most generally useful combination seems to be a molybdenum Coolidge tube at 30,000 volts with a filter of 0·35 mm. of powdered zircon. For metals or other heavy scatterers, a tungsten tube run at 100,000 volts and an ytterbium filter of 0·15 mm. is most suitable. Debye and other German investigators obtained good results with unfiltered copper radiation.

The tube should be designed to give as great an output as possible continuously, with as short a distance between target and powder as is consistent with safety and proper definition

of the beam. This latter is obtained by two slits, 5 cm. or more apart. The powder is contained in a thin tube of a light amorphous substance, such as glass or collodion, which should be rotated if the grains are greater than about 0·01 cm. diameter. The trace of the cones is obtained on a film bent into the form of a semicircle or quadrant with the powder at the centre. This only includes the lines whose glancing angle θ is less than 45°. For special purposes and with softer rays such as the copper series the whole circle may be necessary or a semicircle with the primary beam as diameter. A "Duplitised" film with intensifying screens saves time in exposure but is less exact in definition and intensity. The diameter necessary depends on the wave-length used, the width of the beam and of the powder, and must be sufficient to obtain resolution of the lines.

The width and indefiniteness of the lines obtained is a considerable disadvantage to the accuracy of the resulting determinations and an important and promising modification has recently been described by H. Bohlin (*Ann. der Physik*, **5**, 61, 1920). He uses a cylindrical camera through a slit in which a diverging beam irradiates the powder pressed into an arc of the cylinder. All rays reflected at the same angle are subtended by the same arc, and it can easily be shown that the traces of the cones from all parts of the surface of the powder due to radiation from the nearer edge of the slit are enveloped by the image of that edge. All other reflections due to size of slit and penetration of surface are included within this limit, which appears as a sharp boundary. It seems rather a disadvantage that the proportional error is greatest in the case of the most important lines of small glancing angle and that the intensities are still less amenable to interpretation than in the original method.

On development of the film, the distances of the lines from the central trace are measured and θ obtained for each reflection. The order of the reflections can be seen, and a list made of the spacings of the reflecting planes. The process is now one of trial and error. In the case of metals and substances whose crystal symmetry is simple and known, this is fairly easy. The spacings of the planes, and the number of co-operating planes for each spacing capable of giving a reflection, are calculated and tabulated. This can be done once and for all for any lattice system, altering only the scale for different materials. The list already obtained may then be compared with those for lattices of the same symmetry.

The possibilities and complications become overwhelming with unsymmetrical compounds containing several types of atoms, and none such have yet been analysed by this or any other method.

Working on these lines, a great number of materials have been examined which could not be obtained in a form suitable for examination by the spectrometer. This work has mainly been done by Hull and his co-workers at Schenectady, where a broad-minded and generous commercial research policy has made possible a development which slender academical resources could hardly have undertaken. The metals and other elements have been and are being systematically examined, and many important facts and relations are emerging from the more extensive knowledge of atomic arrangement.

In the realm of atomic theory, the results throw considerable light on the size and shape of the atom. The data can only be profitably considered on the basis of a space distribution of electrons moving about equilibrium positions along such lines as the hypothesis developed by Langmuir (*Journal of the American Chemical Society*, p. 868, 1919.) In this way, Prof. W. L. Bragg (*Phil. Mag.* August, 1920) has very successfully interpreted the distances between atomic nuclei and their constancy for each atom. Hull (*Phys. Rev.*, ix. 84, 1917) has even tried to interpret the intensities of the lines of some lighter substances, viz. diamond, carborundum, and iron, by fixing the position of the electrons within the atom. Little faith, however, can be put in the photographic intensities obtained, despite all the precautions taken, nor is our knowledge of the mechanism of scattering sufficient to justify calculations even from such definite assumptions as the "cubical atom" suggests.

A very powerful means of analysis may be developed from a combination of spectrometer and powder methods, as each supplements the other. The spectrometer can measure more accurately the relative intensities of the lines from any one of the most important planes while the powder photograph tests the existence and compares the intensities on one film of reflections from a great number of planes. Laue photographs give some information about planes of high indices, and, with the knowledge of "atomic diameters" already alluded to, analysis may be extended to crystal systems which are yet untouched.

A great field of application is possible, of interest both theoretical and practical, in the chemical and structural analysis of substances as they actually occur in nature or industry. Examination has shown many so-called amorphous substances to be really crystalline, notably "amorphous" silicon (Debye and Scherrer, *l.c.*).

A. W. Hull has described the use of the method in chemical analysis. Every crystalline chemical compound gives a definite system of lines which can be identified, though the crystal

structure be entirely unknown. The system is peculiar to itself, and, if found in the photograph of any material, the substance must be present. There is no ambiguity, as the whole system of each substance present in quantity must be reproduced intact, and the probability of even one or two lines being the same for different materials is very small. In cases of doubt or of complex mixtures, shorter or longer wave-lengths may be used giving correspondingly complex or simple spectra. The determination is even roughly quantitative, but its main value lies in showing the actual state of combination and in the small quantity of the substance necessary.

Several other practical applications suggest themselves along these lines, and the method is likely to become a permanent institution when refinements of apparatus allow greater economy in the taking of exposures.

The appended list includes the principal papers which have been published dealing with this method and the results obtained by it:

Debye and Scherrer, *Physik. Zeitsch.*, xvii. p. 277 (1916); xviii. p. 291 (1917); xix. p. 474 (1918).

Johnson and Toeplitz, *Physik. Zeitsch.*, xix. p. 47 (1918).

H. Bohlin, *Ann. der Physik.*, 5, 61 (1920).

A. W. Hull, *Phys. Rev.*, x. 661 (1917).

Journ. Amer. Chem. Soc., August 1919.

Collected with other papers in "X-ray Studies," *Gen. Elect. Coy.*, N.Y., 1919.

The Magnetic Properties of Crystals.—Oxley has extended his researches on magnetism and atomic structure (cf. "The Relation of Magnetism to the Crystalline State," Science Progress, April 1920, a summary of his previous work) by the investigation of the deportment of naphthalene crystals and other substances in the magnetic field. From these results, and those of Tyndall, it is seen that we must admit a special distribution of electron orbits in the atom in order to account for magnetic deportment and crystalline structure. It is found that the aggregate projected area of such orbits, on a plane perpendicular to the principal cleavage, is a maximum both for diamagnetic and paramagnetic crystals. This conclusion is not inconsistent with the Lewis-Langmuir theory on which pairs of electrons are held in common. Oxley attributes the rigidity of crystalline elements and non-conducting compounds to highly localised magnetic doublets, such as would exist if the electron itself were magnetic. These determine the orientation of the molecules of the space lattice. The Laplace intrinsic pressure is due to the more open electrostatic doublets, the mechanical stress arising from these having a definite mean value whatever the molecular orientation.

PHYSICAL CHEMISTRY. By W. E. GARNER, M.Sc., University College, London.

The Third Law of Thermodynamics and the Entropy of Solutions and Liquids.- An interesting contribution to the study of the third law of thermodynamics has been published by Lewis and Gibson (*J.A.C.S.*, 1920, **42**, 1529). The third law of thermodynamics as stated by Nernst requires that the change in entropy accompanying any process which involves only solid and liquid substances approaches zero as the absolute zero of temperature is approached; he, however, considered that solutions required closer study. Planck has shown that this statement of the third law must be modified when processes of solution are concerned. Assuming that the entropy of every elementary substance is zero at the absolute zero, he states the third law as follows: the entropy of any pure substance at absolute zero is zero. Lewis and Gibson (*J.A.C.S.*, 1917, **39**, 2554) have collected together a good deal of evidence to show that the third law is valid for pure substances. In the examples given, good agreement was obtained between the values of the entropy calculated from the equation $\Delta F - \Delta H = T\Delta S$, and those determined from the specific heats at constant pressure.

In the present paper considerations are brought forward which lead to the conclusion that some, and perhaps all, solutions are to be excluded in the statement of the third law. The two liquids benzene and toluene, when mixed together, form an almost perfect solution; that is, one in which there is no evolution of heat on mixing, and in which the vapour pressures and fugacities obey Raoult's law. The increase in the entropy, on producing one mol of the mixture containing equal molar fractions of the two constituents, is given by the equation

$$\Delta S = Rln2 = 1{\cdot}4 \text{ cal. per degree.}$$

Since the heat capacity of the mixture is about equal to the sum of the heat capacities of the pure constituents, ΔS must be nearly independent of the temperature, for we have the thermodynamic relation

$$\frac{d\Delta S}{dT} = \frac{\Delta C_p}{T}$$

where ΔC_p is the difference between the heat capacity of the solution and the heat capacity of the pure substances from which it is produced. If ΔC_p remains zero down to the absolute zero, ΔS will remain constant and equal to $Rln2$; but if, as seems likely, ΔC_p varies, owing to the solutions ceasing to be perfect at the lower temperatures, then ΔS may be reduced to

a small value or to zero at the absolute zero. This is unlikely for all types of solutions, and would probably not be the case for a mixture of two nearly identical isomerides. In the extreme case of mixtures of two isotopes, we have substances which are nearly identical in chemical properties. It seems improbable that two such isotopes would undergo any appreciable change in heat capacity on mixing. In this case, therefore, we may conclude that the entropy change on mixing is practically the same at ordinary temperatures as at absolute zero.

Doubt is raised even with regard to pure liquids composed of a single constituent, for the distinction between a solution and a liquid is to a large extent artificial. Water is regarded as a pure substance, yet it is probably composed of several molecular species which are in equilibrium at ordinary temperatures. This might not be the case if water is supercooled to a very low temperature. The same doubt applies to the two forms of sulphur S_λ and S_μ. There seems to be no reason to believe that these liquids fall within the scope of the third law. On the other hand, there are both theoretical and experimental reasons for the validity of the third law in the case of crystalline solids.

The third law is restated as follows : " If the entropy of each element in some crystalline form be taken as zero at the absolute zero, the entropy of any pure crystal is zero, and the entropy of any other substance is greater than zero."

Gibson, Latimer, and Parks have determined the entropies of formic acid and urea from measurements of C_p over a range of temperatures down to 71° K. From these data they have calculated the free energies of these two substances, and compared the values with those obtained from equilibrium measurements.

The values which have been obtained agree with those of Branch (*J.A.C.S.*, 1915, **37**, 2316) for formic acid, and those of Lewis and Burrows (*J.A.C.S.*, 1912, **34**, 993, 1575) for urea.

The specific heats of ethyl and propyl alcohols and their equimolar mixture have been studied in the liquid state down to 78° K., and it is shown that down to this temperature the heat capacity of the mixture is practically the same as the mean of the heat capacities of two pure substances. This fact indicates that the entropy difference between the equimolar mixture and the two pure substances persists without appreciable diminution down to these temperatures, and probably to the absolute zero. The specific heats of crystalline and liquid ethyl alcohol also seem to indicate that if the entropy of the crystalline form be zero at 0° K., then the entropy of the supercooled liquid is not zero.

Lubrication and Chemical Constitution.—The relationship between chemical constitution and lubrication has been in-

vestigated by W. B. Hardy (*Phil. Mag.*, 1920, [vi], **40**, 201). The static friction between a burnished bismuth surface and a bismuth slider, lubricated by a large number of pure substances, has been determined. It is shown that, although the static friction is a function of the molecular weight of the lubricant, an important part is played by chemical constitution. In some simple chemical series, the static friction diminishes regularly as the molecular weight increases, and in the case of chain compounds such as fatty acids, alcohols, and paraffins, it appears that a good lubricant will be found if one goes high enough in the series. The presence of the *d*OH and COOH groups, however, disturbs this simple linear relation to the molecular weight. The first members of a homologous series are frequently abnormal, and in the case of the fatty acids an alternation in the value of the static friction occurs which is similar to that observed in the melting-points of this series. The presence of unsaturated groups increases the lubricating power of chain compounds, whether it be the double bonded oxygen of ketones or acids or the carbon of olefines and alcohols.

In their qualities as lubricants of bismuth, ring compounds are the converse of chain compounds; thus the presence of double bonds and OH and COOH groups decreases the lubricating power. It is observed that no ring compound which was investigated is a good lubricant; a much smaller change in the static friction is obtained with increase in molecular weight, and even cholesterol, with a molecular weight of 366, is a poor lubricant. It is also shown that the more efficient the lubricating power of a substance, the more strongly is it absorbed by a bismuth surface.

Colloidal Electrolytes. McBain has recently summarised his work on the constitution of soap solutions (McBain and Salmon, *J.A.C.S.*, 1920, **42**, 426). From this investigation it appears the solute of a soap solution contains at least five different constituents; the crystalloid soap and its ions, the colloidal micelle with a large negative charge, and the typical colloidal particle, which is neutral or only slightly charged. Of these the greatest amount of interest is attached to the colloidal micelle. In colloidal electrolytes the micelle plays the same part as that of the simple ion in crystalloid electrolytes; it is a heavily hydrated polyvalent particle that carries a large number of electrical charges and conducts electricity just as well or even better than the simple ion it replaces. This high mobility of the colloidal ion explains the high conductivity of concentrated soap solutions. McBain regards the micelle as an agglomeration of the fatty ions of the soap, which, owing to the enhanced electrostatic potential, is heavily weighted with

water and perhaps associated with the true colloid. He suggests the following formula for the colloidal ion:

$$(NaP)_x P_n^{n(-)} (H_2O)_m$$

and points out that this would be the same as

$$(NaP)_x (HP)_n (OH)_n^{n(-)} \cdot (H_2O)_{m-n}.$$

The mobility of this colloidal ion is dependent upon the quantity of adsorbed water, and since the vapour pressure of the solution diminishes with increase in concentration, the particle becomes dehydrated and the mobility is increased. These heavily hydrated micelles are extremely sensitive to lowering of the vapour pressure; the addition of small quantities of electrolytes to sodium palmitate solutions decreases the hydration of the micelle, and thus diminishes the viscosity. The enormous viscosities of soap solutions are due largely to the hydration of the micelle. The addition of larger quantities of electrolytes, however, produces more undissociated colloid and finally salts out the soap.

In dilute solutions the ions are simple, but with increase in concentration the simple fatty ion is replaced by the colloidal micelle, and at moderate concentrations the only crystalloid constituent is the potassium or sodium ion. In concentrated solution nearly half of the current is carried by the colloidal micelle, so that its mobility must be comparable with that of the cation. On diluting the solution the colloid breaks up into simple ions and the simple undissociated soap, and this points to the existence of an equilibrium between the colloid and its ions. The composition of the micelle appears to vary with the concentration, and the conductivity results are best explained on the assumption that there is a gradual transition of ionic micelles to ordinary ions on the one hand, and through slightly charged to typically neutral colloids on the other.

The sodium and potassium salts of the fatty acids have been examined from the acetates to the behenates, and it is found that there is a gradual change in properties as the series is ascended. The solution is the more collodial the greater the number of carbon atoms in the molecule; the greatest change in this respect takes place between the caprate (C_{10}) and the laurate (C_{12}). Generally the potassium soaps are more colloidal than the sodium soaps. In N solutions the total colloid present is at least 15 per cent. in the case of the hexoates, increasing to nearly 100 per cent. in the higher soaps, but the proportion of the colloid rapidly falls off with the dilution.

The theory may be extended to many other colloidal solutions, such as acid and alkali proteins, indicators, sodium

silicate, sodium tungstate, etc. Pauli has examined electrometrically the combination of albumen with each ion of hydrochloric acid, and shows that the maximum difference in the amounts of hydrion and chlorine ion taken up by 1 per cent. albumen occurs at about 0·02 N HCl. This maximum coincides with maximum viscosity and maximum osmotic pressure in an osmometer. Further addition of hydrochloric acid drives back the ionisation with great rapidity. The authors give an explanation from the laws of mass action, and point out that this law will probably apply in only qualitative form to such a colloidal system.

If the equation

$$H_nA^{n+} + nCl^- \rightleftharpoons A \cdot H_n \cdot Cl_n$$

holds, then the dissociation of the colloidal complex should depend on the nth power of the chlorine ion concentration, and this would explain the great effect observed by Pauli.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., University College, London.

THE production of fatty acids by the oxidation of paraffin hydrocarbons has recently occupied the attention of more than one chemist. According to Fischer and Schneider (*Berichte*, 1920, **53**, [B], 922), pure or crude paraffin may be oxidised by heating with dilute sodium carbonate solution in a steel autoclave at 170°, compressed air being forced through the mixture. Under these conditions a yield of fatty acids amounting to 90 per cent. of the paraffin used may be obtained. The acids formed mostly contain an odd number of carbon atoms; those definitely identified correspond to the following formulæ: $C_{13}H_{26}O_2$, $C_{15}H_{30}O_2$, $C_{17}H_{34}O_2$, and $C_{19}H_{38}O_2$. If air is blown through the paraffin at 135°-145° in the absence of water, a mixture of acid anhyrides is obtained, which may be separated from the unchanged paraffin by extraction with acetone, in which the paraffin is insoluble. According to Franck (*Chemiker Zeit.*, 1920, **44**, 309), paraffin oils may be oxidised to fatty acids by heating them with oxygen in the presence of a catalyst consisting of a compound of lead, mercury, manganese, vanadium, or chromium, or even alkalis or alkaline earths. With a temperature of 150° and from 0·5 to 5 per cent. of catalyst, about 50 per cent. of fatty acids of low molecular weight and 40 per cent. of acids of high molecular weight are obtained, together with a small quantity of a mixture of ketones. An edible fat resembling coconut oil was produced by esterifying the mixed fatty acids with glycol.

Two patents have recently been taken for the elimination

of carboxyl groups. Thus, according to the first patent, pyrogallol may be obtained in almost theoretical yield by heating solutions or suspensions of tannic or gallic acids in an autoclave at 160° with alkali or alkaline earth hydroxides or carbonates. According to the other patent, a 95 per cent. of the theoretical yield of pyrogallol may be obtained by heating a mixture of gallic acid with half its weight of sodium chloride at 250° and 15 mm. pressure.

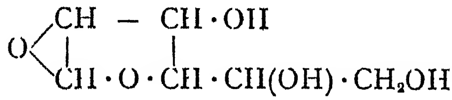
In a study of the Sandmeyer reaction, Korczynscki, Mrozinski, and Vielau (*Comptes rend.*, 1920, **171**, 182) find that the double cyanide of nickel and potassium may replace that of copper and potassium for the preparation of nitriles from diazonium salts, but that the corresponding cobalt salt is unsatisfactory. On the other hand, cobalt thiocyanate is a very good catalyst for converting a diazonium compound into the corresponding thiocyanate. Cobalt and nickel haloids only give poor yields in the preparation of chloro- or bromo-benzene, while zinc or iron salts have no catalytic action at all in these reactions.

Bamberger and Nussbaum (*Monatshefte*, 1919, **40**, 414) find that strong or almost anhydrous hydrogen peroxide forms an excellent solvent for many organic compounds, more especially those containing a number of hydroxyl groups. Thus 60 per cent. hydrogen peroxide dissolves starch, giving viscous or dough-like solutions. Cellulose and its decomposition products also are soluble, but the peroxide requires to be stronger the less decomposed the cellulose is. Water precipitates the cellulose from such solutions in chemically modified form, but no products are formed which are coloured blue by iodine. Cellulose swells in solutions of hydrogen peroxide too weak to dissolve it, the lumen of fibres becoming diminished. Anhydrous hydrogen peroxide is not explosive, and cannot be exploded by a mercury fulminate cap, but in conjunction with some organic compounds it yields explosives which are capable of technical application.

When wool is dipped in concentrated hydrogen peroxide and then well washed with water, the fibres become elastic, but on being dried lose their elasticity. The action of hydrogen peroxide on substances appears to be of the nature of depolymerisation, paraformaldehyde being converted into formaldehyde.

Carbohydrates.—In an earlier article (SCIENCE PROGRESS, 1920, **56**, 565) reference was made to a paper by Pictet and Goudet referring to lævoglucosan. Since then a paper has been published by Pictet and Castan (*Compt. rend.*, 1920, **171**, 243) dealing with glucosan, which compound, originally described in Gélis in 1860, can be prepared, according to these authors, by heating glucose at 150°–155°, under a pressure of

15 mm. From its behaviour and reactions the following formula is assigned to this compound:



The constitution of cellulose is discussed in a lengthy paper by Hess and Wittelsbach (*Zeitsch. Elektrochem.*, 1920, **26**, 232). They reject the view put forward by Pictet and Sarasin (*Compt. rend.*, 1918, **166**, 38), that there is any connection between the cellulose molecule and the lævoglucosan complex, on the ground that ethyl cellulose on distillation under reduced pressure yields no ethyl lævoglucosan.

From evidence derived from the relative quantities of cellobiose acetate and dextrose pentacetate produced from the acid hydrolysis of cellulose, this substance is regarded as being a condensation product of a number of molecules of hydrocellulose, for which latter term the name celluxose is now proposed. The separate molecules of celluxose are assumed to be united through residual affinities of hydroxyl groups. Ethyl cellulose is considered to be a derivative of celluxose, since it is hydrolysed much more easily than cellulose itself. It is further suggested that the physical characteristic of cellulose as a hollow thread is reproduced in the arrangement of the celluxose molecules in the cellulose complex, which would account for the partial breaking down of this complex in the case of cellulose which has been subjected to prolonged beating or grinding. The hydrocellulose or celluxose molecule is regarded as being a dextrose or cellobiose in which all the hydroxyl groups are etherified by dextrose or cellobiose residues.

Iron zymophosphate, $C_6H_{10}O_4(PO_4Fe)_2$, is the iron salt of hexose diphosphoric acid; it is prepared by adding ferrous chloride to a solution of sucrose which has been treated with expressed yeast, toluene, and sodium phosphate. The salt is decomposed in the stomach, yielding both iron and phosphoric acid in easily assimilable form.

In contradiction of the generally accepted view, Neuberg (*Biochem. Zeitsch.*, 1920, **103**, 320) states that hexose diphosphate is not formed under normal conditions of alcoholic fermentation.

Considerable attention has been devoted recently to the chemistry of bread; amongst recent publications may be mentioned one by Whymper entitled *The Conditions that Govern Staleness in Bread*, published by Maclaren & Sons, 1919, and a series of papers by Ostwald and Lüers (*Kolloid Zeitsch.*, 1919, **25**, 26, 82, 116, 177, 230, and 1920, **26**, 66). Attention may also be drawn to a paper on the "Properties affecting

Strength in Wheaten Flour" by Martin (*J. Soc. Chem. Ind.*, 1920, **39**, 247 T).

Vitamines.—A new method of investigating vitamines quantitatively has been devised by Williams (*J. Biol. Chem.*, 1920, **42**, 259). A certain quantity of the solution to be tested is mixed with 100 c.c. of a standard culture solution containing asparagine and ammonium sulphate, and after making up to 110 c.c. the whole is sterilised. One c.c. of a freshly prepared suspension of yeast containing 0·3 gm. of yeast per litre is added, and the mixture is incubated for eighteen hours. Further growth is stopped by the addition of formaldehyde, and after removal of any wild yeast on the surface, the yeast proper is filtered off on a Gooch crucible, washed with water and alcohol, dried for two hours at 103° and weighed. The weight produced minus the amount produced in a control is proportional to the amount of vitamine present. The number of milligrams of yeast produced by the addition of 1 gm. of material is known as the "Vitamine Number" of that substance. A steady stream of papers dealing with vitamines and their occurrence in plants, etc., continues to pour forth, especially from the *American Journal of Biological Chemistry*, as well as our own *Biochemical Journal*, and to these two sources readers are referred for particulars; many of the results obtained by the various workers are contradictory, but the elusive nature of these substances is sufficient to account for this unsatisfactory state of affairs.

The question of whether vitamines are necessary for the development of plants is answered in the negative by Lumière (*Compt. rend.*, 1920, **171**, 271), from the observation that the addition of brewer's yeast heated to 135°, which no longer cured polyneuritis in pigeons, to a poor culture solution considerably improves the growth of fungi on such a solution.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Taxonomy.—Spencer le More, in the *Journal of Botany*, describes several new genera under the names of *Homaliopsis* (Flacourtiaceæ), *Vaughania* (Leguminosæ), *Umbellulanthus* (Erythroxylaceæ), and *Monocephalium* (Icacinaceæ); also new species of *Noronhea, Lasiosiphon, Hemigraphis, Pseuderanthemum, Justicia, Halemacanthus, Nectaropetalum, Stachyanthus, Pyrenacantha, Strombosia,* and *Strombosiopsis.* Mr. Ridley in the same Journal describes a new species of *Entada*, and T. and T. A. Stephenson give a useful account of the British species of *Epipactis.*

The Naturalist (September–November) contains a key to the genera of British Agaricaceæ, compiled by Dr. Wager. A

revision of the Australian Salicorniceæ is contributed by J. M. Black to the *Trans. Roy. Soc. S. Australia*.

In the *New Phytologist* for July and October, J. R. Matthews discusses the so-called species of *Rosa* as found in Britain, and shows that the different types observed are mostly explicable on the assumption of the occurrence of numerous segregates of hybrids between a comparatively small number of species.

Several important papers have appeared during the present year in the *Bot. Jahrbücher*. These include contributions to the Flora of Africa dealing with the Families Piperaceæ (de Candolle), Acanthaceæ (Lindau), and Rubiaceæ (Krause), and to the Flora of Papua dealing with the Anacardiaceæ, Burseraceæ, Simarubaceæ (Lauterbach), Araliaceæ (Harms), Sapindaceæ (Radlkofer), and Pteridophyta (Branse). Central American species of *Piper* are the subject of a paper by de Candolle in the *Bot. Gaz.*

Anatomy and Cytology.—Forsaith has studied the ray structure in alpine and closely related lowland species, and concludes that the colder conditions tend towards reduction in the ray storage tissue.

The anomalous secondary thickening of *Chenopodium album* has been studied by E. F. Artschwager (*Amer. Jour. Bot.*), whose conclusions differ in several particulars from those of earlier investigators. The cambium, which is periodically active and forms xylem throughout its extent on the inner side, is "used up" in those parts where, on its outer face, phloem groups are developed. Cambial continuity is, however, maintained by the formation of arcs of meristematic tissue on the outer face of the phloem groups. These latter are chiefly composed of sieve tubes, whilst phloem parenchyma, which has been regarded as their chief constituent, is relatively unimportant.

The anatomical structure of abnormal seedlings of *Impatiens Roylei* is described by Miss Holden (*Ann. Bot.*), who finds suppression of one root pole associated with syncotyly and also disappearance of the first leaf of the epicotyl.

The anatomy of the stem of *Dioon spinulosum* is described by Langdon (*Bot. Gaz.*, August), who finds 7–9 strands passing into each leaf, of which 5–7 anastomose to form the two characteristic girdle-strands in the leaf-base. Three types of rays are present in the stem. Uphof (*New. Phyt.*, June) describes the anatomy of Xerophytic Selaginellas, and finds that these agree in the proximity of the dorsal and ventral leaves and their similarity in size. The vessels are narrower than in hygrophytic species or plants grown in humid soil. The cells contain a large proportion of oil in contrast to the starch of hygrophytic species. This last observation suggests comparison with xerophytic algæ such as *Hormidium* and *Zygnema*.

It appears that not infrequently there is a fusion between the egg cell and the ventral canal cell in *Sphagnum subsecundum*, though disintegration of the latter is also equally common (Bryan, *Amer. Jour. Bot.*).

Carter (*Ann. Bot.*), continuing her studies on the chloroplasts of Desmids, finds that in *Staurastrum* there is usually a central axis with radiating plates, but in *S. tumidum* parietal chloroplasts are always encountered, a condition sometimes found in *S. grande*.

Ecology.—An excellent and critical résumé of the present position with regard to the Classification of Vegetation units is contributed by Tansley to the *Journal of Ecology*. The view is expressed that the concept of development is essential for their grouping, and that for the higher units life-form is an unsatisfactory criterion.

Bonnier (*Rev. Gen. Bot.*, T. 32) has written a further paper on his well-known researches on the growth of alpine plants in lowland districts which he has conducted during the past thirty-six years. The present paper deals mainly with plants raised from seed, and the results in the main confirm the earlier work on transplanted roots. Seed from a single parent was in each case sown in an upland and a lowland locality. Those grown in the former were much shorter, less robust, and attained maturity at an earlier phase of development. Several annual species at high altitudes become perennial (*e.g. Poa annua*) and even evergreen (*e.g. Arenaria serpyllifolia, Echium vulgare*). For the alpine species there appears to be an optimum altitude at which the vigour of the plant attains a maximum.

A large number of interesting papers have appeared recently dealing with the subject of soil acidity and the action of calcium. Several of these which have appeared in *Soil Science* are of considerable ecological importance. Neller finds that for Sassafras loam the oxidising power of the soil varies inversely with its lime requirement, and that the crop yield is closely correlated with oxidising power. Parker and Truog find a close relation to obtain between the calcium and nitrogen content of plants, and find that a high lime requirement appears to be associated with a calcium-nitrogen ratio. Shedd, who has investigated a number of virgin and cultivated soils in Kentucky, finds that the poorest contain the lowest content of calcium, and the best the highest. The work of Greaves appears to confirm that of Brooks as indicating an antagonistic action between calcium and magnesium.

The effect of climate on the development of annual rings in *Sequoia gigantea* is the subject of a paper by Douglas, who regards them as an indicator of the resultant of the sum total of climatic conditions (*Ecology*). It is suggested that, by the

study of annual rings, it may be possible to map out districts according to the favourability of their climate for tree growth. For this purpose the author proposes to employ the difference between each two successive rings divided by their mean (mean sensitivity). Trunks of *Sequoia gigantea* are now known the annual rings of which take us back to 1305 B.C., so that a period of over 3,000 years is thus available for study.

Economic.—In a valuable paper by H. B. Sifton (*Am. Jour. Bot.*), the results are reported of an investigation of the germinative capacity of some hundreds of samples of common crop seeds which were tested annually over a period of twenty years. Spring Wheat apparently retains its germinative capacity almost unimpaired for the first five years, with but slight decrease up to the tenth year. Between eleven and fifteen years more than 75 per cent. of the seeds die, but a few were still viable at seventeen years.

Out of a total of 179 samples of Oats, the majority showed a slightly increased germinative capacity after five to six years' storage, and lost their viability much more slowly, nineteen-year-old seeds still exhibiting a germinative capacity of 41 per cent. Timothy-grass seed deteriorates rapidly, especially after the seventh year, whilst both Alsike and Red Clover show a regular decline from the first year, which offers a marked contrast to the almost abrupt depreciation exhibited in the other seeds tested.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, M.A., University College, Reading (Plant Physiology Committee).

Permeability.—Since this subject was last dealt with in these pages (SCIENCE PROGRESS, **12**, 575–80, 1918) it has continued to form the subject of much investigation. Foremost among recent researches on the permeability of plant tissues must be reckoned those of K. Höfler, the results of whose investigations are recorded in a series of papers the first of which appeared towards the end of 1917. In this first contribution ("Die plasmolitisch-volumetrische Methode und ihre Anwendbarkeit zur Messung des osmotischen Wertes lebender Pflanzenzellen," *Ber. deut. bot. Ges.* **35**, 706–26, 1917) a new method for determining the osmotic concentration of plant cells, is described. The basis of the method is as follows. A cell is plasmolysed in a decidedly hypertonic solution, as a result of which its volume when plasmolysis is complete is reduced by a certain amount, say by $1/n$ of the original internal volume of the cell. If the latter is completely semi-permeable to the plasmolysing substance, the concentration of the cell sap must have increased to $n/n-1$ of its original value, assuming that in the original condition the cell was not stretched on account of turgor

pressure. Then if V_p is the volume of the plasmolysed protoplast and vacuole, V_z the original internal volume of the cell, C the osmotic concentration of the plasmolysing solution, the original osmotic concentration of the cell is given by

$$O = C \cdot \frac{V_p}{V_z}.$$

If the cell was originally in the turgid condition, its osmotic concentration O_t is given by

$$O_t = \frac{O}{G},$$

where G is a value called the degree of turgor tension, and is the ratio of the volume of the turgescent cell to that of the non-turgescent, but unplasmolysed, cell.

In these formulæ it is assumed that the protoplast contracts at the same rate as the vacuole. It is, however, probably more correct to assume that the protoplast does not contract at all during plasmolysis. Accepting this as the true state of affairs, we have in place of the above formula the following:

$$O = C \cdot \frac{\frac{V_p}{V_z} - p}{1 - p}$$

$$\text{and } O_t = O \cdot \frac{1 - p}{G - p},$$

where p is the proportion of the whole volume of the unplasmolysed cell which is occupied by the protoplast.

Should the protoplast change its volume by a fraction ap the first equation becomes

$$O = C \cdot \frac{\frac{V_p}{V_z} - ap}{1 - ap}$$

The quantity $\frac{V_p}{V_z}$ is called by Höfler the degree of plasmolysis. In cells of suitable shape which undergo a regular contraction on plasmolysis, it is easy to measure both V_p and V_z. In the case of parenchymatous cells from the stem of *Tradescantia elongata*, G. F. W. Meyer, it was shown by Höfler that the degree of plasmolysis was inversely proportional to the concentration of the plasmolysing solution in the case of sucrose solutions, varying in concentration from 0·30 to 0·60 gram-molecules per litre. With a number of solutions of different concentrations consistent values for the osmotic concentration

of the cells were thus obtained, thus confirming the correctness of the method.

The method was first called by Höfler the *plasmolytic-volumetric* method. In the second paper ("Permeabilitätsbestimmung nach der plasmometrischen Methode," *Ber. deut. bot. Ges.*, **36**, 414-22, 1918) this rather cumbersome name is contracted to the term *plasmometric method.* In this paper it is shown how the method can be adapted for the determination of the permeability of plant cells. The principle of the method is as follows. Suppose a cell is completely plasmolysed in a solution and the degree of plasmolysis is P_1. If the plasmolysing substance penetrates into the cell the latter will gradually become deplasmolysed. Suppose that after a time the degree of plasmolysis is P_2. Then, if the external concentration of the solution is C, and the osmotic concentrations of the cells corresponding to the two degrees of plasmolysis are O_1 and O_2, we have $O_1 = CP_1$ and $O_2 = CP_2$, whence $O_2 - O_1 = (P_2 - P_1)\,C$. The change in osmotic concentration of the cell per unit time is then taken as a measure of the permeability of the cell.

In his third paper ("Über die Permeabilität der Stengelzellen von Tradescantia elongata für Kalisaltpeter," *Ber. deut. bot. Ges.*, **36**, 423-42, 1918) Höfler records some measurements of permeability made by his method. The tissue employed, parenchymatous cells from the stem of *Tradescantia elongata*, was somewhat permeable to potassium nitrate. One of the striking results obtained in this work is the extreme variability in permeability of even neighbouring cells of the same tissue, measurements of apparently similar cells under the same conditions varying from, for example, 0·016 to 0·002. Höfler emphasises the advantage of his method over those previously employed, in that the actual permeability of single cells is measured, whereas in earlier methods this is not the case.

In a fourth paper ("Über den zeitlichen Verlauf der Plasmadurchlässigkeit in Salzlosungen. I.," *Ber. deut. bot. Ges.*, **37**, 314-26, 1919) the course of intake of potassium nitrate by stem cells of *Tradescantia elongata* was investigated. The permeability of the same cell was found to undergo reversible changes in its permeability, the cause of which is not at all obvious, since these changes in permeability may be quite different in different cells exposed to the same conditions so that the changes do not appear to be due to the action of the external solution. The fact observed by Fitting, in the case of epidermal cells of *Rhœo discolor*, that there is a decrease in permeability after long action of the salt solution, was observed in some, but not all, cells examined by Höfler, but when this decrease in permeability occurs at all, it comes about much later in *Tradescantia elongata* than in the cells examined by Fitting. In the

hours before death of the cell takes place, there is only a quite gradual rise in permeability.

An examination of the permeability to single salts of the cells of roots of *Lupinus albus*, and the palisade cells of the leaves of *Acer platanoides* and *Salix babylonica* was made by Troendle ("Sur la permeabilité du protoplasme vivant pour quelques sels," *Arch. Sci. Phys. et Nat.*, Sér. **4**, 45, 38–54, 117–32, 1918). The ordinary plasmolytic method was used. It was found that potassium chloride and sodium chloride were absorbed for the first ten minutes at a uniform rate, but that after that time the permeability decreased, the relation between time and penetration of salt being approximately a logarithmic one. From this result Troendle concluded that a phenomenon of irritability is concerned, as the results agree with Weber's rule. The author suggests that the salts irritate the protoplasm, which react to this irritation by the transportation of salt to the interior of the cell. It must be admitted that these conclusions are based on insufficient evidence. Troendle also compared the rate of entrance of a number of different salts. From his results he came to the conclusion that the rate at which any salt entered the cell depends both on the kation and the anion, the rate of intake of salt depending partly on the position of the kation in the periodic classification.

In a later paper ("Der Einfluss des Lichtes auf die Permeabilität der Plasmahaut und die Methode der Permeabilitäts-Koeffizienten," *Vierteljahrsch. d. Naturforsch., Ges. in Zurich*, **63**, 187–213, 1918) the same author corrects his earlier results in which he had assumed that the osmotic concentrasion of sucrose was exactly proportionate to the molecular concentration, but he finds that his results are not appreciably altered and consequently that his former conclusions are still valid.

Miss M. Williams ("The Influence of Immersion in Certain Electrolytic Solutions upon Permeability of Plant Cells," *Ann. of Bot.*, **32**, 591–9, 1918) investigated permeability of cells of the petioles of the leaf of London Pride. Sections of the organ in question, when immersed in a 2 per cent. solution of ferric chloride, do not show any reaction between the ferric chloride and the tannin contained in some of the cells until the lapse of two or three days at least. But after the sections are treated with solutions of certain salts, the reaction is given much more rapidly, owing to an increase in the permeability of the cell. For purposes of comparison an arbitrary degree of increased permeability was chosen, namely, that at which an immersion for three minutes in the ferric chloride solution brought about a reaction with the tannin of the cell, so that a blue coloration was produced. The time taken to bring about this increase in

permeability depends upon the nature of the salt, and its concentration. The relation between concentration and the time required to produce the arbitrary increase in permeability is given by the equation

$$\log T = R - A \cdot \log C + 1,$$

where T is the time, C the concentration and R and A are constants, the latter depending on the electrolyte employed. The method is only applicable to fairly high concentrations of the salts used, 0·2 molecular potassium nitrate and potassium chloride, 0·08 molecular barium chloride, 0·05 molecular barium nitrate and 0·04 molecular aluminium chloride being the lowest concentrations in which results could be obtained. These concentrations are, nevertheless, considerably less than those employed by Höfler in his work noted above.

In an interesting paper by K. Heusser ("Neue vergleichende Permeabilitätsmessungen zur Kenntnis des osmotischen Verhältnisse der Pflanzenzelle in kranken Zustande," *Vierteljahrsch. des Naturf., Ges. in Zurich*, **65**, 565–89, 1917) the permeability of the cells of leaves of *Prunus persica* attacked by the fungus *Exoascus deformans* was compared with that of leaf-cells of the same plant in the healthy condition. The measurements were made by the plasmolytic method. It was found that the attack of the fungus brings about a change in the cells of the host so that the permeability rises to a maximum during the period of most active growth of the parasite.

Osterhout has continued his investigations on the electrical conductivity of *Laminaria* disks under different conditions, and has correlated changes in the conductivity with changes in permeability. Among the questions examined by him by this method is that of antagonistic effects, which he finds between sodium chloride and sodium taurocholate ("Decrease of Permeability and Antagonistic Effects caused by Bile Salts," *Journ. Gen. Physiol.*, **1**, 405–8, 1919) and between sodium chloride and various alkaloids ("Antagonism between Alkaloids and Salts in Relation to Permeability," *Journ. Gen. Physiol.*, **1**, 515–19, 1919).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S., Zoological Laboratory, Cambridge.

In recent anthropological literature, first attention should, perhaps, be given to the *Proceedings of the Prehistoric Society of East Anglia*, of which the section for 1919–20 (vol. iii, pt. 2) has just been issued. As readers of Science Progress are aware, this society has been progressing most creditably during the last few years, and the new *Proceedings* are even more interesting than the earlier publications. The first article is entitled "Man and the Ice Age," and is the presidential address

delivered by Prof. J. E. Marr on March 17 last. The article deals in a comprehensive manner with the problem of the relation of the different so-called palæolithic epochs to the glacial phases in Great Britain. Notwithstanding the immense literature dealing with this subject, there is much which is interesting and highly stimulating in Prof. Marr's article. He finds that there is satisfactory evidence of four cold periods (with, of course, three intervening relatively warm ages), the first of these cold periods being Pliocene, and the other three Pleistocene. The author deals in special detail with the Pleistocene beds of East Anglia, namely the Cromer Till, the Middle Glacial Deposits, and the Chalky Boulder Clay. After dealing with the finds in various localities, Prof. Marr comes to the conclusion that the Chellean implements are older than the Chalky Boulder Clay, which represents one of the great glacial periods. The Chalky Boulder Clay was laid down in the third of Prof. Marr's four cold phases, the fourth being represented by the so-called Northern Drift of Wales. The reader will notice at once the resemblance of this fourfold classification of the glacial periods to the famous fourfold classification which Prof. Penck put forward for the Alpine region. Prof. Marr appears to think that it would be somewhat premature, however, to attempt to correlate his results with those obtained in Continental countries; but one of his colleagues in Cambridge, Mr. M. C. Burkitt, does not agree, and one of the later articles is an extraordinarily interesting attempt by the latter writer to make such a correlation. Mr. Burkitt does not correlate the four periods of Prof. Marr with the four great glacial periods of Prof. Penck. He correlates the age of the Chalky Boulder Clay with the last great glacial period of the Alps—the Würmian. And he thinks that the "Northern Drift" of Wales is to be correlated with the minor "Bühl stadium," a cold phase which occurred shortly after the Würmian period, and which was separated from the latter by the less cold "Achen Recession," an age which was much less warm than the present day, or than the real interglacial periods. In this way Mr. Burkitt is able to fit in Prof. Marr's scheme with the current French opinions, according to which the Aurignacian and the whole of the later Palæolithic must be regarded as post-Würmian. The argument is not, however, altogether convincing. And although it is perhaps unfair to criticise a thesis which has obviously suffered from the necessity of extreme brevity, the logic of the correlation, and of the dating of the Continental periods themselves, seems to be somewhat unsatisfactory. I would suggest that the apparently obvious correspondence of the four English periods with the four periods of Penck should be given very full consideration.

In this case it may well be that that which is obvious is also true. Penck's fourth period (like the "Northern Drift" of Wales) was admittedly less severe than the preceding period, and it must not be forgotten that Penck himself placed the Chellean age in his second, not in his third, interglacial period. Much greater space is needed for dealing with questions of this character, but I would suggest that the Chalky Boulder Clay may correspond, not with the Würm, but with the Riss (third) glacial period of the Alps.

Among the other articles in the *Proceedings* is a contribution by Prof. W. J. Sollas, entitled: "A Flaked Flint from the Red Crag." After dealing in a critical manner with another of the sub-Crag flints, Prof. Sollas adds a postscript to the article accepting as human artefacts certain of the sub-Crag flints, though he suggests (quite rightly, I think) that they were made, not by man, but by some sub-human tool-making animal. This conversion of Prof. Sollas is somewhat notable, as he was for long one of the strongest opponents of the sub-Crag flints.

In the *Proceedings of the American Philosophical Society*, vol. lix, No. 3, an interesting, if not very analytical, article on "Slav and Celt" is contributed by J. D. Prince. The author thinks that there is much in common in the mentality of these two groups of peoples.

The following articles on physical anthropology may be mentioned:

In the *American Journal of Physical Anthropology*, vol. ii, No. 4: "Eruption and Decay of Teeth in Negroes and Whites," by V. Suk; and "Lefthandedness," by A. L. Beeley. And in *Man* (August): "Notes on the Physical Anthropology of Certain West African Tribes," by L. W. G. Malcolm.

And the following articles on prehistoric anthropology may be mentioned:

In the *Proceedings of the Royal Society*, Series B, vol. xci. No. B, 640: "On Some Rostro-carinate Flint Implements and Allied Forms," by Sir E. Ray Lankester. And from the *Proc. Prehist. Soc. E. Anglia* (as above): "A New Celt-making Floor at Grime's Graves," by D. Richardson; and "Implements from the Glacial Deposits of North Norfolk," by J. Cox.

MEDICINE. By R. M. Wilson, M.B., Ch.B.

Introduction.—The writer of the following notes on medical progress does not profess to deal with this from the point of view of the special worker. Rather he addresses himself to all who are interested in scientific thought, and who desire to follow what may perhaps be called the larger movements. Medicine is at the moment in one of its transition stages, and the future is by no means so clear as some appear to think. It is

the object of these notes to trace the main lines of development as they reveal themselves. Owing to the exigencies of space, this cán only be done in outline.

The Third Partner in Disease.—The last few months have witnessed a definite movement away from the purely bacteriological attitude to disease. This does not, of course, mean that anyone doubts that bacteria are the causes of the diseases attributed to them. It means rather that, taken alone, they are probably not so harmful as was supposed a few years ago.

Thus it is now generally accepted that tubercle bacilli are found everywhere, and that most people harbour them at one time or other. Many people seem to harbour them continually. In these, however, they are purely saprophytic and inflict no injury. It is only when some third factor or partner comes into play that the harmless bacillus becomes the deadly parasite.

What this third partner may be is not yet at all clear. In the case of phthisis, silica dust certainly plays such a part, and so gives rise to stonemason's consumption. But it is evident that silica is but one of many agents which are capable of acting in this way.

Again, it has been shown recently that tetanus bacilli or the bacilli of gas gangrene, *if washed* and then infected, remain harmless. No evil effects occur unless at the same time or at a later period an ionisible salt of calcium is injected. If this is done, however, tetanus or gas gangrene rapidly supervenes. Thus the calcium salt plays the part of " third partner," a part clearly taken in nature by the dirt which usually contaminates the wounds from which tetanus and gas gangrene arise.

We may add to these two illustrations a reference to the work of Besredka, of the Pasteur Institute in Paris, on immunity in dysentery and the typhoid group. He found, as others had found before him, that rabbits are singularly insusceptible to paratyphoid infection. On the other hand, if a rabbit had been given some ox-bile previously, it became highly susceptible to this disease. Thus the ox-bile (or similar intestinal irritant) seemed to play the part of third partner, without which infection, even in the presence of the specific germ, could not take place.

The importance of this new trend of opinion is obvious. For, in general, the efforts which have been made to prevent disease by abolishing bacteria have failed completely. A new hope is presented by the idea that, though bacteria cannot be got rid of, the chain of events and circumstances on which their activities depend can be broken. In short, if we can vary or remove the third partner, we may be able to prevent the onset of the disease.

This conception bears a resemblance to the methods employed in the case of tropical disease. Thus, in malaria

the third partner is the mosquito, and the chain can be broken by destroying its breeding places ; in plague it is the rat and the flea ; in Malta fever the goat ; in typhus the louse. The enormous success which has attended the work of the tropical sanitarian in dealing with these pests is full of encouragement.

Finally interest, having to some extent moved from the laboratory, passes again to the victims—the men and women. It passes, too, to their work, and is focused on factory and mine, where so many of the third partners of disease are located. The reason why stone-cutters are specially liable to phthisis, and why coal-miners escape this disease, becomes of vast moment, as does the unusual susceptibility of printers to the same malady and the unusual immunity of engine-drivers. Thus medicine goes to the homes and workshops of the people in search of its new material. The new Science of Industrial Hygiene, which is as yet an infant, is the normal expression of this change of attitude.

The Treatment of Malaria.—The treatment of malaria has proved a matter of vast importance since the close of the war. The great number of infected men from the Eastern theatres of war who have sought help from the Ministry of Pensions has caused this Department to take active measures to clear up various points of difficulty. In that task the Ministry has been assisted by Sir Ronald Ross and other well-known special workers. Indeed, it was Sir Ronald who first suggested the " tropical disease clinics " which now play so important a part in the handling of malaria, dysentery, and other cases.

Nor have these clinics failed to assist the progress of medical knowledge. As a result of prolonged study and trial, Sir Ronald Ross has found that, if good results are to be obtained in malaria cases, the administration of quinine must be carried out on definite principles. The patient is carefully instructed in these principles, and is thus invited to become an active partner in the work of his cure. He is provided with a mixture containing quinine, and told to take his dose each morning immediately before he sits down to his breakfast. Each dose consists of 10 grains of quinine hydrochloride. The treatment is continued for three months.

There is nothing haphazard in this method, for Sir Ronald's earlier researches have shown that three months is the period required to reduce the parasites to vanishing-point, taking the daily death-rate of parasites at a definite proportion of the total present.

In practice the method has proved exceedingly satisfactory, and has undoubtedly resulted in a large saving of public money. What is more important still, it has helped large numbers of men who, in other circumstances, must have drifted to chronic ill-health.

ARTICLES

THE INHERITANCE OF ACQUIRED CHARACTERS

By Prof. E. W. MacBRIDE, F.R.S., D.Sc.,
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The term " acquired character " is a rather ambiguous one. Inasmuch as all the higher animals start their existence as fertilised eggs of similar shape and of similar general structure, though of very different sizes, as these eggs will fail to develop unless external conditions are favourable, there is a sense in which all the typical characters of the various kinds of animals may be said to be " acquired." It was in this sense that Prof. Adam Sedgwick used the term " acquired character " in his article on " Embryology " in the 11th edition of *The Encyclopædia Britannica*; but it is, of course, not in this sense that the inheritability of " acquired characters " has formed the subject of controversy.

Everyone recognises that, given healthy normal surroundings, every kind of egg develops in a special typical way; for example, a perch egg develops into a perch, and a shrimp egg into a shrimp, and so on. If circumstances are changed and become unfavourable, the typical development may fail to come to full fruition, and a stunted, half-developed abortion may result; but no change of circumstances will induce a perch egg to develop into anything like a shrimp, or vice versa. But in certain cases, when the typical shape has become well marked, a change of circumstances may produce some slight modification of the type; as, for instance, when cattle are driven up on to high cold moors, and develop in consequence a thicker coat of hair, or when a blacksmith by exercise develops abnormally the biceps muscles on his arms. The belief in the inheritability of acquired characters is a belief that changes in habit and in the environment of the kind which we have just mentioned produce an effect, not only on the animals directly exposed to them, *but also on their offspring.*

It is to be observed that the changes produced by an altered environment are not to be thought of as the direct effects of

external forces acting on the animal as passive material, but rather as the expression of the response or *reaction* of the animal in its endeavour to *adapt* itself to the changed circumstances. This may be made quite clear to the reader unversed in zoology by considering a very well known phenomenon, viz. the two kinds of sunburn.

The observant onlooker at one of our seaside resorts would be struck by the fact that summer visitors exposed to an unwonted glare of sunshine exhibit two different types of effect. Some become scarlet and suffer a good deal of pain; the skin peels off, leaving raw surfaces, which only slowly heal. In others, however, the skin acquires a deep brown colour, and there is no pain and little or no peeling. The first exemplify the direct effect of sunlight as a destroyer of tissue; the rays of high frequency cause necrosis of the epidermal cells, and pain and inflammation result; but in people belonging to the second category, the body reacts to the unwonted stimulus of the bright light; cells laden with dark pigment migrate to the surface, and screen the underlying tissue from the deleterious effect of the rays. In a word, such people become *used to* the bright light and suffer no further inconvenience; and by the inheritance of acquired characters, we mean simply the inheritance of the effects of *use* and *disuse*. The effects of disuse are, of course, the converse of the effects of use, and are even better known. If a person becomes slightly lame in one foot, so that he acquires the habit of resting mainly on the other, the muscles on the unused side will in the course of a few years dwindle so much that the one leg becomes a feeble copy of the other.

The theory of the inheritability of the effects of use and disuse is usually termed Lamarckism, because it was precisely formulated by Lamarck in his *Zoological Philosophy*, from which the following sentences are taken (English translation by Elliot):

(1) Every fairly considerable and permanent alteration in the environment of any race of animals works a real alteration in the *needs* of that race.

(2) Every change in the *needs* of animals necessitates *new activities* on their part for the satisfaction of those needs, and hence new habits are formed.

(3) Every new need which necessitates new activities for its satisfaction requires the animal either to make more frequent *use* of some of its parts which it previously used less, and thus greatly to *develop* or *enlarge* them, or else to make use of entirely new parts to which the new needs have imperceptibly given birth by inner feeling.

(4) All the acquisitions or losses wrought by nature on individuals through the influence of the environment in which

their race has *long* been placed, and hence *through the influence of the predominant use or permanent disuse* of any organ—all these are preserved by *reproduction* to the new individuals which arise.

The influence of Lamarck's views on Continental believers in the doctrine of evolution, or as they termed it "transformism," was very great. In England it was, of course, completely overshadowed by the doctrine of "natural selection" propounded by Darwin in 1859. But Darwin did not deny that the effects of use and disuse were inheritable. In the 6th and last edition of the *Origin of Species*, we find that he attributed to the inherited effects of disuse the diminution in size of the wings of flightless birds, and the reduction and final disappearance of the eyes in cave animals. The theory of natural selection, however, postulated an indefinite variability in all the organs of all animals; and it postulated, further, that these deviations from the mean could be inherited. But it did more than this, for it implicitly assumed that, if one of these deviations were selected for propagation, the offspring of this exceptional individual would deviate in all directions from the mean represented by the characters of their immediate parent, not from the mean represented by the characters of the grandparent. If these assumptions were justified, it followed that any supposed inheritance of the effects of use was completely superfluous, for any amount of deviation from the mean might result from the continual selection, in the struggle for existence, of those individuals which showed a beneficial variation in the highest degree.

If, however, we were to assume that the effects of disuse were to be accounted for in the same way, we should be committed, as Darwin clearly saw, to the difficult and doubtful position that the reduction in size of a useless organ was such an advantage in the struggle for life as to determine the survival of its possessor. Therefore Darwin conceded the *principle* underlying Lamarck's theory, but regarded it only as one of the factors leading to variation, and therefore evolution.

Much the same position was taken up by Haeckel, the doughty protagonist for Darwin's views. In the *History of Creation* (vol. i, 2nd edition, 1876) we read:

"The gardener as well as the farmer avails himself of the fact of Inheritance in its widest form, and indeed with special regard to the fact that *not only those qualities of organisms are transmitted by inheritance which they have inherited from their parents, but also those which they themselves have acquired.* An organism can transmit to its descendant, not only those qualities of form, colour, and size which it has inherited from its parents, but it can also transmit changes of these qualities

which it has acquired during its own life through the influence of outward circumstances, such as climate, nourishment, training, etc."

The opinion that acquired characters are not inheritable, and have therefore played no part in evolution, was first put forward by Weismann.

When we turn to what he says on the subject in "Germ-Plasm," we are amazed to read: "It is easier to explain the transformation of species on Lamarck's principle, *but this is no reason for the retention of a theory which cannot be accepted on theoretical grounds*, unless no other explanation can be given for the facts." Weismann does not deny that the modifying effects of climate are in some cases transmissible by heredity; but he endeavours to explain this away by the supposition that climate acts directly on the germ-cells; that is to say, he commits himself to the extraordinary position that climate can directly affect the germ-cells buried deep within the tissues of the body, but that changes on the bodily tissues in which these germ-cells are immersed are totally without effect on them!

His theoretical grounds against admitting the transmissibility of acquired characters are:

1. The view that the bearer of heredity is the chromatin in the nucleus of the cell—as witnessed by the fact that the whole of influence of the father on the character of the offspring is carried by the head of the spermatozoon, which is a condensed nucleus consisting only of chromatin.

2. The view that the primary germ-cells which eventually give rise to the genital cells of an individual are set aside at an extremely early period in the development of the egg: that the other cells which constitute the "soma" (body) pursue an independent development, and hence that their subsequent changes cannot affect the nature of the germ-cells, the characters of which are fixed from the outset.

Now, of these theories, No. 1 has been confirmed by later research; but No. 2 is really ludicrous. Even supposing, which is not the case, that definite "germ-tracks" had been shown to exist in the developing eggs of all animals—that is to say, that it has been shown that the lineage of the germ-cells had always been traceable to undifferentiated cells amongst the blastomeres which result from the cleavage of the egg—that would not prove that their hereditary potentialities were unalterable by changes in the body fluids which bathe them.

But definite germ-tracks have been shown to exist in very few animals; the most evident case is in the development of Nematoda. In these worms, as the egg divides, the nuclei of some of the blastomeres undergo changes which appear to be degenerative, and the cells to which these nuclei belong give

rise to the tissues, epithelial, digestive, and muscular, of which the body is built up. There finally remains only one blastomere, whose nucleus does not undergo this degenerative change, and this blastomere divides into two cells, which remain passive during all the embryonic development, and even the youth, of the worm, and then give rise to the genital organs. From this development Weismann draws the conclusion that the nuclear structure varies in different cells of the body, and that only the genital cells retain the complete structure requisite to form the whole body .

Unfortunately, however, for Weismann, this case has been critically examined by Boveri. It occasionally happens that the eggs of Nematodes are entered by two spermatozoa, and then abnormal development results which can exhibit several types. From the comparative study of these types Boveri arrives at the conclusion that the phenomenon of "diminution of the chromatin," as the degenerative change is called, has nothing to do with an unequal partition of nuclear material at a cell division, but is the result of the particular type of cytoplasm in which the nucleus is immersed.

But in fact Weismann's objections are all bound up with his idea of the scheme of development of the egg. He maintains that in the nucleus of the fertilised egg there is hidden a sort of miniature picture of the adult animal, and that development consists in translating this plan into adult structure. "Ontogeny, or the development of the individual, depends on a series of graduated qualitative changes in the nuclear substance of the egg-cell." "The independently and hereditarily variable parts of the body therefore serve as an exact measure for determining the ultimate particles of which the germ-plasm is composed." He accordingly conceives of the building up of the body of the animal as the putting together of the pieces in a picture puzzle, so that each part will only fit into the whole in a particular way.

This conception has, however, received a number of rude shocks in recent years. Even Weismann encountered enormous difficulties when he descended to details. If the nucleus of a cell in the developing rudiment of a leg, for instance, contains only the potentialities which will allow to form the tissues of the particular part of the leg in which it is destined to be placed, how does it come about that, when the leg of a newt is cut off, it can be regenerated ? Obviously these nuclei must contain, besides their ordinary "plasm," some "germ-plasm," and yet this germ-plasm cannot re-form a whole animal, but only a leg.

Then again, if the egg of a Frog or an Ascidian be allowed to segment into four cells, and these four cells be separated along the proper plane, or if the appropriate two be killed, the sur-

viving two cells form a half-tadpole, and the same is true of the egg of a Ctenophore from in which a half-larva can be produced. This seems to fit in beautifully with the picture-puzzle theory of development, but what are we to say of the further perplexing fact that, if the half-tadpole or half-larva remains alive, it will "post-generate" the missing half? But worse blows than even this have been rained on the theory by the progress of experimental embryology. The eye of a vertebrate (leaving out lesser detail) consists of two essential parts, the lens and the retina. The lens is formed from the skin of the side of the head, but the retina originates from the brain. If, now, the skin of a young tadpole be slit open, and the earliest rudiment of the retina be cut off from the brain and pushed back under the skin till it attains the level of the shoulder, or even farther back, the rudiment will continue to live and grow in its new position, and *will compel the skin covering it to form a lens*, although this skin has never in the history of the race formed a lens before. We are really entitled to ask whether this area of skin acquired the "determinants" for a lens in anticipation of the enterprise of a biological investigator? Or, again, if the lens of a newt's eye be cut out, the edge of the retina will bud off a new lens. How is the animal able to foresee this experiment and repair the damage that results? Finally, although the diligent search of Weismann's pupils has enabled them to detect the first traces of germ-cells at an early stage in the development of Vertebrates, yet later investigations have shown that, although in the frog, for instance, these early-formed germ-cells are undoubtedly present, they are not numerous enough to account for the eggs that a female frog lays during its first breeding season, and that many of these, and all those laid in subsequent seasons, are developed from the modification of ordinary tissue-cells! In the white mouse the early-formed germ-cells are all absorbed, and all the viable eggs are produced from peritoneal cells. In fact, all the trend of modern investigation is to show that all the nuclei produced by the division of the egg-nucleus are potentially alike, and that what undergoes differentiation is not the nucleus, but the cytoplasm.

But, although the detailed scheme of the germ-plasm as put forward by Weismann has proved to be untenable, the idea of the unchangeable character of the hereditary potentialities of the egg has taken a firm hold on the minds of Mendelian investigators. As Punnet phrases it, "The creature is not made, but born." What Mendelians have shown is that when a "mutant," which almost always differs from the type in the loss of some feature, is crossed with the type, a hybrid is produced which gives rise to two kinds of germ-cells, one like the mutant and one like the type. They have further put forward

the plausible suggestion that the mutations arose in the first instance by some accident or irregularity in nuclear division, so that one of the two daughter nuclei which were produced received less than its share of the chromatin. When once this has occurred, if such a defective germ-cell unites with a perfect one, the peculiar nuclear divisions which usher in the ripening of the germ-cells in the resulting organism will account for the production of two types of germ-cell.

When we read over the list of "mutations" which have cropped up in the cultures of Mendelians, we are struck with the fact that practically all represent deficiencies and pathological aberrations totally unlike the marks which separate natural species from one another. Dr. Bateson, in his presidential address to the British Association in 1914, even tentatively suggested that evolution might have consisted in the development of a series of defects, and that all the qualities of Shakespeare were implicit in the original amœba. He was far too good a naturalist to suggest that such a theory should be adopted; he only indicated that such a possibility should be considered. When we contemplate, however, such mutants as white-eyed flies, flies with wings the layers of which are separated by bubbles of air, flies with shrivelled wings, we come to the conclusion that such deviations from the normal have played no part in evolution.

If such mutations, then, have not formed the raw material for evolution, can this have consisted in the trifling differences in size, form, and colour which, as Darwin pointed out, are to be found amongst the members of every family, brood, and litter, differences which, he assumed, could be indefinitely increased by selection continued through many generations? To this theory the pure-line investigations have given the death-blow. As an example of these we may take the investigations of Prof. Agar on the heredity of the water-flea Simocephalus.

This is a little Crustacean provided with a bivalve carapace. The space between the body and the carapace serves as a brood-pouch, and into it the eggs are laid, and they are carried about by the mother until they are ready to take up an independent existence for themselves. These eggs develop parthenogenetically, that is without being fertilised by a male. Now, if we select a single female and isolate her, and rear her progeny to maturity, when in turn they will produce eggs, we establish what is termed a "pure line." Amongst the brood there will be slight variations in the length of the carapace. If we endeavour, however, to get a strain with longer carapaces by selecting the longest individuals for further propagation, we fail; and the same fate befalls us if we try to raise a strain with shorter carapaces. Agar found, in fact, that the average carapace length

of the progeny of short individuals was actually longer than the carapace length of the offspring of long individuals. But in any group of water-fleas selected at random there are several different strains which tend to give rise to groups of individuals with different standards of carapace length; and if we select from such a population all those with longer carapace for further breeding, we do get a population with a longer average length of carapace, owing to the elimination of all those strains with short standards of carapace length. It was this circumstance that gave to Darwin the illusion that it was possible *by selection alone* to increase or diminish the size of an organ indefinitely; but when once the population has been purified by the elimination of all strains with different standards from that aimed at, progress comes to an abrupt end.

An exactly similar result has been arrived at by Jennings, who studied the reproduction of the Protist Paramecium (the Slipper Animalcule), and by Johannsen, who studied the weight of beans produced by the scarlet runner. If we consider how far apart in the organic scale a flowering plant, a Crustacean, and a Protist are, and how concordant are the results arrived at, we shall gain a strong conviction that *selection alone, when the environment remains constant, is powerless to effect evolution.* If once this conclusion be granted, the whole supposed advantage of Darwin's theory over Lamarck's disappears; the pathological mutants which Mendelians employ for their experiments are utterly unlike the variations which the study of comparative anatomy induces us to postulate as the material of evolution, and we seem to be driven back on Lamarck's view of the inheritability of the effects of use and disuse as the only alternative.

This feeling is shared by a large number of palæontologists. In the early days of this science, no exact series of related forms had been discovered, but more recently whole successions of closely related forms preserved in strata, following each other directly in time, have come to light. No reasonable person doubts that in these successions we have the actual records of evolution before us. If we now study the changes which take place in these series, they bear no relation to the "mutations" found as sports in gardens and by cattle-breeders, but they do closely resemble those enlargements and diminutions of organs which Lamarck stated would result from the effects of use and disuse, and they can be shown to occur independently in animals of quite distinct ancestry.

We can trace the ancestral horse from a stage in which he had tapir-like feet, and evidently frequented swampy ground, step by step until, by the gradual shrinking of the superfluous toes, he was transformed into the one-toed animal we are

familiar with to-day; and an exactly similar transformation can be followed in an extinct family of South American Ungulates, resulting in the one-toed Thoatherium. Similarly we can trace the ancestry of the camel from the time when it was a graceful antelope-animal with slender feet, until by its migration on to sandy ground it became the splay-toed beast of the present.

Not only Palæontology, but likewise Embryology, strongly suggest that Lamarck's theory is true. As our space is limited, we shall select one case to illustrate this. The Hermit Crab differs from other Decapod Crustacea in having a curved abdomen covered only with thin skin, which it protects by thrusting this delicate member into an empty gastropod shell. The appendages of the abdomen are developed only on one side. Now, the young Hermit Crab swims like a shrimp in the sea; it possesses a straight abdomen with all the appendages developed, and in this it resembles an ordinary shrimp or a lobster. Then it sinks to the bottom, searches for an empty shell, and thrusts the abdomen into it. Now, if all such shells have been previously removed, the search is naturally unsuccessful; but the Hermit Crab's abdomen becomes curved just the same, though not to the same extent as would have been the case had the shell been forthcoming.

How are we to explain this metamorphosis on the "mutation-theory" or even on the old orthodox Darwinian theory? Are we to assume that, in an ancestral race of shrimps enjoying a free life at the surface of the sea, chance mutations occurred, leading to the formation of a curved asymmetrical abdomen, together with a desire to hide the deformed member in a gastropod shell? Is it not infinitely more likely that when certain shrimps began to seek their food on the bottom—because of its abundance there—and turned into the forerunners of our crayfish and lobsters, they all began to hide their most vulnerable parts in any crevice they could find, as lobsters do to this day? Further, that some of them, finding in a gastropod shell a powerful protection, began to thrust their abdomens into these, and so checked growth on one side and promoted it on the other, and that this habit of growth, after many generations, became so ingrained in the constitution that it appeared even before the shell was found. Those who acquired this trick gained an advantage over their allies in that the abdomen was *never* exposed, since the animal when it had to move carried its protection with it, whereas lobsters could be attacked when they emerged from their retreats.

But an ounce of fact is worth a ton of theory, and the answer of Weismann and the Mendelians to the supporters of the Lamarckian doctrine has always been that no experimental evidence has been forthcoming to prove the effects of use and

disuse can be inherited. Indeed Weismann has gone further, and has claimed to have produced evidence that these effects cannot be inherited.

This evidence is not a little extraordinary. If we turn to his *Essays on Heredity* (English translation by Poulton, Schönland, and Shipley) we read: "It can hardly be doubted that mutilations are acquired characters . . . they are merely the reaction of the body to external circumstances." He then proceeds to recount how he cut the tails off a series of white mice, bred from them, found that the young were born with tails, and triumphantly concludes that he has proved that "acquired characters" cannot be inherited. It seems incredible, but it is, nevertheless, true that on the foundation of childish experiments like these are based the confident assertions in many textbooks (especially American), that Weismann had demolished the case for the inheritance of acquired characters. Comment is superfluous!

During the last ten years, however, Kammerer, working under Przibram in the Institute for Experimental Zoology in Vienna, has carried out a series of experiments which, if confirmed, settles the question of the inheritability of acquired characters for ever in the affirmative. Naturally his results have aroused a storm of opposition, but his most keen sighted opponents have seen clearly that the results are far too clear to allow of the supposition of genuine mistakes in interpreting the evidence. Either Kammerer has proved his point, or else he is acting in scandalous bad faith, and this second alternative, more or less concealed in diplomatic language, has been adopted by many.

The subjects of these experiments have been chiefly Amphibia. A few of them may be briefly outlined here. There exist in Europe two salamanders, one the yellow and black form (*Salamandra maculosa*), which inhabits the lowlands, and the other a black species (*Salamandra atra*), which lives at high altitudes. Both are viviparous; but *S. maculosa* gives rise to 30–40 gilled young, which live in water for six weeks before losing their gills and metamorphosing into the land form. *S. atra*, on the other hand, gives birth to only two young, which are at birth land animals, and ready at once to take up the parental mode of life. But if we cut open a pregnant black salamander, we find inside her at least a dozen embryos. Of these, however, only the two situated furthest back and nearest the openings of the oviducts are destined to survive; all the rest in the course of development degenerate into a kind of soup, which is devoured by the hindermost and serves to feed them. These two lucky embryos have long gills, but these gills are absorbed before birth.

Now, Kammerer asserts that, if *S. atra* be gradually accustomed to live under warmer and moister conditions, she will begin to produce first three and ultimately four young at a birth, that these young will enter the world at an abnormally early period of development—even before the gills are fully absorbed; that if these young be reared to maturity under the same conditions, they will give rise to still more young at a birth, and these young will be provided with gills, and will take to the water—in a word, that *S. atra* can be induced to assume the habits of *S. maculosa*, and that these habits will be transmitted to posterity.

Some of Kammerer's critics, while accepting this result, have maintained that it is not a genuine case of the inheritance of acquired characters, because the habits to which *S. atra* reverts under the influence of more favourable conditions are the ancestral habits of the race. But this objection involves a confusion of thought. When a habit and the structures associated with it have completely vanished, if by exposure to different conditions the old habit is reacquired, and descends to subsequent generations, the case is just as genuine an instance of the inheritance of an acquired character as if the new character were one totally foreign to the past experience of the race. It is a superstition to think that evolution is bound to go forward; it may go backward as well. But Kammerer has a neat way of outflanking this silly objection. He has performed the reverse experiment. If *S. maculosa* be subjected to conditions of increasing cold and dryness, it produces fewer at a birth, and these are born in a more advanced stage of development. In three generations a state of affairs is reached when only three to four are born at one time, and in these the gills are mere stumps, the gill clefts closed, and the animals can at once take up their life on land; in a word, *S. maculosa* has acquired the habits of *S. atra*. Now, if *S. maculosa* be taken as representing the ancestral condition of *S. atra*, *S. atra* cannot be ancestral to *S. maculosa*.

Another experiment of Kammerer's which has raised an enormous amount of discussion has for its subject the midwife toad *Alytes obstetricans*. Most toads, though they spend a good deal of their time on land in cool damp spots, nevertheless resort to the water in order to pair. Here the male embraces the female round the waist, maintaining his hold on her by means of a roughened horny pad situated on the ball of the apparent thumb. (The real thumb is absent; the apparent thumb is the modified index finger.) After a period of sexual enjoyment which may extend over weeks, the female emits the eggs, which issue from her in two long strings, held together by a clear jelly-like substance. As the eggs appear, the male fertilises them by emitting the spermatozoa; the fertilised

strings sink to the bottom of the water, and in due time small tadpoles hatch out, each provided with three feather-like gills at the side of the neck. As the tadpoles grow, a fold of skin, termed the operculum, grows back from the head and covers up the gills, and the body of the tadpole then assumes the round plump form familiar to all of us. But Alytes differs from all other toads in the fact that it pairs on land; and as the skin of the female is comparatively rough and dry, the male does not develop the horny pad on his hand, since he can hold her without it. The eggs are much larger than those of ordinary toads, and fewer in number. As they are emitted and fertilised, the male winds the egg-strings round his legs and hops away encumbered with them. Some weeks later he visits the water, and the eggs are hatched; tadpoles emerge which have already covered up their gills, and resemble the later stages of the tadpoles of other toads.

Now, Kammerer found that, if Alytes was accustomed to warm dry conditions, it would still flourish if a tank was provided in which the toads could bathe themselves when they so desired. Under these circumstances they begin to pair in the water, and the egg strings become slippery and fall off the male's legs and lie in the water. Most of them perish, but if the water is kept perfectly sterile, a few will survive. These, when they reach maturity, will pair in the water—and the eggs produced by the female will be much smaller than normal, and more like those of ordinary toads, and the tadpoles will emerge in an earlier stage of development, showing the external gills.

Of these, however, there will only be one on each side—and only *one* external gill is found in the embryos of the normal Alytes, if the eggs are opened before hatching. If, however, a third generation of Alytes is raised under these conditions, eggs will be produced from which tadpoles will hatch out showing *three* external gills on each side as in ordinary toads; and these eggs *when reared will yield males with horny patches on their hands.* As such Alytes had never previously been recorded, this point was fixed on by Dr. Bateson, the leading English Mendelian, to test the reliability of Kammerer as an investigator. He visited Kammerer's laboratory and demanded to be shown these abnormal toads, and this wish Kammerer was either unwilling or unable to comply with. Hence Dr. Bateson felt justified in regarding these results with great suspicion; this was a few years before the war. In 1919, however, Kammerer published a further paper on the subject, giving the results of renewed experiments. He succeeded in rearing these modified Alytes through six or seven generations, and found that the horny pad increased in size and definiteness up till the fifth generation, after which it remained stationary. He explained his inability

to satisfy Dr. Bateson by saying that, as in ordinary toads so in the modified Alytes, the horny pad was only developed in the breeding season, and was a temporary phenomenon. He said that in this, as in other experiments, he had had to decide whether to retain his specimens alive for further work, or to sacrifice them in order to satisfy objectors, and that he had decided on the former course.

Since publishing this paper, however, Kammerer has sent sections through the skin of the hand of the abnormal Alytes to Dr. Bateson, and these Dr. Bateson has been good enough to show to the author of the present article, and in these sections the horny pad with its prickles can be clearly made out.

It will occur to the reader that it is of cardinal importance to the theory of heredity that these experiments should be repeated, and indeed it was partly with the hope of stimulating naturalists everywhere to undertake this task that this article was written. But the difficulty of such work should be clearly realised. It is necessary to find an animal that will respond to a change of environment by a change of habit or structure, and this is by no means easy. If, however, this difficulty is surmounted, and the animal is reared to maturity under the new conditions, and is induced to breed, then the young have to be divided into two lots, one portion being allowed to remain under the new conditions, and the other portion restored to the old conditions. It most frequently happens that those offspring which are thus replaced in the ancestral environment lose the modification produced by the new conditions, and this is often triumphantly referred to by Mendelians as a proof that the modification was merely "somatic," and not "germinal. But this reasoning is fallacious. If the constitution of the animal is so labile that a change from condition A to condition B produces a certain modification, the reverse change from condition B to condition A should undo it. The utmost that we could expect to find—and what, indeed, Kammerer claims to have found in his salamander experiments—is that the offspring of the modified parents, when replaced in ancestral conditions, should in their early development still show traces of the modification (this is the essence of "recapitulation"), and that young exposed to the further action of the changed environment should exhibit the modification in an intensified degree.

In the Zoological Gardens in London attempts are being made to repeat Kammerer's experiments with the salamander. These experiments were begun a year ago, but we must wait between two and three years before the young exposed to the altered conditions become sexually ripe, and three or four years must elapse subsequent to this event before it is possible to

determine whether the altered condition is transmitted to the offspring. Meanwhile, new and startling evidence of the inheritability of acquired changes has come in from a totally new quarter.

Two American observers (Messrs. Guyer and Smith) have published in the *Journal of Experimental Zoology* (vol. iii, 1920) an account of their experiments on rabbits. They took the lenses of the eyes of rabbits, pulped them in Ringer's solution, and injected small portions of the resulting fluid into domestic fowls. After the lapse of a few weeks the serum of the fowl's blood developed an " antibody," which tended to dissolve and disintegrate the lens of the rabbit. Small quantities of this serum were then injected into pregnant rabbits. The mothers were unaffected, but some of the young which they bore showed on one or both sides of the head diminished or completely aborted lenses and correspondingly developed retinæ, for, as we have already seen, the development of one of these elements of the eyeball depends on that of the other. Many of these young with imperfect eyes died, but some survived, and these mated together, gave rise to young some of which showed the defect, and *this defect was observable through six generations, without any further injection of serum.* Two instances also of inheritance through the male alone were noted—that is to say, that an affected male mated with a normal doe gave rise to young some of which showed the defect.

Messrs. Guyer and Smith say : " It is a noteworthy fact that once the defects were established, *without any subsequent treatment,* they became more and more pronounced in successive generations." The authors suggest that " the degenerating eyes are themselves directly or indirectly originating antibodies in the blood-serum of their bearers—*which in turn affect the germ-cells.*"

If this conclusion be accepted, the cardinal principle of the theory of the inheritability of acquired characters, viz. the influence of the soma on the germ, is conceded.

We conclude, then, by saying that a very strong prima facie case for the inheritability of acquired characters has been made out, and that no serious efforts have yet been made to combat the evidence. We are entitled to use the principle of use-inheritance in endeavouring to explain the facts of Palæontology and Embryology, and when we do so, we find a flood of brilliant light thrown on these subjects, and a whole variety of puzzling phenomena become susceptible of rational explanation.

A REVIEW OF RECENT WORK ON ENZYME ACTION

By R. J. S. McDOWALL, M.B.

Introductory.—When reviewing the work recently done on enzyme action, nothing perhaps strikes one more forcibly than the farsightedness of Berzelius, who in 1837, when drawing the attention of chemists to enzyme action, recognised that the process was essentially catalytic in nature, and asserted that "in living plants and animals there take place thousands of catalytic processes between tissues and fluids." Since that time three generations of investigators have studied the subject, only to find how true the words of Berzelius were, although in the light of our present knowledge of the factors involved it is difficult to imagine that the author fully realised the significance of them.

Much of the advance in our knowledge of enzyme action has been made side by side with and is almost consequent on the great advances in our knowledge of inorganic catalysts, surface action and colloids; it is not, however, within the scope of this essay to deal with these subjects except as regards their bearing on the question under consideration.

Definition.—A short detailed definition is so difficult to obtain that the more general one of Euler is possibly the best. The following is from the English translation of Euler's *General Chemistry of the Enzymes*:

"The name *enzyme* is given to animal or vegetable substances which are able to accelerate chemical reactions. The term *enzyme* is included in the general term *catalyst.* By *catalyst* we understand a substance which, without being injured by the acceleration or appearing in the final products, alters the velocity with which a chemical system strives to attain its final condition."

Practically all enzymes are colloids, or are associated with substances having colloidal properties, and in a large number of cases they are found together with protein matter. Thus the amylase obtained by Sherman (1912–17), protease and

lipase show the characteristics of protein, while the invertase described by Nelson (1914) is a carbo-hydrate phosphoric acid complex containing about 1 per cent. nitrogen in the form of protein. The problem of preparing pure enzyme is a very difficult one, and as Falk states (1918): " As a result of the complex nature of the protein or other molecule which includes the enzyme or with which the enzyme may be associated, the problem of isolating pure enzyme must wait for the solution of the problem of the isolation of pure proteins possessing the properties which they exhibit in living matter, using the term *living* to include also matter showing the action of enzymes."

Initiation of Reaction.—If enzymes are to be considered as catalysts, they should be able as such to hasten but not to initiate reactions. It has been observed that starch solutions on keeping pass into dextrin or sugar, and it has also been found that the increase in electrical conductivity which occurs when trypsin acts on ammonium caseinogate will take place slowly if the caseinogate is left to itself. The fact, however, cannot be ignored that such an eminent chemist and physicist as J. J. Thomson considers it possible that a reaction may be begun by an enzyme, and indeed the recent work of McGuigan (1919) on ptyalin would tend to support Thomson's hypothesis at least with regard to that enzyme.

Where the process catalysed is one of hydrolysis, it may be assumed that it is slowly taking place as a result of the H and OH ions in the water of which the solution may be made; but the difficulty of recognising extremely slow reaction is very great. No work on this point appears to have been done in relation to oxidation processes which are known to be accelerated by enzymes.

Relation of Enzymes to Products of Reaction.—In true catalytic action the catalyst should appear unchanged at the end of the reaction, and in its original quantity, but in the case of enzymes this is not always demonstrable. In 1910 Starkenstein and subsequently Bayliss satisfied themselves that enzymes could be obtained unchanged; but the facts that enzymes tend to decay spontaneously when much diluted and that reaction may take place slowly even without enzymes give much experimental difficulty and may lead to erroneous conclusions; such as those of Bradley, who found (1910) that the amount of triolein hydrolysed by lipase was in proportion to the amount of enzyme present. If the enzyme itself played any part in the final state of the reaction, it is to be presumed that the addition of more enzyme would change the amount of the products. The work of Bayliss in 1913 shows that this is not so. Reference to further work on this subject will be made when dealing with " Equilibrium " and " Reversibility."

From what has been said above, it may be assumed that ample earlier evidence exists for the classifying of enzymes as catalysts. The factors governing or influencing this catalytic action will therefore be dealt with.

One of the most important physical properties of enzymes is their colloidal nature, which renders them insoluble in water in the usual sense, but readily allows them to form fine suspensions in what may be considered a very dilute solution of themselves.

Dispersion.—As colloids, therefore, enzymes have the great colloidal characteristic, viz. the enormous development of surface, and it will be seen that many of the properties of enzymes and of factors influencing them depend on the ultimate dispersity of the particles suspended. Important work was done on this subject by Nelson and Griffin (1916) and will be referred to later.

The amount of dispersion of the particles may be seen with the aid of the ultramicroscope, and by this means Casana (1913) found that the dispersion of trypsin particles was greater when the temperature was raised and the activity of the enzymes had increased. Similarly it has been found that the solubilities and action of such enzymes are related to the dispersive powers of the solvent. Thus glycerol, which has good dispersive power over colloids, is frequently used to extract enzymes.

De Jonge (1917), following up the work of Terroine (1910), came to the conclusion that the action of bile depended not only on the emulsification of insoluble fats, but that by lowering surface tension, with consequent increase of dispersity, colloidal aggregation of enzyme was prevented, or a greater surface developed as a result of the production of disaggregation. Further reference will be made to the work of this observer under "Adjuvants." This conclusion was supported by Groll (1918) after his investigation of the retarding effects of metals on enzyme action. He found that the retarding effect of NH_4 was greater than that of K, which was greater than that of Na, and so on, and considered the action to be similar to floccing and other colloidal phenomena. This led him to the general conclusion that very possibly the influence of all neutral salts on enzyme action was due to the fact that the dispersity of the enzyme was modified by the ion in the same way as other colloids. The work of Albert and Alexandre Mary in the same year, although unconfirmed, gives a striking instance of a similar action in relation to an inorganic catalyst. They found that colloidal silica can invert saccharose slightly, and that its activity increases in proportion to the degree of dispersity. They arrived at an optimum temperature, and also found absolute inactivation when coagulation was complete.

They conclude that temperature inactivation of natural enzymes was probably due to a degradation of the dispersed phase.

Surface Action.—It has been shown above that the action of enzymes is related to surface: enzyme action depends rather —to a large extent, if not entirely—on surface. One of the most striking discoveries made recently regarding enzymes is that of Bourquelot and Bridel (1913), who showed that emulsin could either hydrolyse or synthetise in 90 per cent. alcohol in which it was quite insoluble. Subsequently Bayliss (1915) extended the discovery to invertase, lactase, urease, catalase of blood, trypsin and pepsin; while Jacoby (1916) prepared from the soya bean, by extraction with absolute alcohol and evaporation, an active urease powder which was insoluble in water. Similarly Nelson and Griffin (1916), working in America, showed that invertase adsorbed by charcoal or aluminium hydroxide acted quite as well as the same amount in solution.

It is almost impossible to review the work done recently on surface action without mention of the theory of Faraday that inorganic catalytic action depends on the existence of a clean surface and that impurities which "spoil" the surface hinder catalytic action. In support of this theory Bancroft (1917) observed that in gas reactions the surface of the catalyst could easily be made inactive by the presence of foreign gases which were strongly adsorbed; and later (1918) he pointed out that the products of the action catalysed might themselves accumulate on the surface of the catalyst and appear to bring the reaction to an end, or at least to very much retard it. Such an action may be considered a dispossession from surface, and it is obvious that this may readily lead to erroneous conclusions by causing a reaction to appear complete when it really is not. Proof of the occurrence of such dispossession from surface was given by Bayliss (1918), who showed that the hydrolysis of urea by urease adsorbed in charcoal could be retarded by saponin, which displaced the urease in the charcoal. The fact that the urea was finally hydrolysed showed that the enzyme had not been destroyed by the saponin. This gives support to the work of Meyerhoff (1914), who, working with the invertase of yeast, found that the inhibitory effect of a series of alcohols and methanes was exactly proportional in homologous series to their power of lowering surface tension. It may be added that Bayliss found that not only did the saponin replace the urease in the charcoal, but that relatively more saponin was adsorbed at a low temperature, and that the rate of the reaction varied with the temperature. Working on the urease of the soya bean, Ondera (1915) showed that amyl alcohol retarded action in high concentration, and gave further weight to the work of Meyerhoff;

in addition, however, the former investigator found that the lower concentration of amyl alcohol accelerates the action of urease. This must be explained by the fact that the retarding influence due to dispossession of surface is more than counterbalanced by the increased dispersion. The work of the Armstrongs and of Benjamin and Horton (1913) had already led them to assert that beyond doubt enzyme action took place entirely at the surface of colloidal particles suspended in a solution of the hydrolyte and not between substances in true solution. The value of this possession of large surface by enzymes is seen when the phenomenon of adsorption is considered.

Adsorption.—Enzymes, like other colloids, are capable of adsorbing on their surfaces various other substances which may or may not be colloidal. In this process of adsorption the eznyme enters into a peculiar physical combination with the substrate as a result of the diminution of surface energy at the interface between the particles concerned. The existence of such adsorption compounds can readily be demonstrated by the method devised by Bayliss (1911). In this method when a colloidal solution of aluminium hydroxide is added to a dialysed solution of the free acid of congo red, the undissociated acid is thrown as a blue precipitate with the hydroxide. In fact the acid and base act together as an adsorption compound but are not combined chemically. If the precipitate be suspended in water and boiled, chemical combination readily takes place and the characteristic new colour of the salt is evident. Many other proofs could be given, but we need only concern ourselves here with those which refer to enzymes. Thus it is found that an enzyme may be protected from the effects of heat by being adsorbed. This will be referred to further under "Temperature." Or again, trypsin will pass through porcelain, but will not do so if in contact with a solution of caseinogate.

The exact factors which determine the occurrence of adsorption are, however, not clearly known. Like other colloids, enzymes are electrically charged. The importance of this factor has been urged, but it is found that the kind of charge possessed by enzymes may depend on their reaction, and whereas it is but natural that adsorption may be facilitated between colloids of opposite charges, it is difficult to understand why adsorption should take place between colloids of the same charge, or even uncharged. Further reference will be made to this subject under "Electrolytes." Northrop (1919), on finding that the optimum digestion for an enzyme corresponded to the optimum ion concentration for the combination, suggested that the enzyme combined with the ionised protein

and that adsorption was in some way related to the amount of ionisation. In the same paper, however, he notes that charcoal could remove as much enzyme as albumin and even more than casein—facts which do not support his hypothesis. The exact significance of the experiments will be seen later to depend on the condition of the substrate as investigated by Ringer.

Specificity.—The question of adsorption in relation to enzymes is further complicated by their specificity. Specific catalysts and specific adsorption are known to exist in the inorganic world, but specificity is more marked in the case of enzymes, although many would reason that the specificity is by no means absolute, and in the light of further knowledge may be found to be non-existent. Many investigators are just as determined in the opposite direction. It is perhaps but natural that the question should have excited much controversy, as it is so closely related to that of the adaptation of plants and animals.

Following up the early work of Duclaux, who showed that the enzymes of yeast differed according to the medium on which the yeast was grown, Abderhalden and Kapfberger (1910) found that on injection of protein and carbodydrates enzymes were developed which hydrolysed these substances. This work however has not been wholly verified. In 1916 Hulton found that parenteral injection of protein does not increase the proteoclastic enzymes in rabbit serum; but Watanabe (1917) claimed that the blood diastase in rabbits was increased by the injection of human saliva and the parenteral administration of starch. Hatta and Marui (1918) showed that the diastatic power of the pancreatic juice was much reduced if the animal was fed on a fat diet. The earlier work of Benjamine, which was published in 1915 but apparently delayed in its translation till more recently, had shown that the method of administration made considerable difference in the results. This investigator found that when sucrose was given parenterally sucrase appeared in the blood, but this did not occur if the sucrose was given enterally. He also found that lactose feeding or injection caused lactase to appear in the juices of the pancreas and intestine, but not of the parotid. It is pointed out that no lactase was developed when lactose was added to blood, but occurred in organs after perfusion with lactose. The investigations of Lombroso (1915) brought out further evidence in favour of specificity. He found that the pancreatic juice could invert sucrose after the injection of sucrose intravenously or subcutaneously. In this connection it is interesting to note that Fleischmann and Meyers (1918) showed that in infancy animals were incapable of digesting

starch and cellulose and that malnutrition resulted if they were fed on these substances.

Much interest has also centred round the work of Sloan (1915), who, following up Abderhalden's investigations, showed that there was increased proteolytic action of the blood serum during pregnancy. This increase he ascribed in the main to an increase in the polyvalent ferments. The practical importance of this was urged by McCord (1913), who advocates the sero-diagnosis of pregnancy. It is assumed that the increase in proteolytic power of the blood of the pregnant animal is a protracted process against infiltration by chorionic villi.

It would be well perhaps to note that the specificity of the "Abderhalden reaction" has many strong opponents. Bronfenbrenner (1915), reviewing all the work on the subject, came to the conclusion that no new enzymes were formed, but that the anti-ferments of the normal blood were inhibited; while Opler (1916) considers that the apparent specificity is really due to errors in technique, and makes various suggestions as to where these may occur. He considers the position claimed by Abderhalden to be absolutely untenable. One might also mention the work of the latter observer in 1914. He found that by subjecting a mixture of amino-acids obtained from a definite organ to the action of a maceration juice of the same organ for several months, a decrease in the amount of amino-nitrogen and an increase of coagulable protein occurred. This he attributed to the specific action of the cellular enzymes of the tissue.

A somewhat similar specificity has been shown in the plant world by Fischer (1919). He found that in the legumins, especially those used for crops, there is a steadily increasing amount of proteoclastic and peptoclastic enzymes, which reaches a maximum at harvest-time. He has also shown that there is a distinct increase in these enzymes at the time of the germination of the seed.

A degree of specificity is also seen in the influence of certain substances in increasing the output of catalase from the liver, whereby general oxidation is facilitated. This will be dealt with under the heading of "Oxidation."

The enzymes of bacteria grown artificially appear to have a direct relation to the medium on which the bacteria are grown. Thus Diehl (1919) showed that in the absence of organic nitrogen bacteria develop no proteolytic enzymes. When organic nitrogen is added, the enzymes developed correspond to the amino-acids added to the culture medium and will attack these whether free or combined in the protein molecule.

Although the general opinion of investigators tends to

support the specificity of enzymes, there are many facts which as at present understood throw doubt on absolute specificity. Some consider that the work of Armstrong and Horton (1913) negatives specificity, as they found that a series of varied effects could be obtained from the action of emulsin on different glucosides. Similarly Fajans (1910) had shown that emulsin acts on α- and β-glucosides, but more rapidly on the β-glucoside, whereas maltase has the opposite effect. Some, on the other hand, show that enzymes may take part in reactions which could not occur in nature, and hold that there are cases of extremely rapid adaptation—which is unlikely—or of non-specificity of the enzymes concerned. For example, Kondo (1912) found that the liver could synthetise amino-acids from the keto-acids and ammonia, even if the keto-acids used were not those which occur in the body proteins. Working on fungi, Pringsheim (1910) found that they could act on both isomers of amino-acids, but at different rates; while similar work was done by Dox and Neidig (1912) with reference to the action of aspergillus on glucosides. Explanation, however, may be offered for these apparent anomalies. Thus some workers hold that emulsin is composed of several enzymes, and—as Bayliss suggests—it is possible that under certain circumstances enzymes may change the condition of an optically active body, as may be done by inorganic catalysts. The hypothesis put forward by the Armstrongs (1913) must here be noted. Reference will be made to it later, but for our present need it may be put briefly thus. It is supposed that the enzyme has a double function and is composed of two parts, viz. the *acceptor*, which is responsible for the adsorption, and the *agent*, which carries out the chemical change. The Armstrongs hold that the specificity of enzymes depends on the fact that the acceptor portion is closely allied to, if not identical with, a dominant group in the hydrolyte. Unfortunately it has not yet been possible to demonstrate what groups in the molecules concerned are responsible for the combination. Of interest too is the electron theory of Gallerani (1914), who put forward the suggestion that enzymes act somewhat like radioactive substances. He considered that the specificity of an enzyme action was connected with a peculiar syntony and synchronism which exists between the electrotonic intra-atomic motion of the agent and that of the element acted upon. Meantime, therefore, it may be considered that enzymes are specific towards certain groups of substances, but in the light of our limited knowledge of the exact nature of enzymes themselves, the trend of increasing evidence is towards absolute specificity.

Velocity.—As has already been said, enzymes are catalysts and, as such, manifest their existence by changing the rates at

which certain processes take place. Much work has been done on investigation of the factors which influence this velocity, especially with a view to determining the exact nature of the catalytic process. Repeated attempts have been made to formulate laws of velocity for the action of enzymes, but no general law has yet been agreed upon, the chief difficulty being that the heterogeneous systems in which the reactions take place so influence the velocity that it is seldom possible to be certain that the reaction in its entirety obeys any of the known laws of velocity, although it may in part appear to do so.

Concentration of Substrate.—Work on the effect of the concentration of the substrate shows that this factor plays little part in velocity, and that the rate is constant within a wide range of concentration. The early work of F. Armstrong on lactase, and that of Nelson and Vosburgh (1917) is quite definite, but that of Van Slyke and Cullen (1914) on the hydrolysis of urea by urease shows that there is distinct depression of enzymatic activity when the urea is over 10 per cent. It is interesting to note that Colin and Chaudin (1918) differentiated between two stages of enzyme action, as, it will be seen later, did also Van Slyke and Cullen in the work referred to above. In the first stage they found that hydrolysis of saccharose by sucrase was in proportion to the amount of enzyme present, but that the second stage was in proportion to the amount of substrate. In a later work, however, these observers arrived at what has otherwise been shown to be the probable cause of the decreased activity, viz. the increased viscosity, which apparently was not taken into account by Van Slyke and Cullen as no mention is made of it in their paper.

Visicosty.—Colin and Chaudin found that with increasing concentrations from 10 per cent. to 60 per cent. saccharose, the hydrolysis by sucrase diminished steadily. Below there was an upper limit to the activity of a given amount of sucrase in the presence of an excess of sugar. By means of a viscosimeter the retardation was shown to be due to increased viscosity, and the observers conclude that when the saccharose is in excess the hydrolysis is in proportion to the fluidity. A similar explanation can therefore be put on the earlier work of Bayliss (1913), who found that the synthesis of glycero-glucoside by emulsin was directly proportional to the concentration of the glucose. The same observer has shown that the action of trypsin on gelatine is less in the stronger solutions of gelatine; but even in 1919, although admitting of it as a possibility, he referred to it as an "obscure influence." What has already been said on the dispersion of enzymes in relation to their action, and on the effect on dispersion of substances which raise or lower surface tension, appears to make the point clear that the

viscosity diminishes the activity of enzymes by decreasing the dispersity.

Concentration of Enzyme.—The effect of the concentration of enzyme on the velocity of the reaction has already been mentioned with regard to the work of Colin and Chaudin (1918), but before that, it had been established that an increase in the amount of enzyme accelerated the rate of reaction, although there has been considerable divergence of opinion as to the law of velocity in which the acceleration may be expressed. Van Slyke (1914) found that the activity of urease depended directly on its concentration, and late in 1917 Nelson and Vosburgh came to the same conclusion regarding invertase. The latter however found that this was not necessarily so with very dilute solutions, and it was assumed that the lessened concentration was counterbalanced by the increased dispersion, while the spontaneous destruction of enzymes which tends to occur in dilute solutions adds an experimental difficulty. This destruction is very liable to occur when little substrate is present. In 1917 Biedermann confirmed earlier work by showing that the rate of the formation of dextrine from starch was inversely proportional to the amount of enzyme present.

Reference has already been made to the work of Bradley in this connection, and it is obvious that the velocity of a reaction will be retarded by such destruction.

Hydrogen Ion Concentration.—Recently more attention has been paid to the effect of the H˙ ion on the activity of enzymes. It has long been known—especially as the result of the work of Sorensen—that enzymes are very sensitive to changes in the H˙ ion concentration. Certain enzymes will act only within very narrow limits, some only in an acid medium, others only in an alkaline medium. Van Slyke and Zacharias (1914), working on the action of urease, came to the conclusion that the combining velocity of the urease with urea varied inversely with the positive H˙ ion concentration. In the estimation of the H˙ ion present, Falk and Nelson (1915) have shown that the method of estimation is of importance. They prove that it is not accurate to calculate it from the polarity of the acid, but that H˙ ion concentration must be measured either by the electromotive force or by the hydrolysis of sugar.

State of Substrate.—The H˙ ion concentration was shown by Ringer (1916) to be related to the condition of the substrate. This investigation showed that the optimum H˙ ion concentration for digestion is situated at the point of maximum swelling of the substrate ; he found at the same time that the effect of salts on the proteolytic activity of enzymes is parallel to the effect in reducing the swelling of albumin in acid solution. Ringer however found that the point of maximum swelling was

not necessarily at the same H˙ ion concentration for different acids, but depended on the nature of the anion. This has not been corroborated by Northrop, who in 1919 showed that at equal H˙ ion concentration of hydrochloric, nitric, sulphuric, oxalic, citric and phosphoric, the rate of digestion was the same, although for some unexplained reason acetic acid diminished the action. An interesting point in support of the importance of the state of a substrate is that of the digestibility of the elastic and white fibres of connective tissue. The white fibres are easily digested by pepsin, and it is noted that they readily swell if treated with acetic acid; but elastic fibres, on the other hand, are not affected by dilute acids such as acetic, and are only digested by pepsin after boiling for some time. Further facts in relation to this point are referred to under the heading of "Electrolytes," the action of which is more and more being shown to depend considerably on their effect on imbibition or swelling of the substrate, *i.e.* factors affecting the permeability of the substrate.

As evidence accumulates it appears increasingly probable that with a clearer conception of the physical processes at work, the importance of the H˙ ions and of their local concentration as a result of adsorption will be found to play a very large part in enzyme action.

Electrolytes.—In many cases the presence of an electrolyte appears to be essential for enzyme activity: indeed a particular electrolyte may be necessary. For example, pepsin is practically inactive without hydrion and trypsin without hydroxidion. All degrees of similar requirement of electrolytes exist. Thus ptyalin was shown by Bang (1911) to be almost inert in the absence of sodium chloride, while—as will be seen below—the activities of enzymes may be greatly modified by salts, even in dilute solution. An instance of the great sensitiveness of enzymes to electrolytes was given by Pavy and Baywaters (1911) who, when investigating the environment of enzymes—especially diastase and invertase—showed that acid hastened action while alkali neutralised, and that even the basic substances in tap water caused a marked retardation.

Much of the early literature deals with work the object of which has been to determine the relation of electric charges to the effect of the electrolytes and adsorption; while later fuller knowledge of colloid chemistry has directed attention to the effect of electrolytes on colloids—that is, either precipitation or an effect on the dispersion of the colloid; in which connection Bayliss had suggested that the phenomenon of an optimal H˙ ion concentration for each enzyme may be connected with the maximum state of dispersion of the enzyme.

Of interest is the attitude of the Armstrongs (1913) with regard to the action of electrolytes. These investigators stated that action could never take place between non-electrolytes, and it was therefore assumed that the electrolytes themselves acted by bringing about the inclusion of the interacting substances H or OH in the electric circuit, and that the changes themselves take place as soon as the circuit is established. Such an assumption is included in the hypothesis put forward by them that enzymes have a double function, that of attracting or holding the electrolyte in adsorption, and that of determining its hydrolysis ; in other words, that the enzyme retains the hydrolyte in the circuit while hydrolysis is effected through the agency of an electrolyte itself possibly formed from the active radicle of the enzyme. The full hypothesis, which includes a theory as to the constitution of enzymes, is too long to deal with here.

Palladin (1916) reached a conclusion similar to that of the Armstrongs : he found that non-electrolytes stop the action of the proteoclastic enzymes in plants, and suggested that the action was due to a change in the electrical conductivity of the system. He also suggested, however, that the action might be due to a change in the hydration of the proteins ; and it is along this line that recent work on the subject tends.

Reference has already been made when dealing with $H^{\cdot}$ ion concentration to the work of Ringer (1916), who drew attention to the relationship between the action of salt and the swelling of albumin.

Some explanation for the swelling of protein was put forward by Pauli in 1912. He considered that electrolytically dissociated salts of protein are formed by acid and alkali, and that the swelling is due to the affinity of water for the protein ion. It is supposed that the effects of salts in removing water is exerted rather on the water itself than on the protein. Pauli also pointed out evidence that a surface action is in question, in that the viscosity of protein solutions is always lowered by the addition of a salt. The actual process may, as stated by Posnyak (1912), be due to a simple solution of the liquid in the substance of the particles, which thereby change their shape and size, to a filling-up of capillary spaces between the particles of the protein as a result of condensation of water on them, or to a combination of both processes. Zigismondy (1913) considered the second process to be more important.

The possibility of a change in the concentration of the $H^{\cdot}$ ion has also to be considered. Falk and Nelson (1914) showed that the addition of sodium chloride to a solution of hydrochloric acid caused an increase in the $H^{\cdot}$ ion concentration, as estimated

by the electromotive force and the hydrolysis of sugar. A relationship between the $H^{\cdot}$ ion concentration and the state of the substrate has already been referred to.

Groll (1918), on the other hand, after showing that cations retarded in a definite order—$NH_4>K>Na>So>Ba$—came to the conclusion that the action was similar to floccing[1] and other colloidal phenomena, and considered it possible that the influence of neutral salts is due to the fact that the dispersity of the enzyme is modified by the ions in the same way as other colloids. The similar suggestion put forward by Bayliss earlier has already been referred to.

It appears likely that amino-acids—which will be seen later, under "Effect of Products," to assist the action of some enzymes —act by an amphoteric electrolytic dissociation, *i.e.* they dissociate with the formation of $H^{\cdot}$ and $OH^{\cdot}$ ions, and are capable of acting either as acids or alkalies.

The effects of different salts are more difficult to explain, as there is added the complication that protoplasmic activity may necessitate an antagonistic action of salts for its efficient action. Such a necessity is seen in the influence of sodium, calcium, and potassium on the heart. Falk (1918) found that weak solutions of sodium chloride might slightly stimulate lipase but calcium always inhibited its action, and the effect of the latter salt could always be counterbalanced by sodium chloride. He noted also that this antagonism was of a definite stoichiometrical order. In plant life Robbins (1916) had shown that the addition of chlorides and sulphates of alkalies and alkaline earths diminished the amount of starch digested by certain fungi, while nitrates caused an increase. Salts have also an influence on the products formed in a reaction. Koslechev and Frey (1916) showed that zinc chloride causes the production of acetic aldehyde in hefanol and dry yeast, but not in living yeast, and only in the presence of sugar, while the quantity of CO_2 produced exceeds that of alcohol. Subsequently in conjunction with Zabkova he showed that this fact depended on the zinc ion. This appeared to be specific as it was not given by other metals. Similarly in the fermentation of sugar, mannite, and glycerine by bacteria, Neuburg and Nord (1919) showed that in the presence of sodium or calcium sulphite acetaldehyde was formed; or again, only certain functions of the enzyme may be influenced as in the case of calcium or the starch-liquefying enzyme of aspergillus oryzaie. In this, Kita (1918) found that calcium accelerated the liquefying action of the enzyme but not the saccharising action. As there are so many factors involved in each individual case, the problem is a very difficult one, and much of the tangle of

[1] Called "flaking" by Groll.

inco-ordinated facts and theories which exists at present must be left for the future to unravel.

Temperature.—The effect of temperature in increasing the velocity of enzyme action is most constant, and is even more marked than the effect of heat in chemical reactions. In 1910 Mendel and Blood brought forward a most striking instance of the effect of heat. They found that papain at 80° C. digests almost three times as much excelsin in fifteen minutes as it does in seventeen hours at 44° C. In explanation of this phenomenon the work of Caesana (1913) has already been referred to under "Dispersion." It is also to be noted that the work of Euler and Beth of Ugglas (1910) showed that the temperature coefficient of inversion by invertase and maltase is higher than that of inversion by acids.

Although enzymes show an optimum temperature, the majority are destroyed by extreme heat—*e.g.* boiling—and it has been suggested that the destruction is due to action on the complex colloidal system. When enzyme is adsorbed, however, it is not so easily destroyed. This was noted by Bayliss (1911), who investigated the point, using trypsin and charcoal. Similar protection has been shown in regard to invertase and cane sugar. It may here be noted that when the temperature coefficient of urease on urea was worked out by Van Slyke and Cullen, they found it to be that of a chemical reaction rather than that of a physical combination.

The work of Albert and Alexandre Mary (1918) has been referred to already. These investigators, it will be remembered, concluded that the temperature inactivation of natural enzymes was undoubtedly due to a degradation of their dispersed phase. This increase of enzyme action by temperature gives a simple explanation of the effect of heat on muscle activity, and Moore (1918), working with the embryo heart of Fundulus, shows that the rate of heart beat due to heat follows a similar curve to that of the velocity of an enzyme reaction as affected by temperature. The recent work on the physiology of catalase and its importance in the activity of the body mechanisms would tend to support this explanation.

Effect of Products.—As is common to so many processes in nature, the products of reaction may have an effect on the velocity with which the reaction proceeds. In the case of enzymes in nature, however, the products of the reaction are absorbed or are passed on to another part of the body. This interference therefore is more clearly seen *in vitro*, and is distinctly observed when the products have an opposite reaction to that of the medium in which the enzyme exercises its effect. Thus Van Slyke (1914) shows that urease is inhibited by ammonia, a product of its decomposition of urea. Much of the

work on this subject has been done prior to the period under review; but Bayliss in 1919 pointcd out that in dilute alkaline solution the digestion of caseinogen by trypsin is slowed down as a result of production of amino-acids which reduce the hydroxidions in the solution. Also it is shown that when an ester is hydrolysed by water the reaction becomes greatly increased in velocity as the acid formed in the reaction accumulates. Such results closely approximate to the phenomenon to which Ostwald applied the term *autocatalysis*, but usually this can be better applied in the case of enzymes to instances in which the products of one reaction hasten the velocity of another. Rockwood (1916) found that amino-acids caused a marked increase in the formation of reducing sugars by amylase tested in salivary and pancreatic juice. In a continuation of the work (1917) he showed that glycine and protein also increase the amylolytic activity of amylase. Partially hydrolysed proteins also have an accelerating influence. He concluded this was due to the amino radicle, not being exhibited by the salts of the acids from which the amino-acids are derived nor by acid amides. He showed also that at least one of the hydrogen atoms could be replaced, but that the introduction of a sulphanitic radicle neutralised the effect. A similar acceleration by certain amino-acids, such as glycine, glutamic acid and alanine, on the activity of various amylases was also observed by Ujihara (1917), who found that the acceleration was often as much as 45 per cent. to 100 per cent.

Mathews (1915) claimed that maltose and dextrose had an inhibiting effect on ptyalin; but this was not confirmed by the work of McGuigan (1919) already alluded to.

Mention has also been made of the suggestion of Bancroft (1918) that products more highly adsorbed than the substrate might dispossess the latter from surface.

In some cases the products of the reaction precipitate the enzyme from the solution, and it seems probable that this is not due to any special combination between the enzyme and product, but rather to some alteration of the colloidal state of the enzyme or of the colloidal system with which the enzyme is associated.

Equilibrium.—As a rule, however, the reaction proceeds till a state of equilibium is reached between products and substrate, and the reaction stops, unless the products are removed by absorption, as in nature or by dialysis.

The phenomenon of equilibrium has been demonstrated by many observers prior to 1910. Bourquelot and Bridel (1913) showed that a true equilibrium existed in the case of β-ethyl glucoside and emulsin. This work was supported by that of Bailly (1917), who demonstrated that synthesis by emulsin of

methyl alcohol and glucose giving methyl glucoside and water was reversible. Similar work has been done by Bayliss in the case of the hydrolysis of glycerol-glucoside. The latter observer has also confirmed the fact that the concentration of enzyme has no effect on position of equilibrium, which, as already stated, shows that the enzyme plays no part in the system in equilibrium. The importance of the amount of water in determining the point of equilibrium has been shown by the work of Armstrong and Gosney (1914) on the action of lipase on glycerol, oleic acid and olein. These workers showed that the greater the concentration of water the nearer the point of equilibrium was to complete hydrolysis.

The work of the investigators just mentioned also shows that the same point of equilibrium is reached whatever substances they consider the substrate or products. Thus in the equilibrium obtained under the action of emulsin, they show that the same final condition is reached whether they begin with glyceroglucoside or glycerol and glucose, *i.e.* the action is reversible. The existence of such reversible reactions is known in nature; and indeed in Tizard's translation of Nerst's *Theoretical Chemistry*, the general view is held that all reactions could be shown to be reversible if observers could be sufficiently skilful to demonstrate this. The reversibility of a hydrolytic reaction in itself proves the existence also of synthesis by enzymes, which leads us to a consideration of the various types of reaction which are catalysed by enzymes.

Reactions Catalysed.—Having reviewed the work relating to the velocity of enzyme action and the factors influencing it, we shall now turn to the consideration of various types of reactions which are catalysed by enzymes.

Hydrolysis.—The hydrolytic power of enzymes was the first to be discovered, and it is but to be expected that most of the work on the subject should have been done in relation to hydrolysis, more especially as it has an important practical bearing on the physiology of digestion and assimilation. Practically all the work hitherto cited has referred to hydrolysis; indeed by the word enzymes one has almost entirely understood *hydrolytic* enzymes.

Enzymes are, however, concerned in many other processes, but for the most part work on these is in an early stage and has been practically confined to determination of the fact that they do take place. Some of these other reactions are considered below.

Synthesis.—It has been found—as shown under "Reversibility"—that many hydrolytic enzyme actions were reversible, and investigators were thus led to inquire into the synthetic action of enzymes, since the existence of reversibility in a

hydrolytic reaction may be considered to necessitate a synthesis.

On further investigation, the hydrolytic and synthetic properties of enzymes are found to be intimately bound up together, indeed they may be considered to proceed synchronously in a system, and the equilibrium position may be held to show the rates of the velocity of the hydrolytic to that of the synthetic process.

For convenience it is perhaps best to group the work done on this subject under the types of substances which are synthetised.

Fats.—Most of the work done to investigate the synthesis of enzymes has been done on fats or fatty acids, as they afford better material for study than carbohydrates or proteins. They are simpler in that they do not form steriosomers and may be used in more concentrated solution.

Bradley (1913) concluded from his investigation of fats and lipases in the various tissues of the body that it was very doubtful whether fat was synthetised by enzymes at all; he found, for example, that the brain contains more lipase than the mammary gland. Such a result is difficult to explain; but in confirmation of work done prior to the period under review, Armstrong and Gosney (1914)—as already stated—showed conclusively that lipase does synthetise fats. Further, Hamsik in the same year found that lipase from the intestinal mucous membrane, from the lung, and from the liver could synthetise.

Proteins.—Although it has been realised that a synthesis of proteins necessarily takes place in the building up of tissue, comparatively little work has been done on synthesis by enzymes. This is owing to the difficulty arising from the number of products into which proteins readily break down when hydrolysed. Moreover, since the point of equilibrium in most instances of hydrolysis of protein is close to that of complete hydrolysis, it is obvious that the synthesis can only take place to a very small extent; and the fact that the enzyme, if not itself a protein, is usually closely associated with protein matter, is an additional difficulty.

The work of Henriques and Gjaldlec (1911) established the fact, however, that protein could be synthetised by enzymes. These investigators found that the nearer the substances used were to the products of complete protein hydrolysis the simpler was the substance formed. In this connection it is interesting to note that earlier observers found that the body was quite unable to maintain adequate nitrogen content on food consisting of the products of hydrolysis of protein by acid, whereas it flourished on the products of digestion by enzyme.

Carbohydrates.—Since the earlier work of Croft Hill and

Van Hoft, the investigations of Bourquelot and his collaborators, Bridel and Aubrey, have been the most fruitful. Bourquelot was able (1911–13) to prepare a set of β-glucosides from the corresponding primary alcohols with the aid of emulsin ; and in 1913 Bayliss confirmed most of this work. Later Bourquelot and Bridel synthetised α-glucosides from the primary alcohols with the aid of maltase of yeast. In 1916 Bourquelot and Aubrey showed that a galactobiose could be synthetised by the prolonged action of emulsin in a saturated solution of galactose. The biose was subsequently purified by boiling the emulsin and fermenting off the uncondensed galactose by yeast in the presence of glucose. In 1917 the same investigators synthetised galactose and found that the sugar so prepared had the usual reducing power and was optically active. Bourquelot and Bridel (1919) showed in the synthesis by emulsin that the products formed depend on the relative amounts of the substances used. They found that the emulsin of almonds tends to form only β-glucosides when the alcohol is in excess, while if the glucose is in excess the tendency is rather to the formation of polysaccharides.

The work cited in the foregoing remarks would almost lead one to presume that enzymes which hydrolyse also synthetise. No mention need be made of any investigations other than those of Bayliss (1913) and Krieble (1915) : they were unable to confirm the results of the earlier observer, Rosenthal, who claimed to have discovered an enzyme which would hydrolyse but would not synthetise.

Oxidation.—A great deal of our present knowledge of the processes concerned in oxidation is based on the work of Bach, who in his researches in conjunction with Chodat was the first to rescue the subject from confusion. Bach's conclusions have been generally accepted, but the importance of the subject has become increasingly recognised, and the realisation that in it may lie the key to many of the problems—pathological as well as physiological—in the nutrition of animals and plants has induced many observers to take up this subject within recent years.

Oxidase.—The subsequent work of Bach, published in several papers in 1913, is of special importance. It is presumed that an organic peroxide is formed by some auto-oxidisable substance in the cells of the tissues, and that oxidation takes place through the freeing of active oxygen by the enzyme *peroxidase*, which was discovered by Bach in his earlier work. In 1914 Bach gave support to his previous observations by showing the part played by hydrogen peroxide in the reaction. He observed that fresh potato juice rapidly oxidised tyrosine, but that it lost this property if a certain substance or substances be

precipitated from the juice by alcohol. This property, however, returned on the addition of hydrogen peroxide ; showing that the substance precipitated and rendered inactive could be replaced by hydrogen peroxide. Bach showed also that if the taking-up of oxygen by an auto-oxidisable substance—*i.e.* the formation of a peroxide—was prevented (by passing hydrogen in coal gas through the system), no oxidation of guiacum by peroxidase would take place. The oxidation of the guiacum took place, however, if air was admitted or hydrogen peroxide added.

The earlier observations of Yoskida had shown that the production of lacquer varnish from the sap of the lac tree was dependent on an enzyme which brought about oxidation, and which he called *laccase.* This was the first vegetable oxidising enzyme to be discovered. It had been noticed previously that salts of manganese, iron or copper acted somewhat like peroxidase on the decomposition of hydrogen peroxide. Following up this point, Bertram showed that the properties of laccase depended to a great extent on the presence of manganese in its constitution. Later Dony-Hénault was able to prepare an artificial laccase.

More recently artificial oxidases having more general properties have been prepared. Dimichelis (1917) found that certain metallic colloidal compounds exerted catalytic action, and he prepared an artificial oxidase having the chemical constitution $Cu_2.Fe.$ (Cu_6). The term *oxidase* in this connection is loosely used, as more generally it is reserved for the system containing a peroxide and peroxidase.

An artificial oxidase system was elaborated by Reed (1916) as a result of his work on the charging of colloidal platinum with oxygen. This investigator showed that the peroxidase extracted from potato could be charged with oxygen by potassium permanganate. It could then oxidise iodic acid till the charge was exhausted. He concluded therefore that an oxidase was a substance of high oxidation potential formed by charging a peroxidase previously with oxygen or by associating it with a peroxide.

Every year brings further evidence as to the part played by peroxidase in the life of organic matter. Graham (1916) demonstrated by means of naphthol and H_2O_2 that there exists in the granules of leucocytes and myelocytes a peroxidase of the peroxide type. The importance of this in the destruction of toxic agents in the blood is obvious.

In the plant world, loss of toxicity due to enzyme oxidation was instanced by McNair (1917). He found that the exuding sap of Rhus diversibola rapidly became oxidised by the air and lost its toxic properties, while subsequently he extracted

an enzyme which accelerated the oxidation of the phenols. In support of its being a true enzyme, he found that it was destroyed by boiling, was inhibited by acids and rendered more active by alkalies.

Catalase.—Of recent years much attention has been given to the study of catalase, an enzyme which is capable of decomposing hydrogen peroxide with the evolution of molecular oxygen as a gas, but which does not cause oxidation as the oxygen evolved is inactive.

According to Bach (1913) it plays an important rôle in protecting sensitive parts of the cell mechanism from the action of easily diffusible hydrogen peroxide found in the oxidation processes. Bach also states that in a mixture of hydrogen peroxide with both catalase and peroxidase, the peroxide is decomposed partly into active oxygen, and partly into molecular oxygen, according to the relative amount of the two enzymes present. It would appear, therefore, that catalase may act as a regulator of the oxidation process. Here it may be noted that the earlier observer, Loew, found the catalase from tobacco leaves and from the poppy seed were able to oxidise hydroquinone to quinine, but gave no other oxidase reactions.

Other than Bach, no author is apparently willing to commit himself in any further speculation as to the function of catalase. There is, however, increasing evidence as to its importance, since it is so closely related in its distribution and amount to certain activities of the body: indeed it seems very probable that it will be proved to be one of the most important factors in the body mechanism.

It has been stated by Vernon that starvation of rats or frogs did not influence the amount of catalase found in the tissues, although Jolles and Oppenheim found that the catalytic power of human blood decreased in certain diseases, *e.g.* carcinoma, tuberculosis and nephritis. In 1917 the work of Burge and Neill did not confirm that of Vernon, but tended to substantiate that of Jolles and Oppenheim. Burge and Neill found that during starvation catalase was much decreased in the voluntary muscles and adipose tissue (except the heart muscle, where the amount was normal). And it is suggested that the decrease of catalase in these tissues leaves the autolytic enzymes free to digest them.

On the same lines is the conclusion of Megath (1917) regarding the protection of intestinal worms. He found that there were considerable amounts of catalase in the body-wall of intestinal worms, and concluded that the worms were protected from the intestinal enzymes by nascent oxygen produced by the catalase. This gives support to the theory originally put for-

ward by Burge (1914) with reference to the protection of the mucous membrane of the stomach and intestine. The mucous membranes of the intestinal tract possess intensive oxidation processes, and in virtue of the nascent oxygen which is known to destroy pepsin and trypsin, the autolytic enzymes are held in check during life. The same observer in 1917 found that in animals fed on thyroid there was an increase in catalase of the blood, of the skeletal muscles, and the heart, and consequently an increase in body oxidation processes. There was, however, a decrease in catalase of other organs of the body, and in these an increased autolysis was noted. There is, then, in the work mentioned above considerable evidence regarding the regulation of enzyme activity and the protection of the organism by catalase.

Catalase is now shown to bear a direct relation to food. Burge, Neill and Ashman (1916) exposed the livers of dogs, took samples of blood issuing from the liver, and compared it with blood from other veins. They found that catalase was increased by the ingestion of starchy foods, more so by the ingestion of sugars, while an even greater increase was caused by food products, especially protein food. An increase was also caused by the fats of olive oil and bacon. Coffee, cocoa, tea and milk, however, did not make any appreciable difference, but chocolate caused a definite increase. Ripe fruit caused an increase—presumably due to its sugar—while unripe fruit did not.

These observers also claimed to have obtained an increase of catalase by stimulation of the splanchnic nerves. They found also that in severe fatigue the catalase of the tissues was used up, and suggested that herein lay the cause of the non-oxidation of lactic acid which is known to accumulate in such cases. Burge (1916) showed that the amount of catalase varied directly according to the amount of work done, and that warm-blooded animals, in whose bodies more oxidation took place, had more than cold-blooded animals. This latter fact confirmed the work of earlier observers. W. E. Burge and E. L. Burge (1917) showed that there was an increase of catalase in the liver of cats which had been subjected to fright immediately before death.

Some of these results were confirmed by Burnet (1918), who showed that accelerated muscle activity was due to an intense secretion from the liver, which could be stimulated by food, especially protein. He noted also that the extirpated liver liberated for two hours catalase, presumably due to dextrin formed from the glycogen.

The relationship between catalase and the production of shock was brought to notice by Burge and Neill in 1918. They

found that in shock due to loss of blood the tissue fluids which replaced the plasma were poor in catalase, and oxidation processes were consequently much reduced. They concluded from this that the beneficial effect of alcohol in such cases was due to stimulation of the liver catalase.

These facts and reference to the stimulation of the splanchnic nerves—which shows that the excretion of catalase is under the control of the sympathetic nervous system—suggest that a catalase deficiency may play a considerable part in the fall of temperature which takes place in cases of surgical shock. Indeed the recent war has shown that the fall of temperature is just as important as the fall of blood-pressure—upon which so much stress has been laid by Crile and Mummery. In this connection it may be mentioned that although the keeping warm of patients suffering from shock has for many years been considered a point of importance in the treatment of such cases, experience in war has caused great emphasis to be laid on it by modern surgeons, and this factor was very seriously considered in the arrangements for the evacuation of wounded. As the heat of the body varies with oxidation processes, which from the work reviewed above appear to depend to a great extent on catalase, it is possible that the production of shock in cases of severe injury may partially at least be dependent on an inhibition of catalase secretion through the sympathetic nervous system. Unfortunately there is no record of the catalase content of the blood having been determined in such a case of shock, but the decrease in catalase in shock due to loss of blood would tend to support this hypothesis.

Kennedy and Burge (1917) found that the livers of pancreatised dogs were deficient in catalase, and suggested that the lack of oxidation in diabetes might be due to such a catalase deficiency. The discussion of an enzyme theory of causation of diabetes is too long to be entered into here.

Although the careful work of Burge is very attractive, it is nevertheless necessary to add that his results regarding increase of catalase due to rejection of meats and protein food products have not been confirmed by Stehl (1919), who considered the fluctuations to be due to the red blood corpuscles. Bodansky has also attempted to check some of Burge's results, and comes to the conclusion that no satisfactory method for the estimation of catalase exists. Very slight changes in temperature, impurities, and especially the reaction of the hydrogen peroxide affect the result. He found that even with the preparation used by Burge, results may differ as much as from 15 per cent. to 30 per cent.

Burge (1918) showed that catalase is destroyed by anæsthetics, and suggests that anæsthesia may be so produced. Its

importance in plant life is seen in the chlorophyll function; while it was noted by Reed (1916) that in the ripening of pineapple fruit it increased, but the amount of oxidase remained constant.

Whatever, therefore, be the exact function of catalase in the body, it obviously plays a large part in animal metabolism, as it does in plants. It may not bring about the actual oxidation, but there is increasing evidence that it is in some way responsible for it.

Reductase.—According to the more modern view, the power of fresh animal and plant tissues to reduce nitrates to nitrites is now explained by Bach's hydrolytic oxidative-reducing reaction. Bach's view (1913) is that the water of the system is broken up so that an aldehyde is oxidised and hydrogen forms a perhydride, which, through the influence of a perhydrase, brings about the reduction. The so-called *reductase* is therefore a perhydridase plus a perhydride which is formed from water as a result of the action of an aldehyde. Of such a nature is the reduction of milk by methylene blue. One observer, Woker, had earlier put forward the suggestion that all oxidation enzymes were merely different manifestations of an aldehyde, but this claim is strongly refuted by Bach (1915).

Bach (1913) showed the presence of a perhydridase in potato juice, which in the presence of an aldehyde reduced nitrates. He noted that the perhydridase utilised indifferently various aldehydes. Apparently independently Abelous and Alloy (1918) confirmed somewhat the results of Bach. They described a ferment present in milk and vegetable secretions which can reduce nitrates and chlorates in the presence of an oxidisable substance such as salicylaldehyde. This ferment, they found, decomposed water, with the liberation of H· and OH· oin, and then had a reducing and oxidising power. This ferment, which they called an oxyhydrase, they suggested is a factor in the antitoxic defence of the organism and is adapted to the anaerobic life of the cells.

Alleman (1918) showed that the perhydrase apparently followed the usual laws of enzyme action. He found that the rate at which milk decolorised methylene blue was influenced by the acidity of the milk which caused retardation, while the disappearance of colour was hastened by temperature up to 55° C. and retarded at 75° C.

Other Reactions.—Apart from the processes already stated to be catalysed by enzymes, there are others in the animal and vegetable world which apparently depend on enzymes.

It has been shown that the chlorophyll in the leaves of plants is associated with an enzyme which is active in alcoholic solution. This enzyme—which is like a lipase or an esterase in

nature—appears to act synthetically and forms chlorophyll in a concentrated solution of phytol and chlorophyllic acid.

There are those who hold, as did Schweirzer (1916), that deamination is dependent on an enzyme like tyrosine which has oxidising power.

Whitby (1918) showed that the coagulation of rubber is due to enzyme action. He showed that an enzyme is responsible for the coagulation of latex and is the real cause of coagulation when acid is added to latex.

Coagulation of Blood.—It is still customary to refer to the coagulation of blood in relation to enzyme action—as older investigators described thrombin—as a *fibrin ferment.* This so-called ferment has, however, been shown, especially by Rettger, working in the laboratory of Howell (whose work on this subject is a classic), not to possess the usual properties of an enzyme. For example, it is not destroyed by boiling, the velocity of its reaction is not affected by temperature, and to some extent it is used up in the reaction.

The more modern view is that coagulation is an interaction between colloids under the influence of electrolytes, especially calcium salts.

Dale and Walpole (1916) brought forward another possible enzyme factor in coagulation, but of a different nature from that of the earlier observers. They found that fresh fowl's plasma treated with chloroform gave a powerful thrombin, and that a similar production of thrombin occurred if the plasma was treated with trypsin. They therefore suggested that the chloroform acted by destroying the antitrypsin and thereby allowing the normal tryptic enzyme of the plasma to work. Howell had considered that thrombokinase inactivated antithrombin, which normally prevented activation of prothrombin by calcium. But Dale and Walpole do not agree with Howell, as they found kinase, prothrombin and calcium to be the three essential factors in the formation of thrombin.

Howell had noted, as had also Dale and Walpole, that more active kinase could be obtained from a tissue undergoing autolysis. The latter observers explain this by suggesting that a certain degree of protein cleavage is necessary for the liberation of kinase from any tissue. This hypothesis, however, awaits confirmation, and the physico-chemical explanation of blood coagulation has most support at present.

Adjuvants.—It has been noted already that certain substances assist the action of enzymes, and reference to a considerable amount of the work on this subject has been made under " Electrolytes " and " Effect of Products." In extreme instances, such as that put forward by Bang with reference to ptyalin, the enzyme is practically inactive without sodium

chloride. In such an instance the electrolyte must be considered as a co-enzyme, just as the $H^{\cdot}$ ion may be with regard to pepsin. The term *co-enzyme* is in this connection one of degree; but in its original sense as used by Bertram when describing the importance of manganese in the action of laccase it was more confined. The term *adjuvants* has therefore been adopted to cover all substances which accelerate enzyme action, whatever their nature.

There is not much recent work on this subject, and it seems very probable that many of the substances hitherto known as co-enzymes will be found to affect different parts of the complex systems in which enzymes act, and the term will become obsolete.

Vulquan and Lisbonne (1912) found that the amylase of malt was inactive without the presence of sodium, potassium or calcium chloride, or of potassium nitrate. Factors on which this may depend have been mentioned under "Electrolytes."

Similarly early workers, *e.g.* Loewenhart, referred to bile salts as co-enzymes, but as already stated, the action of the cholates—as shown by Terroine and later by De Jonge (1917) (see "Dispersion")—can now more readily be explained physically.

The loss of activity of urease—shown by Falk in 1917—as a result of dialysis can readily be explained physically, but it is not quite clear why the urease can be reactivated on the addition of a very small quantity of fresh urease.

Neuberg (1918) refers to keto acids and potassium phosphate as co-enzymes in relation to fermentation by yeast, but in this instance too a physical explanation could be found.

Some observers have found that certain enzymes help others. For instance, Massai (1918) found that the action of pancreatic trypsin on casein was supplemented by erepsin from animals or yeast. It is possible that the adjuvant action of these two enzymes plays an important part in the completion of digestive processes.

Of unique interest is the observation of Lombroso (1916) with reference to the adjuvants of the glycolytic enzymes of the pancreas. This investigator found that the blood corpuscles were a most important factor in the effectivity of this enzyme. As presumably the effect of the blood corpuscles is purely physical, general support is given to physical explanation of adjuvant phenomena.

Antagonists.—When dealing with the action of electrolytes, reference was made to the inhibiting effect of salts, and to this point many of the general statements made regarding "Adjuvants" apply. Just as it is possible that certain of the properties of the so-called co-enzymes may be explainable

physically or chemically, so also a physico-chemical explanation may be looked for in regard to anti-enzymes.

There has been much controversy on the subject of anti-enzymes, and the elucidation of their nature has not been made any easier by authors ignoring published work of other observers holding a different view, or referring compromisingly to "so-called anti-enzymes."

The greatest advocate of the non-existence of anti-enzymes as definite entities is Bayliss (1919), who bases his argument on his experience when attempting to verify the early work of Hildebrandt, who had claimed to have found an anti-emulsin.

Bayliss (1913) showed that the anti-emulsin effect was really due to change in the concentration of the H˙ ion of the system. He points out also that in the experiments of Ohta (1913)—who likewise claimed to have discovered an anti-emulsin—the latter had not taken into consideration possible changes in the H˙ ion concentration.

Taking as an analogy the inactivation of rennet by shaking —a phenomenon probably due to surface coagulation—Bayliss notes too that substances capable of adsorbing enzymes may reduce their action by causing a reduction in their concentration. In this he is supported by the fact that anti-enzymes are related to the albumin factor of proteins and not to the globulin factor, as is the case with antibodies formed as a result of the injection of bacteria or other toxins.

The work of Jahnson-Blohm (1912) gives support to the above suggestion. This investigator showed that anti-enzyme action could be prevented by a more strongly adsorbed substance. Such a substance, *e.g.* saponin, unites with that body which otherwise would adsorb the enzyme.

Several observers other than Bayliss have failed to obtain increased anti-ferment action in serum on the injection of foreign protein, while some, *e.g.* Visco (1915), have been unable to demonstrate anti-enzyme (anti-trypsin) action in normal blood, if other factors were excluded.

Dezani (1916) was unable to find antipepsin in the blood, and he points out that inactivation of pepsin (in vitro) is due to the alkalinity of the serum, and that if hydrochloric acid be added retardation of action may still be caused by a deficiency of free acid.

Young (1918) could not find increased anti-tryptic action in the blood serum after the injection of trypsin, although formation of the usual precipitins for the proteins injected was noted. Linossier (1916) observed that the already great antipeptic properties of horse serum are increased by heating; further he found that egg-white and gelatine are antipeptic,

and that this property is increased by heat. He concludes therefore that the antipeptic action of normal serum is not specific but is due to a mutual reaction between the colloids of that fluid and the enzyme. Rubenstein (1916) confirms these results and points out that the serum after being heated neutralises the hydrochloric acid necessary for the pepsin.

The term "*anti-enzyme*" has, however, become established, and many writers still use it as if anti-enzyme was a definite entity. From what has been said above there is evidence that such an entity does not exist.

It is probably more correct, therefore, to refer to the *anti-enzyme factor* of the blood or tissues, using the term to include any processes or substances which inhibit or retard the action of enzymes. Under this heading would come a substance such as catalase, or mere physico-chemical properties.

The existence of anti-enzyme factors has been shown by several investigators, but the specificity of the factor is very doubtful.

Hedin (1912) found that if rennet or its zymogen is injected, an anti-rennet factor is produced; but he points out that the anti-rennet factor is not a true antibody in Ehrlich's sense of the term.

Yamakama (1918) describes an anti-enzyme in the serum which is destroyed by chemical reagents such as chloroform, or saturated monovalent ketones. Yamakama goes on to show that the autolysis of normal serum when alcohol is added is probably due to a similar destructive effect on the anti-enzymes.

Mention has been made already of the work of Bronfenbrenner, who, when criticising the "Abderhalden reaction," came to the conclusion that during pregnancy certain anti-enzymes were inhibited rather than that new enzymes were formed.

Other authors similarly neglect the nature of the anti-enzyme factor, but deal with its application. Weil (1916) concludes from his work that the anti-tryptic reaction of the blood may not be specific for any special diseases, but is a valuable adjunct in differential diagnosis, since it is found in some diseases and not in others.

Much of the work done on anti-enzymes bears a strong resemblance to that of Burge referred to under "Catalase," and if the results of the latter investigator are confirmed a solution will be found as to the nature of many of the "so-called anti-enzymes."

Perhaps the various anti-enzyme factors which have been suggested as the protective mechanism of the intestinal tract give as good an idea as any of the processes or substances which may be at work, although actually the statement of Hunter

that the protection is due to a " vital strength " is as nearly accurate as many modern ideas.

Early observers, *e.g.* Claude Bernard, considered that the protection was a mechanical one and was due to the rapid shedding of the superficial epithelium and to the mucus which was being constantly poured out. Weinland and others describe anti-ferments. Klug has suggested that the mucin forms an adsorption compound with the enzyme. And Bayliss extends the idea to food which may also adsorb the enzyme. Burge—as already stated—considers that oxygen produced from the catalase which is specially plentiful in the wall of the stomach and intestine is the protective factor.

The work and conclusions of Bolton (1913) in relation to ulcers of the stomach are very interesting on this point. He showed that ulcer formation was in no way dependent on the neutralisation of an anti-pepsin; while Wolff in the same connection found that the stomachs of patients who had suffered from gastric ulcer contained no less anti-pepsin than normal stomachs.

It is clear therefore that more work must be done before the exact nature of the anti-enzyme factor can be decided.

Conclusion.—From this review of the recent work on enzyme action, it will be seen that advance in knowledge of the subject has been slow, and has involved much careful and tedious work. An attempt has been made to show the stages which have been reached in the various branches of the subject, and at the same time to indicate, without undue formation of hypotheses, the lines along which recent work would lead us to expect that solutions of the many problems may be found.

In the first instance, the value of a more exact knowledge of the actual nature of enzymes is noted, although the subject of the essay does not admit of it being dealt with in detail.

The factors governing the velocity of enzyme action are shown more and more to be capable of a physical explanation, so much so that some have suggested that the whole of enzyme action (even the nature of enzymes themselves) may be so explained. The work of Beatty (1916), although hypothetical, is very interesting on this point. He puts forward a theory of enzyme action which reduces it to the simplest possible explanation, viz. that all the reactions concerned are essentially the addition or splitting-off of $H^{\cdot}$ or $OH^{\cdot}$ ions. The theory is an advanced one, but certainly modern evidence tends in that direction.

The work on the synthetic action of enzymes has established definitely that synthesis does occur, but much remains to be done on the factors governing the reaction.

Our knowledge of oxidation and other processes is seen to

be in its infancy, and to consist to a very large extent of an accumulation of incoherent facts which are the result of purely abstract laboratory work. Without doubt, however, the practical value of these results will eventually come to light, and well it is to remember the passage from Spratt's *History of the Royal Society*, which supports the existence of a society for the purpose of making experiments: "If they will persist in condemning all experiments except those which bring with them immediate gain and a present harvest, they may as well cavil at the providence of God that He has not made all seasons of the year to be times of mowing, reaping and vintage."

Some authors, *e.g.* Trolland (1917), consider that enzyme action and specific catalysis provide a definite general solution to all fundamental biological problems, and deplore the recrudescence of vitalism.

A discussion of the problem of "vitalism" cannot be entered upon here, but certain it is that with elucidation of the nature of enzymes will come the solution of one of the most important factors on which life depends.

THE CYTOLOGICAL PROBLEMS ARISING FROM THE STUDY OF ARTIFICIAL PARTHENOGENESIS

D. WARD CUTLER, M.A. (Cantab.)

Introduction.—Early in the present century biologists became acquainted with the work of Loeb and others, showing that, without the intervention of the spermatozoa, the eggs of many animals could be treated and made to develop ; though without such treatment they would undergo no further development.

As is usually the case, these results were unknown to the general public until the press gave a more or less accurate account of how Bataillon had caused frogs' eggs to develop simply by pricking them with a red-hot needle. This remarkable fact fired the imagining of the more enterprising newspaper writers, who at once speculated as to what would occur when this, or similar methods, were applied to the higher animals—even man himself. The feminist movement was reaching its height at this time, and, as can be easily imagined, artificial parthenogenesis, as the phenomenon was termed, for a short time was keenly discussed.

Natural parthenogenesis, or the development of an egg not fertilised by the male element, is, of course, a well-known phenomenon in the animal kingdom, though, with a few exceptions, confined to one big group—the Arthropoda—to which belong the insects and crustacea.

The present discussion, however, will be confined to artificial parthenogenesis, though certain aspects of natural parthenogenesis [1] will have to be considered, in order to elucidate much which follows.

As I have just stated, artificial parthenogenesis did not receive much attention at the hands of biologists until the beginning of the present century, but as early as 1847 Boursier stated that a virgin silkworm placed in sunlight and then in shade produced eggs from which caterpillars had developed. Tichomoroff in 1886 obtained similar results by rubbing the eggs between two pieces of cloth, and in 1902 by using concentrated sulphuric acid as stimulating agent. These experi-

[1] For a full discussion see Cutler, D. W., *Memoirs Lit. and Phil. Soc.*, Manchester, **62**, 1917.

ments are, however, open to criticism, as later work has shown that a small proportion of unfertilised silkworm eggs will develop parthenogenetically without the use of stimulating agents.

Our first real knowledge of artificial parthenogenesis we owe to Loeb, Delage, Bataillon, Lillie, and many others, who worked in the first place with Echinoderm and Frog eggs.

We now know that the phenomenon can occur in animals widely separated, as, for example, Insects, Echinoderms, Mollusca, Amphibia, to mention only a few cases. Further, the methods which can be employed are very varied, the chief ones being:

(1) Hypotonic solutions (distilled water).
(2) Nearly isotonic solutions made by adding to sea-water or to distilled water the following substances: acids, alkalies, neutral salts.
(3) Hypertonic solutions made by the addition of the above mentioned substances.
(4) Mechanical shocks, thermal changes, fat solvents, alkaloids, blood sera, bile salts, etc.
(5) Electrical shocks by induction coils.

These various agents are not effective on all species of eggs, indeed, some are only of use for one particular species. It is one of the problems of the future to ascertain whether there is to all the methods a common factor capable of initiating the development of unfertilised eggs; until this is done it is impossible to decide what action the sperm has at fertilisation causing the complicated series of evolutionary changes culminating in the adult animal.

For such reasons it is impossible to accept the complicated and highly problematical physico-chemical explanations given by Loeb [1] and others.

The discovery of artificial parthenogenesis is, however, of great importance in demonstrating that many, if not all eggs, are capable of development without the co-operation of the male element, provided that suitable means are adopted to effect this result. In the past it was assumed that the egg was an inert protoplasmic mass incapable of performing its functions unless it were revivified and changed by the spermatozoa. The truer conception appears to be that the egg has an evolutionary course before it leading to the adult animal, and that the sperm or artificial agents simply direct this course without in any way altering or initiating the process.

It is no place here, however, to enter into the many interesting problems that artificial parthenogenesis raises, but rather to discuss the narrower cytological ones.

[1] Loeb, J., *Artificial Parthenogenesis and Fertilisation.* Chicago, 1913.

This aspect of the problem is of importance because of its bearing upon two fundamental cytological theories—the individuality of the chromosomes, and the sex chromosomes—those peculiar bodies which appear to be closely connected with sex-determination. It will be convenient, therefore, at this stage briefly to summarise the salient points upon which these two hypotheses are built, leaving to a future article the discussion as to how the cytological investigations on artificially parthenogenetic eggs affect the two theories. For a full discussion of the theory of chromosome individuality and sex-determination the reader is referred to the two excellent books by the late Prof. Doncaster, *An Introduction to Cytology* and *The Determination of Sex*, both published by the Cambridge University Press.

Individuality of the Chromosomes.—This theory, largely due to the classical work of Boveri, is accepted by most cytologists, though dissentient views are not rare. The early upholders of the theory considered the chromosomes as entities, which persisted without suffering any change from one cell division to another. This extreme view, as it may be termed, has had to be abandoned, research having demonstrated that in many cases the chromosomes do not persist in this way, but may become broken up into smaller units. The eggs of Ascaris afford a good example of this. In the fertilised egg there are two long chromosomes, which, during the subsequent cell cleavage, lose their thickened ends, and in those cells, not destined to give rise to the germ tract, the rest of the body of the chromosomes becomes resolved into small granules. There are other reasons also for believing that chromosomes cannot be regarded as units.

To meet these facts, the original hypothesis has been replaced by one regarding the chromosomes as composed of small portions, to each of which individuality is assigned.

In this modified form the theory has many experimental facts to support it, and the chief ones may be mentioned here.

It has long been known to biologists that, for a given species of animal and plant, the number of chromosomes is a constant. To this rule there are exceptions tending, however, to support the hypothesis of individuality rather than otherwise. Thus the study of abnormal variations in the chromosome number in the egg of Ascaris gives, to my mind, an extremely strong proof of the hypothesis. Sometimes during the second maturation division one or more of the chromosomes, destined to be extruded with the polar body, is accidentally left in the egg. Such chromosomes then form a normal reticulate nucleus, and, at a later period, in this nucleus there appear as many chromosomes as went to its original formation. If there was

no individuality in these units one would have expected an aggregation of these chromosomes into the number characteristic of the species, but such, as we have seen, is not the case. From such observations, Wilson [1] was able to state that "whatever be the number of chromosomes entering into the formation of a reticular nucleus, the same number afterwards issued from it."

A confirmation of this statement is supplied by those experiments in which non-nucleated fragments of eggs have been fertilised by a spermatozoon. Larvæ have been reared from such eggs, and in every case the chromosome number has been half that characteristic of the species.

The retention of a constant number of chromosomes in the nucleus is also illustrated by those cases of natural parthenogenesis in which, through the reduction division of the germ-cells, the chromosomes became reduced to one-half their usual number (haploid condition). This reduced number persists in the germ-cells of such animals, though Nachtsheim [2] has shown that, in the case of the male bee, they are restored to their original number (diploid) in the somatic cells.

Finally, in this connection mention may be made of the recent interesting observations on the Crustacean Artemia.

Artom [3] finds that there is a race of this animal from Capodistria in the Adriatic, which reproduces parthenogenetically, and has a constant chromosome number of 84, while a race from Cagliare reproduces sexually, possessing 42 chromosomes, half the size of those of the parthenogenetic race. Similar doubling of the chromosome number has been found and described under the term tetraploidy in several plants.[4] In the majority of animals and plants the chromosomes cannot be distinguished in the resting stage of the nucleus—which, by the way, is a most unfortunate term, as, whatever else may rest, the nucleus does not.

In this so-called resting stage of the nucleus, however, in a few animals and plants it has been found possible to trace the outlines of the chromosomes from one mitosis to the next.[5] Though this is an advance on the work of the early cytologists, yet Boveri and Van Beneden showed in Ascaris that at the formation of the spireme in mitosis the chromosomes reappear in the same position they occupied at the formation of the reticulum. By applying the conclusions from such observations to fertilisation, Boveri gave effect to the now famous statement that "in all cells, derived in the regular course of division from

[1] Wilson, E. B., *The Cell in Development and Inheritance*, p. 296. Second Edition. New York, 1900.

[2] Nachtsheim, H., *Arch. Zellforsch.*, **11**, 1913.

[3] Artom, C., *Arch. Zellforsch.*, **7**, 1912, p. 277.

[4] Gregory, R. P., *Proc. Roy. Soc. B.*, **87**, 1914, p. 484.

[5] Stout, A. B., *Arch. Zellforsch.*, **9**, 1912, p. 114.

the fertilised egg, one half of the chromosomes are of strictly paternal origin, the other half of maternal." [1]

This conclusion received strong support from the later work of Rückert,[2] Hacker,[3] and others, who have been able to trace and follow through the later cleavage stage of the egg the two sets of chromosomes.

Investigations on the cytology of hybrids has added a further proof in support of the theory of chromosome individuality; an excellent example of this is furnished by the cytological work of Doncaster on hybrids of two species of moths.[4] *Nyssia zonaria* has 112 small chromosomes as the unreduced number, four of which are larger than the rest. In the related species of moth, *Biston hirtaria*, there are 28 chromosomes, four of which are smaller than their fellows. After the maturation division has taken place each species has the reduced chromosome number, viz. Biston 14, two smaller than the rest. Nyssia 56, two larger than the rest.

The chromosomes of the hybrid are 70 in number, 14 of which are noticeably larger than the rest. Perhaps the most interesting part of all is, however, the subsequent behaviour of these chromosomes during spermatogenesis. As each species has only contributed a single set of chromosomes, it is impossible for them all to pair together at synapsis, with the result that the nuclei of the spermatocytes contain only a few less chromosomes than do the spermatogonial nuclei.

Except on the theory of the individuality of the chromosomes this fact, and others which might be cited, is inexplicable.

Sex Chromosomes.—It has already been mentioned that the chromosomes of a species are in general constant in number, and that it is legitimate to conclude that half of them are of paternal origin and half of maternal. During the development of the germ-cells, however, this diploid number becomes reduced to one half—the haploid number. The cell divisions by which this is brought about are known as the maturation divisions, and are comparable in both sexes; the chief difference being that, in the case of the male, four spermatozoa are formed by the division of the spermatogonium, or mother-cell, but, in the case of the female only one egg with its polar bodies, which ultimately disappear. Broadly speaking, the polar bodies are the means by which half the chromosomes are extruded from the maturating egg.

This reduction occurs by a complicated method which cannot be dealt with here, but briefly it consists in the pairing together of the maternal and paternal chromosomes, which subsequently

[1] Boveri, Th., *Merkel und Bonnet's Ergebnisse I.*, 1891, p. 410.
[2] Rückert, J., *Anat. Anzeiger.*, **10**, 1895.
[3] Hacker, V., *Zool. Jahrb.*, **5**, 1892.
[4] Harrison, J. W., and Doncaster, L., *Journ. genet.*, **3**, 1914, p. 234.

separate to pass indiscriminately one to each pole of the division spindle. In this way the chromosomes are randomly distributed to the resulting cells.

Up till quite recently it has always been assumed that the chromosomes of the two sexes of an animal or plant were alike in number, and a connection between sex and chromosomes was never anticipated. In 1902, however, McClung,[1] arguing on *à priori* grounds, suggested that the accessory chromosome was a sex-determiner. This accessory chromosome was discovered by several observers, who found that in the males of certain insects of the Hemiptera and Orthoptera groups there was one chromosome which behaved differently from its fellows during the maturation divisions. Thus it took no part in the pairing process just described, but passed undivided to one pole of the division spindle when the other chromosomes did so. In this way the resulting spermatozoa were of two classes, viz. those possessing the accessory chromosome and those lacking it. Wilson[2] and others who took up the investigation speedily discovered that this unpaired heterotropic or X chromosome—names by which it is often known—is paired in the female, who thus possesses two such chromosomes. These behave, during maturation, in a normal manner, with the result that all the mature eggs contain a single X chromosome.

Thus at fertilisation two classes of eggs are produced.

(1) Eggs fertilised by a sperm carrying the X chromosome giving rise to a zygote with the XX constitution.

(2) Eggs fertilised by a sperm carrying no X chromosome giving rise to a zygote with the X constitution.

The first type of zygote will develop into a female, the second type into a male. This is indicated in the first diagram below, the symbol O representing the absence of an X chromosome.

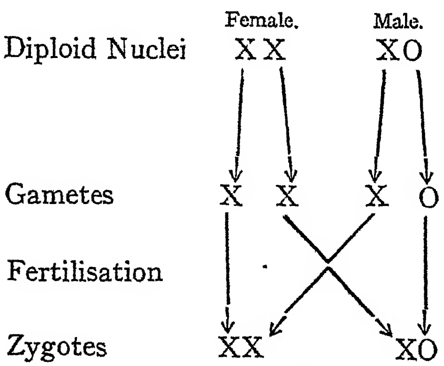

Fig 1.

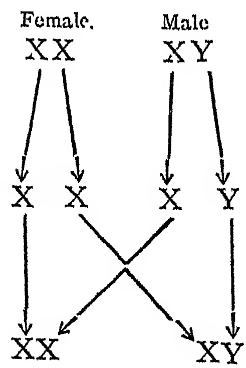

Fig. 2.

[1] McClung, C. E., *Biol. Bull.*, **3**, 1902.
[2] Wilson, E. B., *Journ. Exp. Zool.*, **3**, 1906, p. 1.

Early in the investigation Wilson found, in Hemipteran insects, that there was a second case in which the X chromosome of the male is accompanied by a mate of a different type, usually much smaller than X and termed the Y chromosome. In this case, X and Y conjugate during maturation so that half the resulting gametes contain X and half contain Y. The X class are female-producing, the Y class male-producing. (Fig. 2.)

These two types are connected by a series of intermediate conditions, which in the main are comparable with the two simple cases given above.

This postulate of the fertilisation of an egg by two different classes of spermatozoa has received proof from observations on the nematode, Ancyrocanthus, where the chromosomes remain visible in the mature spermatozoa and can be counted in the living object. Mulsow traced both types of spermatozoa into the egg and was able to convince himself of the sexual differences in the germ nuclei. Further, an X chromosome has recently been described in a Liverwort.

It is impossible to go into all the evidence for the conclusion that the X chromosomes are intimately connected with sex-production ; suffice it to say that the peculiar type of inheritance known as sex-limited fits so beautifully with the sex-chromosome hypothesis as to render it difficult to accept any other view. This aspect of the subject is admirably treated by Morgan in his latest book, *The Mechanism of Heredity*.

Finally, mention may be made of the surprising position of the Lepidoptera and Birds as regards sex-production.

For some years it has been known, from a study of sex-limited inheritance, that in these two classes of animals the usual condition was reversed, leading to the conclusion that there were male and female determining *eggs*, not sperms, as in other classes of animals.

Cytological investigations by Seiler [1] and Doncaster [2] have shown that, as regards the Lepidoptera, there are females producing two types of eggs. Thus we must conclude that, in the majority of animals, the spermatozoa are different, and the eggs alike, but that in the Lepidoptera, at least, it is the sperms that are alike and the eggs which differ, as regards the accessory chromosomes.

To account for the mode of action of the accessory chromosomes in sex determination two hypotheses have been put forward—the qualitative and the quantitative.

In the former it was assumed that the female was heterozy-

[1] Seiler, J., *Zool. Anzeiger*, **41**, 1913, p. 246; *Zeitschr. f. indukt. Abst.*, **18**, 1917, p. 81.

[2] Doncaster, L., *Journ. Genetics*, **4**, 1914, p. 1.

gous for sex, and that the two X chromosomes carried respectively male and female factors, femaleness being dominant. The male was, on the other hand, homozygous, the single X chromosome carrying a male factor.

Thus, if M and F stand for maleness and femaleness respectively, every female has the constitution M F and every male the constitution M O, O denoting the absence of any sex-determiner. Mature eggs would, therefore, be of two kinds, those carrying M and those carrying F; and two kinds of sperms, M and O carriers.

At fertilisation four classes of zygotes would be produced, MM, MF, MO, FO, if any class of sperm was capable of fertilisating any class of egg. As FO and MM would, *ex hypothesis*, give rise to no known sex, the theory was modified by assuming selective fertilisation, in which only F-bearing eggs could be fertilised by M-bearing sperms, and M-bearing eggs only by O-bearing sperms.

In the Lepidoptera and Birds, on this hypothesis, maleness is dominant to femaleness and the male is of constitution MF, the female FO.

The great objection to this theory is the assumption of selective fertilisation, for which there is at present no justification. The assumption that the male is heterozygous for sex, and the female homozygous, as Correns[1] has suggested in his theory, overcomes the difficulty of selective fertilisation, for it is obvious that either a male or a female carrying sperm can fertilise the ova, which are, in this case, all alike in having the female constitution. On other grounds, however, the theory must be abandoned. For instance, taking the case of the bee, it is well known that the males are produced from parthenogenetic eggs, which have undergone their reductive divisions. The chromosome number, therefore, is halved or haploid.

Now, such eggs must contain the male determiner, for otherwise how do drones develop from them? But, according to the theory, no such determiner is present in such eggs.

These qualitative hypotheses to account for sex-determination are subject to such grave objections that they are now generally abandoned in favour of the simpler quantitative hypothesis first propounded by Wilson[2] and later by Castle.[3]

Here it is suggested that there are not two distinct factors, one for maleness and one for femaleness, but that the sex depends on the quantity of sex-determining substance carried by the X chromosomes. Thus, in the majority of animals the female is quantitatively greater in some substance than

[1] Correns, C., *Bestimmung u. Vererbung des Geschlects*. Leipsig, 1907.
[2] Wilson, E. B., *Science*, **29**, 1909, p. 53.
[3] Castle, W. E., *Science*, **29**, 1909, p. 395.

the male: "femaleness is due to maleness plus something else."

The first objection to the hypothesis is the inheritance of certain characters in Lepidoptera and Birds, when it is necessary to assume a reversal of the above order of things. As mentioned above, however, there is experimental evidence that cytologically such a reversal is justified for at any rate the Lepidoptera.

This is undoubtedly an unexpected conclusion, but, as Doncaster has said, "It can hardly be coincidence that the spermatozoa should be dimorphic in respect of a chromosome in the forms in which sex-limited inheritance by the male takes place, and the eggs dimorphic in the same way in those in which sex-limited transmission is by the female."

Although it is impossible to give any large part of the evidence in support of this quantitative theory of sex-determination, yet it is sufficiently great to warrant its general acceptance.

I say the general acceptance, because none of the theories outlined above are completely satisfactory. The quantitative one is certainly the most fitting at the present; but there are objections to it, as, for example, the two kinds of parthenogenetic eggs laid by the gall-fly *Neuroterus lenticularis*,[1] one of which produces males only and the other females. Obviously in this case the sex is determined before maturation.

Further, the experiments of Geoffrey Smith [2] and others have shown that in certain instances the sex of an animal can be modified or even reversed by changed metabolic conditions. Such cases do not appear to accord with the theory that the quantity of sex-determiner, carried by the X chromosomes, is the cause of sex-determination. Cause is, however, a word that should not be used in this connection, for it is highly probable that the X chromosomes do not cause a particular sex to develop, but are rather links in a long chain of events. This has already been pointed out by Doncaster, who makes use of the following analogy to illustrate the point:

"A locomotive engine has a reversing lever which determines whether the engine shall go forwards or backwards, but the lever is not the cause of the motion of the engine; and by altering other parts of the machinery it could be arranged that when the lever was reversed the engine should go forwards. In the same way it is possible that, other things being equal, the presence of a certain chromosome may lead to the development of a particular sex, but it is not impossible that other

[1] Doncaster, L., *Proc. Roy. Soc. B.*, **82**, 1910, p. 88; **83**, 1911, p. 476; **89**, 1916, p. 183.

[2] Smith, Geoffrey, *Quart. Journ. Micros. Sci.*, **55**, 1910, p. 225; **57**, 1911, p. 251; **59**, 1913, p. 267.

changes may in exceptional cases so alter the whole mechanism that this effect is not produced."

It must always be remembered that the chromosomes and the cytoplasm are in intimate connection, and that the latter may exert modifying influences on the former. This may be illustrated by another analogy of Doncaster's "If in the hydrocarbon CH_3H the fourth hydrogen atom is replaced by chlorine (CH_3Cl) or by hydroxyl (CH_3OH), substances of very different properties are produced. The cytoplasm, in this analogy, is represented by the hydrocarbon radicle $C\ H_3-$ and the effects on the general nature of the resulting compound of substituting $-Cl$ or $-OH$ for the $-H$ of the hydrocarbon represents the effect produced by substituting a chromosome 'bearing' one factor for a chromosome bearing another."

A parallel series could be C_2H_5H, C_2H_5OH. C_2H_5Cl. The hydrocarbon radicle (cytoplasm of the analogy) is not negligible since $C\ H_3Cl$ differs considerably from C_2H_5Cl; but it is true to say that the substitution of a hydrogen atom by an hydroxyl radicle turns hydrocarbon into an alcohol: and in the same way the replacement or addition of a chromosome may considerably alter an egg's potentialities.

The cytological aspect of Artificial Parthenogenesis will be considered in a future article; but this somewhat long introduction is necessary to make clear many of the problems raised by a consideration of the results obtained in this field of research.

POPULAR SCIENCE

THE SOYA-BEAN PROBLEM

By DOROTHY MARGARET ADKINS, B.Sc.,
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THE remarkable characters of the soya-bean, and the new light that recent research has shed on its properties, seem to render a wider recognition of its economic value desirable. It is a plant that has been a staple article of industry for thousands of years in China, and has during the last fifty years been shown by modern methods of research to possess a supply of accessory food factors and exceptionally valuable protein; on account of these and other properties, it has recently obtained an increasingly important position in American agriculture and industry. It is with the hope of reviewing our present position with regard to this plant that the following outline has been drawn up.

The soya-bean, *Glycine hispida*, is an erect annual with branching hairy stems and trifoliate leaves. The flowers are pale lilac or violet; the seeds may be yellow, green, brown, or black, and are elliptical or spherical in shape. Under favourable conditions the plant attains a height of four feet, and may produce as many as two hundred pods, although the average number per plant is nearer forty. The flowers are self-pollinated, thus the yield is entirely independent of insects. Like all leguminous plants, the soya-bean increases the fertility of the soil owing to bacterial tubercles on its roots; it has been demonstrated that only when a large number of nodules is present can the plant reach its full development. Thus when seeds are sown on soil sterile with regard to the soya-bean bacillus, inoculation is necessary for the production of a good crop.

The soya-bean is indigenous to China, Manchuria, Korea, Japan, and Indo-China; the Annals of Old China record its extensive cultivation and use five thousand years ago, and its products still form one of China's most important exports. During the nineteenth century the soya-bean was introduced into America and Europe: in the United States it gained prominence as a forage crop, and has recently been grown on a large scale for seed; its introduction into agriculture in Europe has, however, never become established.

In the Far East oil is expressed from the seed in primitive factories, and the bean cake is used for food. In the United States, cotton-seed mills are employed in the preparation of soya-bean oil, large quantities of which are produced annually. During the Russo-Japanese War the soya-bean crops in Manchuria were greatly augmented in order to meet the increased demand for food, with the result that after the war it became necessary to find new openings for the soya-bean trade. Beans were therefore sent to Europe; but the enterprise was not successful, owing to bad shipping, on account of which the beans arrived in an unsatisfactory condition. In 1908 another trial shipment was sent to English oil mills; the consignment was a success, and larger imports were made. Since that time the soya-bean oil has received attention from many oil-producing companies in England, and a growing industry has arisen.

Practical Applications of the Bean

Modes of Preparing the Bean for Food.—In Japan, China, and some parts of India the soya-bean is second only to rice in the food of the natives. Since the beans contain much protein and oil, they are admirably suited for compensating a rice diet, which is composed almost entirely of carbohydrate. The following preparations are used widely [1]:

Tofu, or Bean Cheese (Japanese).—Beans are soaked, crushed, and mixed with water, then boiled and filtered. Mother liquor from the preparation of salt from seawater is added to the filtrate, a precipitate occurs and is pressed and served.

Miso (Japanese), *similar to Chiang* (Chinese).—Beans are steamed, powdered, and mixed with barley, salt and water. The mixture is caused to ferment by the addition of a fermenting agent.

Shoyu (Japanese) *and Chiang-yu* (Chinese): *Soya Sauce.*—Beans are steamed and mashed; an equal quantity of powdered wheat is added, also spores of *Aspergillus oryzæ*. The whole is left in a cellar at a constant temperature of 10° C. Heat is evolved; after six days the contents of the trays are dried and ground finely, water and salt being added. The mixture is then allowed to ferment for one to six years. A thick brown liquid resembling meat extract is formed.

Natto (Japanese).—Soya-beans are boiled; small portions of the boiled material are wrapped in straw, and placed in a cellar where there is a fire. The beans ferment, a thick sticky mass forms round them and makes them very palatable.

In Japan beans are germinated until the sprouts are about

[1] Oshima, *U.S. Dept. Agr. Off. Exp. Sta. Bull.*, **159**, p. 224, 1905.

five inches long, and eaten raw with vinegar ; beans, germinated and treated with brine, have also been noted in Spain.

Soya-beans may be cooked and used in the same way as haricot-beans, and may also be picked when young and treated like green peas, in which condition they may be canned.

In China vegetable milk is prepared from soya-beans ; it is suitable for replacing milk in cakes, chocolates, and custards. If the vegetable milk is kept for several days it turns sour, and can be used as butter-milk. In Japan the vegetable milk is condensed. One firm in England is engaged in the production of a similar milk from soya-beans.

Beans may be grown till their shoots are six or seven inches high, and then cut as asparagus; also seedlings about two inches high may be blanched with boiling water, washed in cold water, dried, and used for salad.

In Switzerland and America soya-beans are roasted, ground, and used as a coffee substitute.

Soya-bean flour is employed in the United States for bread-making ; experiments show that the bean flour increases the nutritive value of the bread without detracting from its flavour. In pastries, muffins, marzipan, pastry-fillings, etc., soya-bean flour can be used successfully. In Germany soya-bean flour is mixed with rye flour for brown bread.

On account of its low carbohydrate content, the soya-bean finds a place among diabetic diets, and is produced by firms specialising in foods of low starch content.

In the manufacture of macaroni, soya-bean products are sometimes used.

Utilisation of Soya-Bean Oil

Soya-bean oil can be refined for edible purposes, and used in compound lards and cooking-fats ; in Italy the oil is used largely in culinary operations.

The oil has almost replaced linseed oil in the preparation of soft soap, and can partially take the place of cotton-seed oil in hard soap.

The semi-drying and non-congealing nature of the oil renders it useful as a substitute for linseed oil in paints, varnishes, and enamels.

In China the oil is used for illuminating purposes ; no lamp is necessary to hold the oil, which is placed in a basin or plate in which a wick has been inserted.

It is the custom in North China and Manchuria to grease axles and various parts of native machinery with soya-bean oil.

Soya-bean oil is used also in the manufacture of linoleum,

explosives, waterproof goods, rubber substitutes, and printing inks.[1]

Utilisation of Soya-Bean Cake and Meal

For many centuries the sugar plantations of South China have been treated with soya-bean meal; its fertilising value depends mainly on its store of nitrogen, but it contains also some available phosphorus and potassium. The meal is employed in Japan as a fertiliser for yams, rice-fields, and mulberry-trees.

The cake and meal are in great demand for feeding stock throughout the agricultural world. Analyses of both products show a high food value; feeding experiments with animals prove that cattle, sheep, and pigs show very satisfactory growth when soya-bean derivatives form a reasonable part of the daily food; injurious results, however, are obtained when the ration is not well balanced. The meal and cake are as efficient as other rich foods and are less expensive.

Food Value of the Bean

The high food value of the soya-bean is due to its oil and protein content; the oil when refined for edible purposes is well digested by the human body, and is useful for cooking purposes.

The protein content of the bean has formed the subject of much investigation, especially in Japan and America. The chief protein constituent, glycinin, contains both lysine and tryptophane, which are recognised by students of nutrition as essential for "growth" and "maintenance" respectively.[2] Experiments on rationed animals have shown conclusively that glycinin is superior for growing animals to gliadin, hordien, and zein. Soya-beans compare very favourably with garden peas and kidney-beans with regard to protein value and utilisation, for the protein of peas and kidney beans is of low biologic value and badly digested.[3] Damets and Nichols[4] hold that their experiments indicate that soya-bean protein is as valuable as the casein of milk.

The exceptional position held by the soya-bean is summarised by Osborne and Mendel as follows[5]:

"So far as we are aware, the soya-bean is the only seed hitherto investigated, with the possible exception of millet, which contains both the water-soluble and in a limited quantity

[1] Table in Morse, *U S Dept. Agr Farm. Bull*, 973, 1918.
[2] Osborne and Clapp, *Amer. Journ Physiol.*, **19**, p. 468, 1907.
[3] Holmes, *U.S. Dept. Agr. Bull.*, 717, 1918.
[4] *Journ. Biol Chem.*, **32**, p. 91, 1917.
[5] Osborne and Mendel, *Journ. Biol. Chem.*, **32**, p. 369, 1917.

the fat-soluble unidentified dietary essentials or vitamines. This fact, taken in conjunction with the high physiological value of the protein, lends a unique significance to the use of soya-beans as food."

The same authors conclude that soya-beans may be properly classed as a substitute for meat and wheat, and they should form a part of human dietary.

The Cultivation of the Soya-Bean

The methods employed for the cultivation of the bean in China and Japan are very primitive, but they prove efficient in producing a good crop. The plough used is drawn by a mixed team of oxen, mules, and donkeys, and has only one handle and a rough, steel-tipped cutter. Sowing, hoeing, and reaping are done by hand. For threshing, the mature plants are strewn on a floor and a cylindrical stone with longitudinal slits in it is drawn over them. The beans are winnowed by being thrown against the wind. In spite of these wasteful methods of procedure, the soya-bean fields are a success.

Culture of soya-beans in the United States is carried on along very different lines. Throughout the country the crop has been tested at fully-equipped experimental stations, with the result that modern methods of preparing land and handling crops have been adapted to suit the plant and have produced good crops. Experiments have shown that the soya-bean is useful in binding and enriching the soil, is fairly drought-resistant and capable of withstanding much moisture. The beans are grown for hay, pasturage, ensilage, and grain; also they are used in mixtures with maize, cow-peas, sorghum, or Sudan grass.

During the last twenty years attempts have been made in various parts of the British Empire to cultivate soya-beans. In Australia, New South Wales appears to be the most successful state in raising the crop. The "Black" and "Guelph" varieties are specially recommended. A most interesting "sport" variety [1] has arisen from the "Ebony" variety, and is under cultivation for further exploitation. In Victoria and Queensland certain districts have proved favourable for soya-beans. West Australia and South Australia are mostly too arid for successful soya-bean culture. In every state in Australia it is impossible to procure the requisite labour cheaply; thus Australian beans cannot compete with Manchurian beans on the European market, for in Manchuria almost unlimited cheap labour can be obtained.

In South Africa success has been achieved in growing the

[1] *Proc. Linn. Soc. N.S. Wales*, **43**, p. 611, 1918.

plant; in 1910 the outlook was so hopeful that a project for constructing oil mills was suggested. Unfortunately the bean was not taken up by farmers, who preferred to cultivate maize, as it was an easier crop to produce. Thus no extensive culture of the bean was attempted, and the subject was dropped.

In other parts of the Empire, for example the West Indies, British East Africa and West Africa, trials of soya-beans have proved successful, but in no district have promising early experiments been followed by tests on larger areas. It is obvious that, unless a large acreage is devoted to the production of the crop, it is impossible to erect oil mills on the spot with a view to commercial enterprise, or to export beans on such a scale that they can assume any economic importance.

In certain parts of India, for example Burmah, soya-beans are grown on a large scale and are consumed by the natives. Work has been done by Woodhouse[1] and Taylor to encourage a more extensive use of the bean, and to obtain varieties rich in oil which are suitable for local conditions. When an attempt is made to export the beans, only low prices can be obtained, so at present the financial outlook for the grower is not very hopeful.

In England several tests[2] have been made on soya-bean growing, but in most cases no adequate returns have resulted, the seed, even when it was produced, failing to form mature plants the succeeding year. In 1913 the Royal Botanic Society obtained a few seeds of a variety which flourished in Central Europe, and from these seeds increasingly large crops of the beans have been raised in England. Experiments on this variety are still in progress in different parts of the country.

From agricultural tests performed, it is permissible to note the following points:

(1) In order to be successful with soya-beans, it is necessary to acclimatise the plants, growing crops on a larger scale from locally produced seed.

(2) Tests by the United States Department of Agriculture on five hundred varieties of soya-bean show that it is of utmost importance to choose the variety according to the local conditions of climate and soil.

(3) No extensive experimental plot has been grown in any of the districts in the British Empire which have proved favourable to the growth of the soya-bean in small areas. Until a test on a large scale is performed, it is impossible to predict whether the crop will overcome difficulties caused by lack of labour and cost of initial outlay.

[1] *Agr. Journ. India*, 8, 391, 1913; *Memoir Dept. Agr. India, Bot. Sec.*, 5, iii, 1912; *Agr. Journ. India*, 9, p. 308, 1914.

[2] *Journ. Bd. Agr.*, 22, 1916,

Summary and Conclusion

The soya-bean has been established for thousands of years in Eastern countries as a staple food and valuable export. In the United States it has become an important fodder crop, and is rapidly gaining a high reputation as an oil seed.

As a result of recent research, soya-bean varieties which are suitable for many different soils and climates have become known. The equipment necessary for handling the crop successfully as regards inoculation, treatment of land, machinery for cultivating and harvesting the crop, and information concerning optimum time for sowing, etc., can be obtained readily. The actual food value of the soya-bean is shown by highly modern methods of experiment to be especially high, both for human beings and for stock. The oil from soya-beans can be widely employed by soap, paint, and other manufacturers.

Experiments in some parts of the British Empire show that the local conditions are favourable to the cultivation of the soya-bean, but difficulty in obtaining the necessary labour or the great initial expense has prevented any serious attempt to start soya-bean culture on an economic basis. At a time such as the present, when it is essential for the Empire to make the best use of every opportunity for the improvement of economic conditions, *it is hoped that we shall continue to encourage this promising crop, so that we may avoid repeating the history of our belated support of the sugar-beet.*

NOTES

William Crawford Gorgas (Sir Ronald Ross)

THE death of Gorgas at the Military Hospital in Millbank on July 4, 1920, removed one of the very few workers at that most important and most despised branch of human effort, the science and art of sanitation. He was born at Mobile, Alabama, on October 3, 1854, being the son of a high officer of the Federal Army. He was educated in New York and Tennessee, and entered the Medical Corps of the United States Army at the age of twenty-eight. When the Americans took Cuba from the Spaniards he was appointed sanitary officer there, and when, at the end of 1900, Reed, Carroll, Lazear, and Agrimonte showed that yellow fever is carried by mosquitoes—as I had previously shown malaria to be—he was instructed by General Leonard Wood, himself a medical man and Commandant of the American forces in Cuba, to attack the mosquitoes in Havanna on a large scale. I had advocated this measure two years previously, but Gorgas, being supported by the American Government, performed the work with great success, while I had been rather opposed than helped by the Governments of my own country. When the Panama Canal was commenced about 1903 he was appointed head of the whole sanitary and medical staff, and by his vigorous anti-mosquito work was able to exclude yellow fever and malaria almost entirely from the canal zone. Later he was made Director-General of the American Army Medical Service—because he was the most distinguished medical man in that service, a thing which seldom or never happens in British administration. After the war he went to various places in South America to help the authorities to deal with the same diseases; and a few years ago was asked by the British authorities to form a commission to study and prevent yellow fever in West Africa, our own countrymen being excluded from this task, first because they had invented the method which Gorgas used, and secondly because they were Britons. Shortly before his death, however, our King, who, I am glad, does not share all the qualities of his subjects, offered him the honour of the K.C.M.G.; and it was when he had come to England to receive this honour that he was taken ill with his last sickness.

Fortunately, His Majesty conferred the honour on him while he was in hospital.

I had corresponded with Gorgas since the beginning of this century, if not before, and when the Canal was being started he asked me to go to Panama to advise regarding the sanitation there. I met him first as I was leaving New York for Panama in the s.s. *Advance* on September 27, 1904, while he was home on leave, and again on several occasions when he came to England, especially once when we gave him a dinner in honour of his work. He was a man of fine tact, and an agreeable manner, and his actions were always to the point. After his death there was a memorial service at St. Paul's in his honour on July 9, 1920. He was Surgeon-General in the American Army, and an M.D., D.Sc., LL.D.; and had been made Director of the International Health Board of the Rockefeller Institute. He was buried in the United States.

As there is much ignorance both here and in America regarding the relation of Gorgas's work to that of other men on the same lines, I thought it advisable to send a brief history to *The Times* (July 24), giving a chronological summary of the facts; and I now think it advisable to reprint this summary in SCIENCE PROGRESS for a record—especially as the subject is constantly being misstated in the British medical and lay press. The truth will not diminish the great honour which the world owes to Gorgas.

The story begins long ago. The ancients were well acquainted with malaria and connected it, not only with marshes, but with insects in marshes; and indeed Empedocles is said to have delivered Selinus in Sicily from it by drainage about 450 B.C. Morton (1697) and Lancisi (1717) repeated these speculations, and the latter studied mosquitoes in Rome, and employed drainage against the disease. In 1851 and later, F. Küchenmeister and R. Leuckart proved that many of the larger parasites of men and animals live, not only in one species of "host," but in two species, one of which feeds on the other. In 1858–9 Leuckart suggested and Fedschenko proved that the famous Guinea-Worm of man lives partly in a "water-flea" called *Cyclops*. In 1877 P. Manson showed that another parasite of man, a *Filaria*, which causes elephantiasis, has a similar development in a kind of mosquito—but neither Fedschenko nor Manson completed the life-histories of these organisms. In 1880 A. Laveran discovered that malarial fever is caused by millions of minute animal parasites in the blood—not bacilli, as often stated. In 1881 and later C. Finlay suggested, without proof, that the mosquito called *Stegomyia fasciata* carries yellow fever directly from man to man by its bites. In 1883 A. F. A. King recorded a number of arguments in favour of his view that mosquitoes carry the infection of malaria from the marsh to human beings; and next year both A. Laveran and R. Koch suggested, without citing reasons, that malaria is carried by mosquitoes. After 1885 C. Golgi and other Italians made classical observations on the parasites of malaria; and in 1889 T. Smith and P. L. Kilborne showed that Texas Cattle Fever is due to similar organisms carried by ticks—but without actually finding the former in the latter. In 1894 P. Manson added a strong argument to the mosquito-theory; certain forms of the malaria-parasites produce

active bodies then thought to be merely "flagella"; but Manson now maintained that they were really flagellated spores, meant to infect mosquitoes which bite infected men; and he thought that the infected insects carry the germs from men to the marsh, from which the germs infect healthy men in exhalations—just the opposite of King's hypothesis. The Italians rejected these ideas; and none of the theorists made experiments on the subject, though all of them could have done so. In 1895 D. Bruce showed that certain trypanosomes of cattle are carried by tsetse flies. In 1895–7, I failed to make the malaria parasites develop in mosquitoes of the genera *Culex* and *Stegomyia*; but in August 1897 succeeded in doing so in mosquitoes of the sub-family Anophelina—which really proved the mosquito-theory, though my work was interrupted for six months at the moment of success. At the same time MacCallum and Opie in America showed that what Manson took to be flagellated spores were really sperms—thus explaining certain details of my observations. In 1898, not being able to work with human malaria, I traced out the whole development of birds' malaria in mosquitoes, showed that the parasites pass into the salivary glands of the insects, and infected twenty-three out of twenty-eight healthy birds by the bites of mosquitoes—and was then interrupted again by official duties.

It was thus demonstrated that mosquitoes carry the germ, not from the marsh to men nor from men to the marsh, but from man to man; and in 1898–9 the Italians, with full knowledge of my work, completed my observations on human malaria and infected four healthy men in Rome by the bites of mosquitoes. In 1899 I proceeded to Sierra Leone with E. E. Austen (of the British Museum), H. E. Annett, and R. Fielding-Ould, completed my work on human malaria, found the two chief malaria-bearing Amophelines of Africa, demonstrated their habits, showed how it is that malaria is connected with marshes, and worked out the details of my method of preventing malaria by "mosquito-reduction"—these results being widely published in 1899–1900. In 1900 P. Manson infected several volunteers in London by the bites of mosquitoes brought from Italy. At the end of 1900 the Americans, Reed, Lazear, Carroll, and Argamonte proved by direct experiments that yellow fever is carried from man to man by *Stegomyia* as malaria is by Anophelines; and next year W. C. Gorgas commenced, under the orders of General Leonard Wood, to banish both malaria and yellow fever from Havana by mosquito-reduction. In 1901, also, seeing that little was being done in British possessions, I went again to Sierra Leone with L. Taylor to give an object-lesson on mosquito-reduction by means of privately subscribed funds; and proceeded to Lagos, where the Governor, Sir William MacGregor, was starting the same work; and in that year similar excellent campaigns were commenced by Malcolm Watson in the Federated Malay States, and under F. Clarke in Hongkong; and the first volume of F. V. Theobald's classical monograph on mosquitoes was published by the British Museum. In 1902 I went for a third time to Sierra Leone; and then with Sir W. MacGregor to Ismailia, where the Suez Canal Company completely banished malaria by my method. In 1903 A. Balfour began mosquito-reduction at Khartum. In 1904, on the invitation of the American Government, I visited Panama, where the Canal was being commenced, and met Gorgas at New York *en route*; his results are well known. In 1906, E. H. Ross began to clear *Stegomyia* mosquitoes from Port Said; and I visited Greece in that year, Mauritius in 1907 (with C. E. P. Fowler), Bombay in 1909, and Spain, Greece, and Cyprus in 1913, in which latter place R. A. Cleveland has made a notable reduction of malaria.

I do not mention many local or temporary campaigns; nor large campaigns done chiefly on a quinine basis in Italy, Greece, and in French and German possessions; nor work carried on during the war; nor numerous abstract researches of various kinds.

We should note that (1) the various theories enunciated up to 1894

suggested only that some relation holds between malaria and mosquitoes. They were more often wrong than right, and failed entirely even to indicate those two great unknown quantities, the species of mosquito concerned and the form and position of the parasites within it. Both these unknown quantities were disclosed simultaneously by my very lucky observation of August 20, 1897, which provided the key of the whole mystery, and even helped to unlock the yellow fever problem in 1900. (2) The sanitary measure of mosquito-reduction was fully described in 1899, before Gorgas used it; but he was able to employ it (and other measures) against yellow fever as well as against malaria. (3) The whole work has been an international one, in which the British have taken a considerable part. But the British work has been due almost entirely to the initiative of private medical men; and I, for one, have never been employed by my countrymen in an executive capacity to give effect to my own suggestions. On the other hand, Gorgas worked with the whole support of the American State behind him.

Right well did he do it; and we are all glad that the King recognised his achievement before he died.

THE "SCIENTIFIC" LOVER

By CLOUDESLEY BRERETON

FROM the Laboratory he came
And in Love's incandescent flame
Annealed his soul; of chemic school
Alumnus, he reduced to rule
And theorem, every excellence
To which his Love could make pretence.
And first he strove to analyse
The prismic colours of her eyes,
And when he stroked her waving hair,
On the electric fluid there
He made deductions; each sensation
Provided him a new equation.
And when he kissed her in the dark
He calculated out the arc
Her lips described, correcting it
When she the tell-tale gas relit;
And when she raised her under-jaw,
Applying each kinetic law
He found in n and r and a
Her masticating formula.

Her shapely figure does but serve
As typic of some lovely curve;
And when about her voice he raves,
His mind is full of tonic waves;
Weeps she, he finds her tears ancillary,
To thinking out of things capillary;
And since he finds her, like her sex,
A mystery, he calls her 'x.'
And when to get him she doth try
To name the day, he answers, Why? (y)

The Nobel Prizes

We have been informed by the Nobel Committee at Stockholm that the following gentlemen have been selected for the Nobel Prizes given during 1920.

Physics : Charles Édouard Guillaume.
Medicine : August Krogh.
Literature : Knut Hamsun.

The $\sqrt{-1}$: A Protest (Amateur)

Probably modern mathematics differs from past mathematics chiefly in the stress which is now laid upon " Complex Numbers." When algebra was first invented numbers were conceived to be signless; then gradually the so-called negative numbers were introduced; then mathematicians went on to separate rational from irrational numbers; and now their pupils are obliged to twist their brains by the consideration of complex numbers. Every book one reads nowadays commences with a series of paragraphs on these different kinds of numbers, and the reader is often obliged to generalise the simplest functions in terms of the last mentioned. Is there really any advantage in all this? And, though I am only an amateur, I should like to maintain that there is no such advantage, and, moreover, that complex numbers do not exist at all—though I am aware that such a statement will expose me to adverse or even contemptuous criticism. To begin with, it may of course even be doubted whether there are such things as negative numbers—and this doubt has been frequently expressed by the greatest experts. Negativeness is not a property of number itself but merely an expression of the fact that a number has been subjected to the inverse operation of addition. We write -1 merely as a convenience and because no method of expressing operation apart from number is now in general use. If A expresses the operation of addition and A^1 the inverse operation of addition, then instead of writing -1 we should more correctly write $A^1(1)$. This of course would be very cumbersome and we therefore write -1 instead of $A^1(1)$, but merely for convenience and brevity. I mean that in the idea of -1 we possess, not only the idea of a number, but also the idea of an operation acting upon a number—that is, of something more than a number. We therefore have no right to say that negative numbers exist by themselves. So also, all fractions are the results of the inverse operation of multiplication. On the other hand, what are called irrational numbers appear to me to be much more real numbers than negative numbers or fractions, because, the notion of a signless number consisting of the sum of an infinity of signless numbers constantly diminishing in magnitude can be immediately comprehended.

Now let us consider $\sqrt{-1}$. If there is no such thing as the number -1, then, *a fortiori*, $\sqrt{-1}$ is not a number. But more than this, even supposing that we assent to the position that -1 is a number, still there actually is no number which when multiplied by itself produces -1. In fact $\sqrt{-1}$ is not a number and cannot possibly exist as a number. What our modern mathematicians really do is to pretend that it is a number and then to write enormous volumes on the basis of this pretence, thus complicating the whole of the simple and beautiful science of numbers in general—and not only complicating it but, to my mind, falsifying it. Our fathers called complex numbers *unreal* numbers or *impossible* numbers, and they were right. We give them another name, that is complex numbers, and try to persuade ourselves that they are both possible and real. Why not drop the whole pretence so far as algebra is concerned?

But of course I am talking only of algebra. Is there no *operation*, which when once applied to and then once repeated upon a positive number, will turn

it into a negative one? There is nothing of the kind in the science of number, but there is something of the kind in the science of space. If in geometry there is something which can turn a line through a right angle, then that something will, if applied twice, turn the same line through two right angles—turn it from the positive position into the exact opposite and negative position. Therefore the something which can turn a line through a right angle may be admitted as the equivalent of $\sqrt{-1}$; but in geometry only. But, even when looked upon in this light, it is not in any way a number, but is a versor, that is, an operation—which is quite different from any number.

It seems to me (a very humble amateur) that such distinctions should receive more attention in our books. For example, Whittaker and Watson say on page 4 of their *Modern Analysis* that, "From the logical standpoint, it is improper to introduce geometrical intuitions to supply deficiencies in arithmetical arguments"; and yet they go on immediately to appeal to geometrical intuitions in the case of complex numbers and then accept Argand's diagram. To my painfully laborious mind this diagram is wrong because it can apply only in two dimensions. The proper explanation of the rectangular versor is not by means of Argand's diagram but by means of the magnificent solution given by Hamilton—which applies to three dimensions and is universally applicable. Why on earth, so many years after Hamilton, do we continue to talk about Argand's conception? Hamilton generalised it just as Darwin generalised Lamarck's theory. Argand and Lamarck are now both comparatively out of date.

Hamilton's vectors and versors are not numbers but more than numbers. They are the results of operators acting upon numbers and producing something else. The versor which is incorrectly written $\sqrt{-1}$ is not a number but something else. Why not abandon it entirely in algebra, and, if we use it in geometry, use it in the sense of Hamilton and not of Argand? Why pretend what is not true; why persist upon the *number* $\sqrt{-1}$ when we do not insist upon the logarithms of negative numbers to positive bases, and other conceptions which are really non-existent because impossible? Yet our books go on repeating this tale, just as they repeat the fabulous equation $\phi^0 = 1$!

Scorbutic Cooking (R. Ross)

Much work on Scurvy has been done recently by Miss Harriette Chick and her colleagues at the Lister Institute and with the help of the Medical Research Council. In the *British Medical Journal* for October 9 she and Elsie J. Dalyell described an epidemic of scurvy in a children's clinic at Vienna last year. The diet of the children should have contained enough vitamines, and sufficient vegetables were provided for this purpose, but the authors conclude that the scurvy was due to the fact that the vitamines in the vegetables were destroyed; and they make some very important remarks upon the danger of over-cooking vegetables, and also some useful suggestions for removing this defect. They advocate steaming vegetables instead of boiling them. They also advocate the use of germinated seeds (beans, peas, pulse, etc.), which are well known to contain much anti-scorbutic substance.

In my opinion this matter is of very wide importance throughout this country, and I do not think that it affects children only. In my Malaria Clinic (Ministry of Pensions) I see a very large number of cases of chronic malaria. The treatment for this disease by quinine has now become almost certain as regards the destruction and ultimate banishment of the parasites of malaria; but I gather that in many cases the men are quite possibly suffering from an insufficient vitamine-content of their food. The large, number of persons of inferior physique seen in this country suggests that there are some universal errors in dieting. It is the fashion to attribute

such poor physique to various diseases, but personally I doubt whether a simpler cause could not be found. English cooking is notoriously poor, and we have the habit of boiling vegetables, and even fruit. The French seldom eat cooked fruit of any kind, and no one knows why we are addicted to such dishes, in which nearly all the invaluable vitamines of the fruit are probably destroyed—causing waste of money in the purchase of the fruit and in the cost of cooking. Cooking reform may possibly be a much more important reform than any one advocated by our politicians—if only we could get the people to attend to the matter. Of course much is written on the subject, but apparently without marked effect on the habits of the people.

British Microscopes and Microtomes, and other Notes

We have recently been shown some very excellent British-made microscope stands. These stands are being manufactured to a standard type by several of our leading makers, such as Watson, Baker, and Beck.

The "Standard" type of microscope stand is admirably designed, and can be used for all purposes such as marine work and delicate cytological study. The stage will take a large petri dish, or a fine mechanical stage; the fine adjustments in both the Watson and the Beck models (we have not seen Baker's) are very good.

Scientific workers are urged to inspect this new British model before buying foreign microscopes. Watsons, we understand, have spent a great deal of money in fitting out a factory for the making of "Standard" microscopes, and in a short time will be able to turn out large numbers cheaply (that is if the labour troubles subside).

On the other hand, we hear that Leitz & Co. are prepared to lose £30,000 in order to get back their British pre-war trade. Leitz microscopes which we have lately examined are certainly not up to pre-war standard, while the British microscopes have improved wonderfully.

The Cambridge Instrument Co. have lately produced a new "Universal Microtome," which was recently exhibited at the Royal Microscopical Society. This model embodies most of the peculiar and efficient mechanical devices of the older model, but is really quite differently arranged. It is in fact the only microtome of its kind. We have not yet used one of these new microtomes, but the mechanical arrangement of the parts is most original and theoretically perfect. The microtome is selling somewhere about £20.

Just before going to press we received a little catalogue giving an interesting description of Watson's new factory for the manufacture of their "Service" Microscope. The pamphlet contains photographs of the instrument at various stages of manufacture. A new "Service" microscope which we saw recently at the Imperial Cancer Research Bureau impressed us very much. We wish the makers the best of good fortune in their new project.

The Report of the Food Investigations Board, of the Department of Scientific and Industrial Research, for 1919 has come into our hands. Some most interesting new facts of general interest have been ascertained, and the Report indicates great activity—successful activity, we might say—on the part of Research Members of the various sub-committees. The greater part of the research work has, during the year under survey, been devoted directly or indirectly to the preservation of food by cold. The investigations have been somewhat hampered by lack of suitable freezing plant for experimentation.

The Meat Committee have completed some interesting experiments upon the freezing of beef. It is a remarkable fact familiar to the industry that whereas mutton can be frozen without damage, beef cannot. The effect of

freezing in the latter is so to alter the muscle substance as to cause the meat, on thawing, to exude a fluid rich in nutritive material and coloured with haemoglobin. The new experiments of the Meat Committee have shown that, provided the beef is frozen *quickly enough*, the damaged effect ordinarily present on thawing is avoided. Whether such experiments can be applied on a commercial scale is rather doubtful, but further work will be undertaken when the new Research Station has been completed.

The writer of these notes, an Oxford man, has been disappointed to observe that the new Research Station is to be erected at Cambridge. The scientific staff of the University of Cambridge always seems to show a more active spirit than that of Oxford ; the excuse offered by some Oxford men, that Oxford is not the centre of scientific work in England, is pure nonsense, and is merely a whitewash calculated to cover the want of acumen and activity of many of the Oxford professors. Why should the various work and money of Government Departments always be snapped up by Cambridge ?

The Fish Preservation Committee has also been very active. Two Research members, Mr. G. Adair and Mr. J. Piqué, have worked extremely well at Billingsgate Market. Unfortunately the experimental plant lies in a dark, dank, and noisy basement, and both workers became ill, but are to be congratulated on having unearthed some extremely valuable facts.

An Engineering Committee has been at work in the form of three separate Sub-Committees, and has been investigating the scientific side of refrigerating agents, insulation and hygrometry.

A Fruit and Vegetable Committee is attacking such problems as how best to store strawberries, pears, plums, and apples. Indeed we feel that the whole Food Investigation Board is to be congratulated on the useful work which has been carried out. We believe that many of the results will prove of use to British industry.

Some interesting material has recently been brought forward with regard to fertility in the human species. Prof. C. B. Davenport, writing in the *Journal of Heredity*, describes a remarkable case of a Mrs. W. G. C. The proposita Mrs. C. has been interviewed at different times by three representatives of the Eugenics Records Office ; to each the history given has differed a little in details. Mrs. C. married her first husband, and by him had twins ; the husband died fourteen months after their marriage. Two years subsequently the proposita married again, and had twins, Violet and Clay. Violet married and had a single child, but two years later twins, who, with the mother, died shortly after birth. To return to the proposita, Mrs. C., the next children were triplets, named Esther, Flossie, and Theodore—these all died young. Two years after there were again born twins, Anne, who still survives, and Charles, who died young. Next year another pair of twins was born, Irene, who still survives, and Clarence, who died young. The second husband died, and the proposita married again—by the third husband she had first a pair of twins, one of whom died of hydrocephalus, and the other died later ; then in the next year triplets were born, two stillborn and one who is now alive. A year later the proposita had a miscarriage of triplets. The proposita now appears to have had a rest of two years, but in 1907 was hard at it again, for in this year she had twins, a boy still living, and a girl who died an hour after birth.

Then in the following year she had a miscarriage of quadruplets, two boys and two girls—this seems to have been brought on by the poor health of the mother. In 1909 there were born twins, a boy still living and a girl, who died at ten days of age. In the next year triplets were born—a boy Ronald still lives, one of the girls died at the age of one week, and the other was born and died abnormal.

In 1912 the proposita gave birth to four girls in the month of March, and in December of the same year she had a miscarriage at about five months' time caused through a severe burn. In April 1913 the proposita had another miscarriage of three girls.

It will be seen that the proposita has averaged nearly three children at a birth, and has had thirty pregnancies within twelve years in the last mating, in addition to eleven by the two former matings. The foregoing remarkable case is by no means unique, but accords very well with similar cases that are reported by Gould and Pyle in *Curiosities of Medicine*.

Special efforts are being made in Australia to cope with many of the injurious parasitic animals. We notice in *Science and Industry* for June 1920 an account by Prof. T. Harvey Johnston of the Bionomics of the Cattle Tick and of certain parasites of horses in Australia. There are several ticks which may infest cattle, but one especially, *Boophilus australis*, is particularly identified with cattle. The tick pest obtruded itself upon public notice in Australia about forty years ago in the Northern Territory. It then spread to Queensland, and in 1906 it had reached the New South Wales border. Its most southern limit is marked by the Richmond River. It is also found in the north-west of Western Australia. It has been estimated that during the six years from 1894 to 1900 Australian cattle breeders lost £3,500,000 sterling. As is well known, the cattle-tick Boophilus causes bovine Piroplasmosis.

The British Thomson-Houston Company has decided to establish two scholarships, one of which will be allotted to Cambridge. It proposes to select from the engineering graduates of that university who have worked with the firm for not less than six months a scholar who will be sent to their American associates, the General Electric Company. The company proposes to allow for the student's expenses for one year an equivalent of $1,800 dollars. After a year's study in America he will be expected to return to the British company.

Dr. C. C. Little, of the Carnegie station for experimental evolution, in a recent paper on the *Journal of Genetics*, 1919, has contributed an interesting paper on colour inheritance in cats, with special reference to black, yellow, and tortoiseshell. He criticises the hypotheses of Doncaster, Ibsen, Whiting, and Wright which have been brought forward to explain the occurrence of tortoiseshell males. He considers that the genetic constitution of the normal colour varieties of cats is as follows: B = factor producing black pigmentation, Y = factor which restricts black from the coat, y = factor allelomorphic to Y and hypostatic to it, allowing black pigment to extend to the coat.

Sterile tortoiseshell males may possibly be, according to Little, "near males" formed as a result of non-disjunction of the X chromosome and therefore YBX in constitution.

We have received the annual report of the Director of the Department of Experimental Evolution of the Carnegie Institute of Washington. In some interesting researches by Dr. E. C. MacDowell data have been collected on the intelligence of alcoholised rats. It is of some interest to note that Dr. MacDowell believes that the alcoholised strains are mentally inferior, but he states that much detailed study will be required before any generalisation can be drawn. In the same Report Dr. Ezra Allen, of the Wistar Institute, has found that in alcoholised rats there is more testicular degeneration than in normal rats. The type of degeneration found is much the same as that produced by the X-ray.

We are sorry to hear of Prof. Leonard Doncaster's untimely death, which could only be referred to very briefly in the last number of Science Progress.

Prof. Doncaster, F.R.S., was one of our most distinguished cytologists and Mendelians, and his books, *The Determination of Sex* and *Cytology*, are well known. Doncaster possessed that valuable gift of interesting his students and friends in his subject. His teaching and writing have been largely responsible for a creation of interest in the cytological study of animal breeding and Mendelism; most of the younger cytologists in this country have been pupils of Doncaster, in some way or another.

Prof. Doncaster's death again brings us face to face with the fact that British Zoological Laboratories are not now so well stocked with promising men as they were twenty years ago. Unless more and better men undertake the study of zoology, we have no hesitation in saying that in future vacated chairs will have to be filled with mediocrities (the process has already begun) or we shall be obliged to import foreigners.

Possibly the amelioration of conditions of university teaching might induce more of the better-class students to study pure sciences like Zoology and Botany. At any rate one cannot fail to notice that the zoological horizon is rather devoid of budding Lankesters and Batesons.

We can see no justification for the advancement of men simply because of seniority. The professorial chair needs more than seniority for its fruitful occupation; a man who has done nothing and knows nothing, and who is third-rate, will not be able to attract around him workers, except third-rate men. His department will be an inferior one just so long as he occupies it.

In another page of this volume we have inserted a review by Dr. Oscar Brady, on Schrenk-Notzing's late and much-discussed work. We cordially agree with Dr. Brady's attitude towards the printing of such valueless material in these difficult times (page 483).

Some time ago we received a book for review entitled *Sex, the Key to the Bible*, by Sydney C. Tapp, Ph.B. Mr. Tapp has apparently been very active as a writer of what we have little hesitation in calling pornographic commentaries on the Holy Bible. The author states on the fly-leaf of his book that his production is only sold to adults. We notice that the book has been published by the author himself in Kansas City, Mo., and we do not wonder at this fact, because we feel sure that no decent publisher would handle such truck. We cannot understand how any man can write such disgusting words under the cloak of religion, and prefer not to review this book in our pages.

We understand that the United States authorities have prohibited the sale of Dr. Marie Stopes's books in America. While we do not for a moment question the wisdom of any of the measures taken by our American friends, we do wish that they would display a like activity with reference to Mr. Tapp's pornographic work published in Kansas City. We do not think that the importation into our clean country of dirty books published abroad can be for the welfare of the nation.

Report on the Proceedings of Section A, British Association, Cardiff, August 1920 (A. O. Rankine, O.B.E., D.Sc.)

The proceedings of this section were this year of exceptional interest, and reflected the rapid and important progress which has been made recently in physical science. All those present were deeply impressed by the excellence of Prof. Eddington's presidential address on August 24. Contrary to expectation he did not deal with Relativity, but chose for his subject the "Internal Constitution of Stars." Nothing could have been more fitting than the way in which recent stellar theory was brought into line with the most modern developments of physical-laboratory observations, concerning which details were subsequently given by Dr. Aston and Sir Ernest Ruther-

ford. The address was so full of pertinent and illuminating material that it is most difficult to treat in the cursory manner necessary in this article. Prof. Eddington first showed that we are more likely to get information from the giant stars, on account of their very low density. Such a star is to be regarded as a vast storehouse of heat, of which, however, a large proportion exists as radiant energy imprisoned by the particles. It is only those ether waves which succeed in escaping through the material which represent energy lost by the star. The next point dwelt on was the effect of the *outward* radiation pressure, due to the non-escaping radiation, on the equilibrium of the star. Prof. Eddington then proceeded to show how to calculate the absorption coefficient of stellar material, and announced the conclusion that the penetrating power of the radiation is much the same as that of X-rays. Other conclusions arrived at are that all giant stars lie between limits of mass in the proportion 3 : 1, and that it is possible to calculate the periods of variable stars, with results in fair accordance with experience. It is possible also to apply the theory to the estimation of angular diameters of stars, from direct consideration of their surface brightness. It is hoped shortly to compare these estimates with measurements which the Mount Wilson telescope seems likely to be capable of performing. Perhaps the most interesting portion of the address was that in which Prof. Eddington dealt with the source of the energy of stars. He threw over very definitely the contraction-by-gravitation theory. A quotation will be appropriate. "Only the inertia of tradition keeps the contraction hypothesis alive—or rather, not alive, but an unburied corpse. But if we decide to inter the corpse, let us frankly recognise the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the subatomic energy which, it is known, exists abundantly in all matter. . . ."

Prof. Eddington believes that the energy is supplied during the aggregation of hydrogen atoms to form other elements, and regards as significant the discrepancy from the ratio 4 : 1 found in the masses of helium and hydrogen atoms. The deficit represents the mass of the electrical energy set free when hydrogen transforms to helium. This is the only discrepancy yet found by Dr. Aston, but with more refined measurements others may soon be detected. Prof. Eddington appeared at the end of his address as an apologist for speculation—a rôle which he filled with great brilliancy. His arguments were a delightful mixture of truth and fun.

Two interesting astronomical papers followed. MR. J. EVERSHED read a paper on the *Measures of the Shifts of the Frauenhofer Lines and their Interpretation, particularly with relation to the Einstein Theory*. The results of his extensive work at the Kodaikanal Observatory, India, must still be regarded as inconclusive in relation to the theory in question. Mr. Evershed referred especially to his elegant method of observing the lines in the sunlight reflected from Venus. In this case he finds the resultant shifts which are observed in direct sunlight to be absent. This is in opposition to Einstein's prediction, which would apply equally to light leaving the sun in all directions.

SIR F. DYSON gave a short description of the new star discovered by MR. DENNING in Cygnus on August 19. The magnitude was then increasing, and the spectrum was the same as that of α Cygni. On August 26 PROF. A. FOWLER was able to extend the information by exhibiting slides of the spectrum taken by Dr. Lockyer on August 21, and subsequent days. The development of the spectral arrangement appeared to be following the same course as Nova Aquilæ (1918).

The rest of the morning was occupied by mathematical papers by PROF. H. HILTON and MR. T. C. LEWIS.

On August 25 the chief papers were by DR. F. W. ASTON and SIR ERNEST RUTHERFORD. Dr. Aston gave a lucid description of his positive ray measure-

ments of the masses of various atoms, illustrating his paper by excellent slides. Many elements were shown to consist of two or more isotopes, *i.e.* atoms having the same atomic number (or nuclear charge) but different masses. Neon, for example, has at least two isotopes of masses 20 and 22 respectively; krypton has six, and bromine, although its atomic weight as determined chemically is almost exactly 80, is really a mixture in nearly equal proportions of isotopes 79 and 81, thus containing *none* of mass 80. Many new results may be anticipated in the near future.

SIR E. RUTHERFORD, in his paper on the *Building up of Atoms*, recapitulated his recent Bakerian lecture, showing how it was possible to eject hydrogen atoms from nitrogen by α-ray bombardment, and to produce by similar methods from nitrogen and oxygen a new atom of mass 3 and nuclear charge 2—an isotope of helium. He then proceeded to indicate how, possibly, the central nuclei of various elements are built up of aggregations of elementary positive charges and negative electrons. The fundamental positive charge he regards as the nucleus of the hydrogen atom, *i.e.* a hydrogen atom with the outer electron removed. [Sir O. Lodge subsequently suggested that an entity of such importance should have a name and suggested "proton." This suggestion will be adopted in what follows.] A proton with an electron *closely* attached to it would form an undiscovered element of mass 1 and zero atomic number. Such an element would be likely to evade discovery, since, having no attached electron at a comparatively great distance, it would pass readily through all materials, and it would give no spectrum. By suitable additions of protons and electrons to this nucleus, it is possible to construct models having the correct masses and atomic numbers of the elements. Thus four protons and two electrons would give the correct nucleus for ordinary helium, whereas three protons and one electron would fulfil the conditions necessary for the helium isotope already referred to. The latter, according to Sir Ernest's model, is a very stable arrangement, and likely to survive in the disintegration of nitrogen and oxygen nuclei by α-ray bombardment. Sir Ernest insisted that speculation on this subject should not be allowed too great freedom, but it appears likely that very important developments may be expected shortly.

PROF. R. WHIDDINGTON read a paper on *The Ultramicrometer*. This consists of a parallel plate condenser the variations of capacity of which are observed by means of a heterodyne arrangement of oscillatory triode valves. Variations of distance of only a two hundred millionth of an inch between the plates are detectable by means of the change in the pitch of the "beat" note. SIR O. LODGE and MR. F. E. SMITH criticised the results in relation to their applicability to measuring actual distances, and the author replied, indicating that great precautions were necessary in maintaining constancy of temperature of the condenser, and of the frequencies of the oscillating circuits.

Astronomical papers followed by LT.-COL. F. J. M. STRATTON on the *Spectra of Nova Aquilæ III*, and FATHER CORTIE on *Comparison of Drawings of Solar Faculæ and Photographs of Calcium Flocculi*.

On August 26 the greater part of the session was occupied by a discussion on the *Origin of Spectra*. This was opened by PROF. A. FOWLER, who gave a general outline of the various types of spectra obtained from the elements and their representation by well-known formulae. The series into which the lines fall were well illustrated by slides. PROF. J. W. NICHOLSON followed with a description of Bohr's theory of atomic constitution and the production of spectra. To this theory, with its subsequent modifications, particularly by Sommerfeld, he has become converted, and he indicated the numerous ways in which the theory has received support in observations. PROF. J. C. MCLENNAN added further evidence in favour of Bohr's theory by reference to his own measurements of monochromatic spectra produced by electron bombardment. The amounts of electron energy required for this purpose bear

out the quantum theory. The note of criticism was sounded by PROF. W. L. BRAGG, who drew attention to the remarkable evidence derived from the X-ray examination of crystals in favour of Langmuir's theory of atomic structure. This theory demands electrons in stationary or nearly stationary positions with respect to the nucleus, and is thus fundamentally opposed to the Bohr view. DR. A. E. OXLEY laid stress upon the fact that the Bohr theory does not fit in with the magnetic properties of hydrogen. This point was dealt with more fully in his subsequent paper. MR. C. G. DARWIN dwelt on the difficulties in the way of reconciling the rival theories of Bohr and Langmuir, but was hopeful regarding ultimate agreement.

The Report of the Seismology Committee was then taken. MR. J. J. SHAW described the work which has been proceeding at Oxford, particularly in relation to the almost continuous occurrence of Microseisms, or small tremors of the earth. First steps have been taken in order to measure their rate of propagation and to elucidate their origin.

SIR O. LODGE read a paper entitled "Controversial Note on Popular Relativity," and in the discussion which followed MR. A. A. ROBB and the PRESIDENT took part. Neither side was, however, convinced.

On the last day of the meeting, August 27, there were numerous papers.

PROF. S. CHAPMAN read a paper on *Terrestrial Magnetism* and kindred subjects. He showed how both the sun and the moon, by producing atmospheric tides, give rise to air currents, which cut the earth's vertical magnetic field and produce electromotive force. When the air is ionised currents are developed, leading to changes in the magnetic field. Both lunar and solar effects are diurnal, and theory fits in well with observation. Magnetic variation is greatest at periods of the year when the sun's altitude becomes great, in which case there is greater ionisation. This type of ionisation is due to non-corpuscular radiation. In magnetic storms, however, it appears that the ionisation is due to corpuscular radiation proceeding from definite spots on the sun, the storm recurring with the sun's rotation on its axis. The theory also gives an explanation of aurorae.

The next paper was by DR. A. E. OXLEY on *Magnetism and the Structure of the Atom*. The author pointed out that hydrogen is found experimentally to be diamagnetic, although, according to Bohr's theory, the hydrogen molecule in the liquid state should be paramagnetic to the same extent as nickel at ordinary temperatures. He therefore regards Bohr's theory as radically wrong, and proposes an atomic structure in which the electrons execute small local circuits, the atoms being bound together in molecules by eletromagnetic, instead of electric, attraction. On this view it would be unnecessary to depart from Newtonian mechanics. PROF. A. W. PORTER, in the discussion, pointed out that the production of spectra must not be left out of account. The correct solution will be one which is consistent with *all* the experimental evidence.

MR. A. T. DOODSON gave the *Report of the New Committee on Tides*, dealing with the work in connection with tidal prediction at the Liverpool Tidal Institute. Apparently the Kelvin tide-predicting machines do not give the accuracy demanded by modern shipping. These demands are for an error no greater than 3 inches. Actual variations amounting to 1 foot occur, particularly in shallow waters, and the origin has not yet been detected.

MR. J. H. SHAXBY read a paper on *Vapour Pressures*, in which he indicated a modification of Dieterici's equation which, without the addition of a new constant, gives a closer approximation to observation.

There were also papers by DR. P. V. WELLS on *The Thickness of Stratified Soap Films*, and by MR. H. P. WARAN on *A New Type of Interferometer*, an ingenious modification of the Lummer plate, the glass slab being replaced by a layer of water or other liquid floating on mercury.

The proceedings terminated with a vote of thanks to the President.

British School of Archæology in Egypt (Prof. W. M. Flinders Petrie, F.R.S., LL.D.)

The work of the British School last winter has explained further the ethnology of early Egypt. A series of graves were found of the 1st dynasty; they were of many different types, merging one into another—open pits, or with chambers, stairways, or portcullises. Yet the pottery and stone vases proved that all of these were contemporary. This shows, therefore, a vision of several different ancestral customs in the dynastic invaders, due probably to their being a group of allied tribes in different stages of habits. Another matter was illustrated by the different stature of the bodies in the open pits (which are the prehistoric type) compared with those in the more complex tombs of the invaders. The variant examples in each group scarcely overlap at the extremes, the stature of the later people being about four inches shorter than that of the earlier.

This accords with results found six years before in a cemetery of the two races, just before the 1st dynasty. There the female long bones closely agreed in variation with the probability curve, while the male bones showed a marked secondary group, as a hump on the main group. By taking out all the numbers of all the skeletons within the limits of the hump region on each curve, and then seeking what numbers occurred in all of these regions, the individuals composing the secondary group could be distinguished. They were 4% of the male population, and were about four inches shorter than the earlier people whom they had conquered. The leg bones were not distinctive apart, but showed grouping clearly when femur and tibia were added together. This suggests that growth takes place indifferently on either bone at the knee, and the joint is the unit for growth. The skull-measures did not show any distinct grouping by which they could be separated. The details, and photographs of the skulls, are published in *Tarkhan II*.

The conclusion is that the later prehistoric people were about 69 inches high; they were peacefully penetrated by shorter settlers, who thus reduced the stature to 67½ inches; lastly, the invading race broke in, amounting to 4% of the males and only 63½ inches high, conquering a long decadent people by sturdy ability. Later, the influence of the older stocks and the climate restored the taller stature. The conquerors were a mixture of related tribes in different stages of development. These results are only a small part of the work of the School, which is mainly engaged in tracing the cultural civilisation in all periods, and training students for future work.

Dinner to the International Commission for Weather Telegraphy

On November 26, 1920, the Majaraj Rana of Jhalawar gave a dinner to the International Commission for Weather Telegraphy at Bailey's Hotel. His Highness remarked that the war has done one good thing at least, and that has been to give a great impetus to the improvement of aircraft, through which air-conditions may be better studied nowadays. Another good it has produced has been to encourage international co-operation and co-ordination for advancing investigation, not for the destruction but for the preservation of the happiness of mankind. His Highness welcomed the delegates, representing several nationalities, and mentioned especially the veteran meteorologists M. Angot and Captain Ryder, and delegates from France, Italy, Holland, Spain, Scandinavia, Switzerland, and Iceland. He then proposed a toast to the President of the International Meteorological Committee, Sir Napier Shaw, F.R.S., and described the distinguished work which Sir Napier had done and some of the work of the Meteorological Office; and said for himself that those who have looked at weather charts and failed to make head or tail of them might realise that " it is not an easy job to penetrate into these secrets "—with which many will agree.

In replying, Sir Napier Shaw said that the Maharajah's kind references aroused a number of reminiscences. He remembered Maxwell bringing Stokes to arrange about some experimental work, for the then newly formed Meteorological Council, at the Cavendish Laboratory forty years ago. He remembered his first attendance at a meeting of the Council in 1880: Henry Smith, Warren De la Rue, Captain Evans, Francis Galton, G. G. Stokes, and Richard Strachey, a most powerful body of scientific directors. He remembered an occasion in Cambridge in 1897 when the members of the Senate assembled in Senate House Square were being pelted by undergraduates with flour, eggs, and other missiles as an expression of opinion on the admission of women to degrees. In the midst of the turmoil, Michael Foster used the occasion to notify the wish of the Council of the Royal Society to appoint him a member of the Meteorological Council; the first business at the subsequent meeting of the Council was a resolution of protest against the appointment on the ground that the Royal Society had not consulted the Council. He remembered a meeting in Paris in the small chamber at the very top of the Eiffel Tower in 1900, a doleful year for Englishmen in Paris, where and when he was elected a member of the International Meteorological Committee in succession to Dr. R. H. Scott, who had been secretary of the Committee from the beginning in 1873. He remembered another meeting in Paris in 1907, when Mascart, president of the Committee, was very ill and unable to preside, and he had the awkward duty of taking the chair at his own election as president, with a consequential understanding that it must not happen again.

And there were other reminiscences. He was old enough to have taken part in the work of the Quarterly Weather Report, that great effort of the first Meteorological Committee of the Royal Society which explored the phenomena of travelling cyclones, and upon Galton's initiative and resourcefulness issued facsimile reprints of the autographic records of seven observatories for twelve years, probably the most stupendous piece of meteorological work ever undertaken, and now perhaps unknown to the meteorologists of the present company. He remembered also the great effort of the thirteen months of daily charts of the Atlantic for the circumpolar year 1883, which was intended to disclose the secrets of the travel of storms across the Atlantic, and has had the opposite effect of convincing us of our abiding ignorance.

He was reminded of the inauguration of telegraphic reports from Iceland in 1907 as the occasion for the Treasury making the first advance in nearly thirty years in the grant for meteorology. The history of the past was conclusive that the science of meteorology was nothing if not co-operative, and that all nations had to contribute to the stock of knowledge. In that connection he recalled the great service of Colonel Chaves, director of the meteorological service of the Azores, in placing at the disposal of the meteorologists of Europe the observations of that important group of Atlantic islands. It was curious that, though the weather was a matter of interest and importance to every inhabitant of the globe from the Pole, if there was anyone there, to the Equator, where Dr. van Bemmelem was in charge, yet we had no organised profession which provided for the study. It was apparent that it must be largely an international profession because all the different aspects must be taken into account.

His most vivid recollection was that of the meeting of the Commission for Weather Telegraphy in 1912, when he was president of the Commission and His Highness did them the honour of attending the meeting. His Highness had many wide educational and scientific interests, and his interest in meteorology, to which he gave expression by inviting the Commission to dinner, was only part of his multifarious activities, and at the same time a striking illustration of the universality of weather study. The speaker called upon the company to drink His Highness's health.

As everyone knows, His Highness Sir Bhawani Singh, K.C.S.I., is a warm lover of science, and the dinner which he gave was much appreciated by all who were present, including many Fellows of the Royal Society. Our own aristocracy appear to be so much engaged on politics and sport that they have no time to think of science and art.

Mount Everest

The Royal Geographical Society have in view a joint expedition with the Alpine Club to explore and climb, or, at any rate, attempt the ascent of Mount Everest, the highest measured mountain in the world. The foremost object of this expedition is the actual ascent of Mount Everest itself; but the mapping and exploration of the whole of its vast massive form is the secondary objective. And it is hoped not only to map and to photograph, but to make other scientific observations covering a fairly large field, the physiological side of the question being one of the most important. Dr. Keller's most recent observations, being his expedition of the present year on the Kamet massif in British Garhwal, are awaited with interest. Owing to political difficulties, at present unsettled with Tibet, it may not be possible for the expedition to start this year, but it is hoped that by next year all will be settled. In all likelihood the first year will be spent in reconnaissance and trials of porters and in establishing suitable bases, and it will probably not be feasible to tackle the mountain until the second year; and even a longer time may be necessary.

The mountain itself lies on the Nepal-Tibet boundaries and no part of it is in British India. Consequently, as Nepal is not open to British travellers, the mountain must be approached via Sikkim and thence through Tibet to the Tingri plain directly to its north. Further, as the plain itself is less elevated and fairly easy of access, it offers very great facilities; the northern face of the mountain has an infinitely drier climate, as it is but little affected by the great monsoon winds and rains.

The Federation of Medical and Allied Societies

We have little space in SCIENCE PROGRESS to deal with events in connection with applied science, but should note that the formation of this new body promises to do a great deal in connection with medicine. On November 5, 1920, it discussed some clauses in the new Health Bill proposed by the Ministry of Health. The burning question is, How are we to continue to run the great hospitals? Owing to the rise in prices of all commodities it is becoming increasingly difficult to find sufficient funds from private benefactors; and Lord Knutsford was even obliged to state that the London Hospital, of which he is the head, will be obliged to close down very shortly. The brutal truth is that many of those who used to subscribe to hospitals are now finding that British working men are, fortunately, so much better off, chiefly because of their vigorous trades-union action, that they can afford themselves to pay for their medical benefits. Of course there is a residuum of pauperism which perhaps cannot pay anything; but we think with many others, that the time has come when patients should pay as much as possible, at least for their maintenance in hospital. It is a moot question whether subscriptions of this kind added to private benefactions would alone suffice to maintain the hospitals, and national or municipal additions to the hospital funds are therefore being suggested by many. This would involve more or less state or municipal control of the hospitals, to which many doctors object—perhaps with some reason. We think that those who cannot pay anything for medical treatment must be paupers, and should be treated as such by the State. Those who are not paupers should subscribe, not only for maintenance in hospital, but also for their medical treatment—just as, we think, every person who is not a pauper should pay income-tax, and a share of municipal rates. The tendency of our pseudo-philanthropists to try to get the "poor" excused

from all kinds of payments, for education and even for feeding their children, as well as for other benefits, seems to us to be a pernicious one which helps to pauperise the whole country. The fact is that most working men can afford to pay for their beer and for rides in trams for short distances, not to mention numerous attendances at the very silliest cinematograph shows; and they can therefore afford to pay for many other things. To a man with a hundred pounds a year, a glass of beer costs as much as a bottle of wine costs to a man with a thousand a year—and a tram ride to the former costs as much as a taxi-ride to the latter. If a man can spend a shilling a day for beer and amusements he can afford the same for medical benefit when he is ill. Moreover, we are by no means so confident of the alleged great success of the voluntary hospitals. It is doubtful whether medicine in this country is as advanced as some pretend; and we have heard the opposite stated by distinguished foreigners. We could mention many facts in support of this doubt, such as the former opposition to antiseptic surgery, to the introduction of bacteriology and parasitology, and to many other innovations in practice and science; and to-day the medical profession is managed, not by its most distinguished members but by quaint people who have never really done anything but who get their names upon committees and acquire wealth and influence by means of a skilfully elaborated "bedside manner." Medical science remains, of course, in the doldrums, in spite of the Medical Research Commission and its considerable funds; and there are a hundred different lines of research which are now being neglected entirely, in spite of grants, learned societies, universities, and scholarships—for the simple reason that "science does not pay." A national hospital system brought into closer contact with the needs of the people might do a great deal; and we are certainly of opinion that the vast body of medical practitioners should have greater power than they now possess—and so should the few men who have really advanced medical science and who are at present conspicuous by their absence from among those who occupy the seats of the mighty in medicine.

Medical Congress Resolution in Brisbane

The recent Australian Medical Congress held at Brisbane, Queensland, spoke emphatically on the policy of a "White Australia" which it strongly endorsed. The main resolution which was carried stated that: "After mature consideration, Congress is unable to find anything pointing to the existence of inherent or insuperable obstacles in the way of the permanent occupation of tropical Australia by a healthy indigenous white race. They consider the whole question of the successful development and settlement of tropical Australia by white races is fundamentally a question of applied public health in modern science, such as has been demonstrated and practised, with success, amongst civil populations, under far more difficult conditions, by the American authorities in the Philippines prior to the Great War, and throughout the military forces of every Allied Power during that war. They consider the absence of semi-civilised coloured peoples in Northern Australia simplifies the problem very greatly, but they desire to emphasise, in the strongest manner, that any considerable extension of population and settlement under the existing loose conditions of sanitary administration and sanitary practice (using these terms in their modern, wider sense) which prevail at the present time in tropical Australia cannot hope for lasting success, and cannot fail to result in ultimate disaster. Congress recognises that a large amount of work still requires to be done in working out the practical details of any scheme of settlement, but they consider it presents no difficulties beyond those of the organisation of the staff, time and money. They realise that a great national question is involved, but they are unable to discern any obstacles which cannot be overcome by the earnest and skilful application of the principles of statecraft."

Dead Darwinism

It is always easy to win a victory by pretending that the other side has already been defeated; and for many years past we have heard public speakers, prelates, pseudo-philosophers, and literary men claiming that Darwinism has now been completely disproved, that the theory of evolution is unsound, and that everything came about in some mysterious manner which each speaker has evolved for himself, as if he himself were the Deity! Quite recently one of our most distinguished literary men has been destroying Darwinism in this way—relying upon the ignorance of his readers. It was therefore with much pleasure that we saw Sir Ray Lankester enter the lists against him fully armed in the manner which has always been such a terror to the opponents of science; and in half a minute the clever writer was rolling in the dust of the arena. In other words, Sir Ray gave him a proper drubbing in the columns of *The Sunday Times* of September 26, 1920. "Far from Darwinism being dead, it is just as living as before, and is not even sick or weakening. It is more firmly approved by those who have adequate knowledge of biology, and by a larger body of people, than it was thirty years ago. It is simply a shameless misrepresentation to say that Darwinism is not in a sound and healthy condition. The wish is father to the thought." The defeated wit now protests from the dust that Sir Ray got angry and hit him too hard! Serves him right.

The Triumph of Education

In October we were informed by many of our learned newspapers that at the Pasteur Institute a microbiologist has examined one-franc banknotes and has found them to be swarming with millions of bacilli like star-fish, lobsters, centipedes, shrimps and grinning hobgoblins in appearance. This was described by our learned press as a terrifying revelation. The great biologist tore off a minute scrap from one of the notes and put it below the microscope. Across the lens "monsters of more hideous form than we ever imagined in a nightmare crawled and jumped. There were some like tadpoles, with feathery tops; some like worms, some like parrots about the beak, some like earwigs, and some like geometric and astrological signs." Of course bacilli have no such shapes, and appear under the microscope as nothing but dots and dashes—to give a curt description. Imagine the state of education in a country in which our principal newspapers can put forth such trash in the hope that it will be believed by their readers. Yet the subject of bacilli is one of life and death to everyone in the country. We are spending millions on education and have a number of great universities and public schools. Is this the final result?

The Encouragement of Science

The Times of October 26 last states that Prof. Max Margules, the eminent Austrian Meteorologist, has died at the age of sixty-five from *starvation*. He had been subsisting on a very small pension and was too proud to beg for assistance. When men give up their pleasant little habit of crucifying their benefactors then they may perhaps begin to emerge from their present state of barbarism.

The Unknown Warrior

The burial of the unknown warrior in Westminster Abbey—the body of an unnamed soldier found in France—was perhaps the most beautiful and poetic tribute which any nation has ever paid to those who have fought and suffered for their country in war; and the event certainly showed that the emotion excited by poetical ideas has not yet died out in Britain. We

understand, from a note in the *Morning Post* of November 13, 1920, that the idea was originated by the Rev. David Railton, M.C., the present Vicar of Margate. It is stated that the thought occurred to him about a year previously; that he wrote to the Dean of Westminster about it; and that the large Union Jack which covered the coffin was the gift of Mr. Railton. It had been used in the war, sometimes as a pulpit cloth and at other times to cover the remains of dead soldiers. The nation is much indebted to Mr. Railton for his beautiful thought.

Such is Fame

The Americans show more appreciation of the services of great men than we do—or rather they recognise that greatness consists not merely in politics and sport. They have long possessed an American Hall of Fame, and we read in November that amongst six new admissions to the roll were Mark Twain, the humorist; A. Saint Gaudens, the sculptor, and W. T. G. Morton, the discoverer of ether as an anæsthetic. There must be much difficulty of selection in these cases. In the great library of Washington there are, or used to be, a number of statues representing the world's greatest men, and one saw those of Shakespeare, Columbus, etc., placed side by side with various Smiths, Joneses, and Robinsons with whose claims humble Europeans were not fully acquainted. But that may be our fault.

The Middle Ages

On October 30, 1920, Sir Herbert Warren, President of Magdalen, Oxford, addressed the Oxford Branch of the Modern Languages Association and enriched our store of beautiful things by a lovely figure describing the so-called Dark Ages. These, he said, were not a period of Egyptian blackness, but were " a long twilight lit by the stars of Cicero, Horace, Ovid, and Lucan, and the moon of Virgil reflecting the sunken sun of Homer "—a most admirable summary of classical literature.

A Word of Appreciation

In our October number we remarked, in connection with some very kindly appreciation of Science Progress which appeared in Japan, that scientific publications seem to be appreciated anywhere except in their own country. The *Literary Supplement of the Times* points out that it has often had occasion to commend this Quarterly. That is true, and we have always been profoundly grateful to that very fine review—perhaps the finest review now being issued. It has reviewed almost every number of Science Progress, and has not been niggardly in its praise of the works of our contributors when these met with its approval. We are also frequently reviewed in the pages of *The Yorkshire Post, The Aberdeen Journal, The Aberdeen Free Press, The Oxford Chronicle, Nature* and other Journals.

But these few swallows do not make a summer; and most of the great dailies and weeklies ignore us completely, though we send them copies on issue. A little while ago we even went so far as to write to their editors and ask them whether they wished to continue receiving review copies of Science Progress. Nearly all of them replied that they did so wish, but, nevertheless, they continue to give us scant notice. Probably the fault does not lie with the editors, but is due to the difficulty of obtaining scientific reviewers for a work which covers so much ground as Science Progress does. Our effort to obtain better recognition has not been made merely on our own account, but is part of the war now being waged to obtain a better place in the sun for science in general.

Impressions of the New Germany

A meeting of the Sociological Society was held on Tuesday, October 26, at Leplay House, 65, Belgrave Road, the offices of the Society, at which, under title of "Impressions of the New Germany," Mr. Raymond Unwin and Mr. A. G. Gardiner (who was in the chair) spoke on the post-war condition of Germany, and two papers on the same subject by Mr. Huntly Carter and Dr. Marcel Hardy (head of the Agricultural Department of the Reparation Commission in Germany) were read. Summaries of papers by Mr. William Mann (late organising secretary to the Society, and now assistant to Dr. Hardy in Berlin) and Professor Foerster (of Munich) were also read. There was a general consensus of opinion amongst both speakers and contributors of papers that the militarist outlook in Germany was no longer dominant. Mr. Unwin and Mr. Gardiner both spoke of the friendly feeling towards England now shown by the Germans. Dr. Hardy dealt in his paper with the good agricultural and economic conditions prevailing in Germany, and particularly in Brandenburg. The latter point was criticised by Mr. Gardiner in his subsequent speech. Both Mr. Gardiner and Mr. Huntley Carter (in his paper) laid stress on the suffering caused, especially to the professional and lower middle classes by the present economic pressure. The bad physical condition of the children, as a result of under-feeding, and the excellent work of the various relief organisations, American and British, were dwelt on by Mr. Gardiner, and were brought up in the subsequent discussion, to which contributions were made by workers in relief organisations. Mr. Unwin suggested that valuable work might be done by the Sociological Society if it could impartially examine the reactions evoked by the war and the Peace Treaty in the late belligerents, with a view to lessening existing bitterness caused by ignorance and prejudice, and thereby assisting the formation of a lasting peace.

The American National Research Council

The Carnegie Corporation of New York some time ago made a gift of $5,000,000 to the American National Research Council and National Academy of Sciences, of which about one million dollars is to be devoted to the erection of a building in Washington to serve as the home of these two closely related scientific organisations. The remainder of the total sum is to serve as an endowment for the maintenance of the Council.

A site for the building, comprising an entire block of land near the present Lincoln Memorial in Potomac Park, has just been obtained at a cost of about $200,000 through gifts from about a score of generous individuals, most of whom are business men associated with great industrial concerns or generally interested in the promotion of American science.

The National Research Council, which was organised during the war to aid the Government in mobilising the scientific resources of America, both in personnel and material, for attack on scientific problems connected with America's war-time activities, has now been reorganised on a peace-time basis as a permanent institution for the promotion of scientific research and the dissemination of scientific information. It is not a government department or bureau, but is privately supported and is wholly controlled by the co-operating scientific men of the country. The major part of its membership is composed of appointed representatives of about forty American major scientific and technical societies. Dr. George E. Hale, director of the Mount Wilson Solar Observatory, is the honorary chairman, and Dr. H. A. Bumstead, Professor of Physics at Yale University, is the active chairman for the present year. Dr. Vernon Kellogg, formerly of Stanford University, is the permanent secretary.

The Percentage of Liars

We have heard that there is a good method extant for estimating the proportion of liars to the general community. A large number of men are now under treatment for a disease acquired during the war. The treatment involves daily dosage of a specific medicine, and the men are told to take it regularly. When they return to see their doctors most of them declare with the most perfect candour that they have taken the prescribed dose; but they are ignorant of the fact that their statement can be immediately tested by a very simple and valuable chemical procedure. It has been found, we understand, that something like 75 per cent. of the men who have been brought to this test have been proved to be liars. Fortunately this does not give the accurate percentage of liars among the population, because it naturally happens that it is only the more doubtful men who are examined; but it shows scientifically that, in spite of our costly system of education, the liar is a very frequent phenomenon.

Eels

Dr. Johannes Schmidt, who has taken up the Eel question for the International Council, was the leader of the recent Danish marine research expedition across the Atlantic, and has discovered the breeding place of the European eel. By means of successive trawls of the larvæ, the region where they were youngest and most abundant was found to be about 27 deg. N. and 60 deg. W., not far south of Bermuda. This interesting discovery has as yet only been briefly referred to in the Press, but Dr. Schmidt's report on the subject will be looked forward to with great interest.

Notes and News

The President and Council of the Royal Society have, with the approval of H.M. the King, awarded Royal medals to Mr. W. Bateson for his contributions to biological science, and to Prof. G. H. Hardy for his researches in pure mathematics. The Copley medal has been presented to Mr. H. T. Brown for his work on biochemistry; the Rumford medal to Lord Rayleigh for researches into the properties of gases at high vacua; the Davy medal to Mr. C. T. Heycock for his work on the composition and constitution of alloys; the Darwin medal to Prof. R. H. Biffen for his work on the application of scientific principles to plant breeding; and the Hughes medal to Prof. O. W. Richardson for his work on thermionics.

The following is the list of the new Council of the Society: *President*, Prof. C. S. Sherrington, M.A., M.D., Sc.D.; *Treasurer*, Sir David Prain, C.M.G., C.I.E., M.A., LL.D.; *Secretaries*, Mr. W. B. Hardy, M.A., and Mr. J. H. Jeans, M.A.; *Foreign Secretary*, Sir Arthur Schuster, Ph.D., Sc.D., LL.D.

Other members of Council: Mr. J. Barcroft, C.B.E., Sir William Bragg, K.B.E., Dr. A. W. Crossley, C.M.G., Prof. J. B. Farmer, M.A., Sir Walter Fletcher, K.B.E., Prof. A. Fowler, Dr. A. C. Haddon, M.A., Sir Robert Hadfield, Bt., D.Sc., Sir Thomas Heath, K.C.B., K.C.V.O., Prof. J. Graham Kerr, M.A., Prof. H. Lamb, M.A., Sir William Leishman, K.C.M.G., C.B., Dr. S. H. C. Martin, Prof. J. W. Nicholson, M.A., Mr. R. D. Oldham, and Prof. W. P. Wynne, D.Sc.

Sir Edward Thorpe has been elected President of the British Association for the 1921 meeting at Edinburgh. For 1922 the Association has accepted an invitation to visit Hull, and 1923 will, most probably, see it in Canada.

Dr. A. W. Porter, F.R.S., has been elected President of the Faraday Society for the current session, and Dr. R. Knox, M.D., President of the Rontgen Society.

Sir John Bretland Farmer, F.R.S., of the Imperial College of Science and Technology, has been appointed to be a member of the Advisory Council for Scientific and Industrial Research.

Sir F. W. Dyson has been elected honorary member of the American Astronomical Society.

Dr. G. E. Hale, director of Mount Wilson Observatory, succeeds the late Lord Rayleigh as a foreign member of the Società Italiana delle Scienze, Rome.

The Canadian Council of Scientific and Industrial Research has awarded $5,000 to Prof. J. C. McLennan of the University of Toronto for his researches on helium.

Prof. R. Roux, director of the Pasteur Institute of Paris, has been awarded the Distinguished Service Medal by the Government of the U.S.A. in recognition of the importance of his work to the American Expeditionary Force.

The following well-known scientific men have passed away since the notes were last written :—Dr. H. Alsberg, the anthropologist of Cassel ; Eric Doolittle, Professor of Astronomy in the University of Pennsylvania ; Arman Gautier, Professor of Medical Chemistry at the University of Paris ; Sir W. Mather, the well-known manufacturer and educationist ; K. H. Struve, Professor of Astronomy at Berlin University ; Prof. Wilhelm Wundt, the psychologist.

The list of donations for scientific purposes is much shorter than usual this quarter ; but is, as usual, headed by a gift from the Rockefeller Foundation. Harvard University School of Medicine has received $650,000 from this source ; $350,000 for the development of psychiatry and $300,000 for obstetric teaching. In addition Radcliffe College for Women, which is essentially a part of Harvard, received $175,000 from the estate of the late Miss Annette P. Rogers. The late William K. Vanderbilt left $500,000 to Vanderbilt University and the University of Buffalo has received a gift of $400,000 for the erection of a chemistry building from O. E. Foster. American munificence to places abroad is represented by the sum of 100,000 francs. given by Mr. M. D. Flattery to the Institute of Bacteriology at Lyons to found an annual scholarship for a student who will specialise in laboratory work on the bacteriology of infectious diseases. Any gifts made by Englishmen to our own scientific institutions have unfortunately escaped our notice, indeed the only record is decidedly negative, for the £100,000 which the shareholders of Messrs. Brunner, Mond & Co. decided to distribute among the university colleges for the use of their chemical departments has been held up by the legal action of one shareholder, who disputes the right of the company to distribute any of its surplus assets in this manner.

The University of Chicago has drawn up plans for securing $10,000,000 during the next five years ; $4,000,000 is required to endow salary increments already given or authorised, while the remainder is to be used to form institutes for conducting " such research and training in pure science as has an immediate bearing on the application of science to industry."

The settlement of the west coast of Greenland by Danish emigrants began with the voyage of one Hans Egede in 1721. To commemorate this event an expedition, under the leadership of Lauge Koch, left Copenhagen on July 15 last for further exploration of the northern part of the country. It was intended to devote the summer months to laying a depot in Warming's Land by means of motor tractors working from Robertson's Bay, the wintering station for the expedition. The main work will be carried out in 1921 with dog sledges. It includes journeys to Peary's Land, and to the north of Adam Biering's Land.

An expedition, fitted out by the Swedish Society of Anthropology and Geography, has started from Yokohama on a two years' trip to Kamchatka. It is hoped to carry out a complete survey of the whole peninsula, including its geography and geology, zoology, botany and ethnography. The district

has flora and fauna which, in their richness and variation, are quite unusual in such high latitudes, but the scientific details are comparatively unknown.

In order that a research programme relating to the physiological effects of cold and low pressure may be carried out, the Department of Scientific and Industrial Research is rendering assistance to Dr. Kellas in an expedition to Mount Kamet in the Himalayas. This explorer proposes to reach a height of 25,000 ft. or so with the aid of oxygen mountaineering outfits, and he is being provided by the Department with the necessary equipment for his research

It is reported in the daily press that Amundsen has been forced to leave Nome in Alaska for the north with a crew of three and an Eskimo cook. The other members of his crew struck for a wage of £300 a month!

The centenary of Oersted's discovery of the magnetic field associated with an electric current was celebrated at Copenhagen on August 31 and September 1 by anniversary meetings at the Town Hall and University. His experiments would seem to have been made in the winter 1819–20, in the fourteenth year of his appointment as Professor of Physics in the University of Copenhagen and seven years after he first turned his mind to the subject. His paper describing the work bears the date July 21, 1820, and recognition of its importance was immediate, for he was elected For. Mem. R.S., and awarded the Copley medal four months later. It is recorded also that Ampère, having heard of the matter on September 11, 1820, published the first of his classical papers on electromagnetism on September 18.

The serious cost of scientific publications, which has already been referred to in these pages, is dealt with very sympathetically by the Scientific and Industrial Research Committee. It is suggested that the societies should take common action by placing before the Government a definite statement of the extent to which the national interest is made to suffer by the present increase in the cost of printing. In Sweden government aid has already been provided. For example, the Swedish Medical Association is to receive 5,000 crowns towards the cost of publication of its three journals (a fortnightly, a quarterly, and the *Transactions*). Three journals devoted to hygiene are given from 1,000 to 1,500 crowns and four specialist journals from 500 to 1,200. In return each journal has to donate a number of copies to the university libraries.

We have received a cutting from the *Karnataka* (July 24, 1920) referring to the appointment of a successor to Sir Alfred Bourne, Director of the Indian Institute of Science, Bangalore, from which it would appear that the appointment was to be given to an English administrator, who has no experience whatever of scientific work. Such an appointment in the leading scientific institution in India is really deserving of the strongest protest, but is, after all, only one more example of the incompetence of our classically trained bureaucracy. In this particular instance, however, there is evidently more involved than the question of official *v.* scientific administration, because it is stated that the European professorial staff was in favour of the proposed appointment. In the absence of any information concerning the local conditions, all that can be said is that it is very regrettable that the scientific staff could not agree to support the cause of a more appropriate candidate.

In a letter to *Nature* (September 30, 1920) Messrs. J. N. Brönsted and G. Hevesy of the Polytechnic High School, Copenhagen, state that they have succeeded in separating from ordinary mercury two isotopes, one having a specific gravity ·999980 and the other 1·000031 as compared with mercury as standard. This separation was obtained by evaporating mercury at a low pressure and condensing the evaporated atoms on a cooled surface. The rate of evaporation is inversely proportional to the square root of the atomic weight of the isotopes, so that a partial separation should be obtained. The densities were measured with a pyknometer, the accuracy claimed being greater than one in a million.

A. W. Hull of the General Electric Co., Schenectady, N.Y., gives in *Science*

(September 3, 1920) a summary of his most recent work on the structure of the metals. A narrow beam of monochromatic X-rays passed through the powdered metal produces a pattern of fine lines on a photographic plate due to the reflexion of the X-rays from the faces of the crystals—one line for each kind of face. From the positions and intensities of these lines the positions of the atoms in the metallic crystals can be calculated. The results show that the atoms of calcium, palladium, and iridium, like those of platinum, have a face-centred cubic arrangement. Titanium has a centred cubic arrangement like chromium and iron; tantalum one like tungsten. The structures of cadmium and zinc are hexagonal, close packed, and elongated in the direction of the hexagonal axis; this corresponds to a close-packed arrangement of prolate spheroids. The atoms of iridium are arranged in a face-centred tetragonal lattice elongated 6 per cent. in the direction of one of the cubic axes, so that it, also, is a close-packed arrangement of prolate spheroids. The structure of ruthenium resembles that of cadmium and zinc, except that its lattice is shortened where that of the others is elongated. It is thus a close-packed arrangement of oblate spheroids.

A second note in *Science* (September 24) by D. M. Dennison, writing from the same laboratory, gives details of the structure of ice-crystals. No details as to the experimental method are supplied, but it is stated that the results indicate that the molecular formula for ice is H_4O_2 or $2(H_2O)$.

Col. W. B. Greeley, the new chief forester of the U.S.A., who has lately completed an inspection of the natural resources of Alaska, is reported to have said (*Science*, September 10, 1920) that that country could supply pulpwood for the annual manufacture of 1,500,000 tons of paper in perpetuity. This is about one third of the present consumption of paper in the States, and approximately equal to its import from Canada. In view of the present cost of paper, it is to be hoped that the Alaskan supply will be tapped forthwith, and that some of the Canadian surplus will then reach this country at a reasonable price.

The Canadian Commission of Conservation has issued a long report on the Water Power of British Columbia, from which it appears that the surveyed sites in that Province would yield no less than 3,000,000 horse-power, while there are, in addition, large areas virtually unknown and considerable possibilities of water storage.

Another report on Power in Alberta contains an account of the enormous coal deposits in that district. It contains about 87 per cent. of all the coal in the Dominion (estimated at over a million million tons—Great Britain about 200,000 million tons). Unfortunately, three-fifths of the Alberta deposits are of the lignite type; but, even so, the supply of good coal in Alberta exceeds that in this country. Incidentally it may be noted that the U.S. Bureau of Mines has completed plans for co-operative research on the carbonisation of lignite, a fund of $200,000 having been provided for the erection of the necessary plant in N. Dakota.

Special Report No. 3 from the Fuel Research Board entitled *The Coal Fire* (H.M. Stationery Office, price 4*s*. net.) contains an account of the very interesting results obtained by Dr. Margaret Fishenden from her experiments with ordinary domestic grates, performed at the instance of the Air Pollution Advisory Board to the Manchester Corporation.

The main work was directed towards the measurement of the heat radiated by a fire into the room. This heat was estimated by suitably integrating thermopile measurements taken at different points on the surface of an imaginary hemisphere with its centre at the centre of the fire. The radiant efficiency of the fire was defined as the ratio of the total radiation so estimated to the calorific value of the coal burnt. The results showed that, while the type of grate used is relatively unimportant, yet the old-fashioned grate is definitely more efficient than the modern barless grate, the actual figures

being 24 per cent. for the former, and 19–20 per cent. for the latter. Further, the radiant efficiency is not affected by the draught, though of course more coal is burnt and more heat radiated when the dampers and fender-holes are wide open. The radiant efficiency when anthracite was used instead of coal rose from 24 per cent. to 27 per cent., and with dry coke the figures were slightly higher. Weight for weight dry coke (calorific value 12,800 B.Th.U. per lb.) gave 5 per cent. more radiation than coal (calorific value 14,500). With coke produced by low temperature carbonisation the efficiency increased to 33 per cent., with briquettes it was only 19 per cent., while a good gas fire gives 60 per cent. The patent powders advertised as "doubling the heating value of the coal," etc., had, as might be expected, no effect whatever. They consist mainly of common salt, with small quantities of other sodium and magnesium salts and ferric oxide.

The design of the grate has an important bearing on the distribution of the heat. The greatest intensity of radiation with all the grates tested was found along an upward line, through the approximate centre of the fire, and inclined at an angle of 60° to the horizontal, but with the old-fashioned barred grate, the variation between 20° and 80° with the horizontal is very small. Thus for the general heating of persons and objects situated some distance from the fire the old type vertical surfaced fire is distinctly the best. The report brings out two other points of great importance in the design of fire-places. First that they should always be set on an inside wall, so that the heat conducted through the fire-back helps to warm adjacent rooms. Secondly, that the grate should be as little recessed as possible. The comparative cost of *continuous* heating with coal, gas and electricity is worked out in the paper. Taking coal at 45*s*. per ton, gas at 4*s*. 6*d*. per 1,000 c. ft., and electricity at 1*d*. per unit, electricity is five times, and gas three times as expensive as coal, for the same heating capacity in each case.

The Report of the Metric Committee of the Conjoint Board of Scientific Societies shows, as might be expected, that when the *compulsory* adoption of the metric system of weights and measures is considered as a possible reality the difficulties become very much more serious, and the advantages much less obvious than they appear when depicted by enthusiastic supporters of the Decimal Association. The Committee is agreed that, if an international system is ultimately adopted, it must unquestionably be a metric system; but has to point out that in 1913 no less than 54 per cent. of our foreign trade was with non-metric countries of which the most important are China, the U.S.A. and the British Empire. If a change is ever made it will need to be as the result of an agreement with the latter countries. The British system has grown naturally, if haphazardly, with the needs of its users, and is probably better established in their habits than any modern system can be. Its units are of convenient size and, being based on the duodecimal system with its many factors, are infinitely more convenient in use than those of the metric system. Illustrations of this fact are provided by the binary subdivision of weights in metric countries (*e.g.* the half and quarter kilogram), and by the fractional quotation of prices on the American Stock Exchange in spite of the decimal coinage in that country. The manner in which the British system is commonly used is undoubtedly very clumsy, and there may be confusion in the units employed by different trades, but these defects can be remedied very easily, without introducing the metric system. To these ends the Committee recommends: The abolition of the many unnecessary intermediate units, namely, the league, furlong, and pole; the grain, dram, stone, quarter, and hundredweight of 112 lb.; the peck, bushel, quarter, chaldron, and barrel. (2) The abolition of the whole of Apothecaries' Weight. (3) That all areas should be expressed in acres and decimals thereof or in square feet. (4) That the system should be decimalised as far as possible, *e.g.* that lengths be expressed in miles and decimals of a mile or similarly in

yards or feet or inches, but not in a complex of miles, yards, feet, and inches. As regards coinage, the opinion is expressed that no change should be made in the existing system at the present time

The Report contains an account of a modified system of French legal units awaiting adoption by the French Senate in the early part of 1920. It contains two extraordinary blunders, which can hardly be excused by the fact that they occur in the original text (if, indeed, they do). The first is contained in the definition of a new unit of force—the Sthène. It is a force *which in one second gives a mass of* 1 *tonne* (*i.e.* 1,000 kilograms) *an acceleration of* 1 *metre per second!* The other defines the "unit of density" as "that of a body with a mass of 1 tonne in a volume of 1 metre cubed." A confusion of density and specific gravity hardly to be expected in a report from the Conjoint Board.

The Report of the Committee of the Privy Council for Scientific and Industrial Research for the year 1919–20 (H.M. Stationery Office, 1*s.* net), is so comprehensive that it is only possible here to refer briefly to a few of the more important points. The index alone runs to seven pages of small type. Selecting from it at random, we find as consecutive items such divers matters as ferro-zirconium research, fertilisers, fighting services—boards for co-ordinating research, file-making, fire prevention, fish-freezing . . . etc, a list which gives a fair idea of the wide sweep of the work for which the Department is now responsible. Referring first to finance, it is stated that about £800,000 of the million pound fund placed at the disposal of the Committee will be needed for research associations already formed or in view; £63,800 has actually been used, associations completely established will take £450,000, those approved and waiting licence £120,000; while others in the earlier stages of their formation should require the remaining part of the total. The work of the National Physical Laboratory has so extended that £203,000 has been provided for its maintenance during the current year. Its income in 1914 was only £40,000. The researches now in progress or recently completed at the Laboratory include work on heat-insulating materials for cold-storage work; low temperature hygrometry; research on the lubricating efficiency of various oils; experiments on the lighting of picture galleries and of public buildings and offices; investigations relating to searchlights, ships' navigation lights, miners' lamps and motor car head lights. In addition a building has been erected in the laboratory grounds for the study of problems peculiar to the needs of the Navy. The work of the Fuel Research Station is getting into full swing and covers the whole field of enquiry from oil fuel for the Navy through the design of the domestic grate to the utilisation of Irish peat. A special branch of this Board is dealing with possible methods for the artificial production of motor fuel, but nothing very hopeful has yet come out of it. The Royal Society has assumed responsibility for the annual grant of £200 to the International Commission of Publication of Annual Tables of Constants and Numerical Data, as well as for all the other subscriptions of the Government for all classes of international research. The publication of the tables is proceeding satisfactorily, and plans have been drawn up for the issue of critical revision tables at intervals of five or ten years. The system of grants to individual workers on the personal recommendation of their professors or responsible directors has extended very considerably. In 1916–17 the total sum involved by these grants was only £3,200, in 1919–20 it was nearly £27,000 and for 1920–21 has been estimated at £45,000. The publication of the results of the researches aided in this manner is not now restricted in any way, except in the few cases in which results of a commercial value are obtained. It has been decided to appoint an Inter-Departmental Committee to consider the fairest method of dealing with patents granted for inventions which are the outcome of State-aided research.

CORRESPONDENCE

To the Editor of "Science Progress"

STARVATION PAY OF BRAIN-WORKERS

From F. H. Perrycoste, B.Sc.

Dear Sir,—I believe that I am correct in stating that a raw youth of about eighteen, if possessing a good physique and a fair character, and if normally "intelligent," is started, even whilst under training, in the police force at a pay of £182 per year; and he may rise to the rank of superintendent at a minimum annual pay of £450: and every rank in the police force carries a substantial pension. Incidentally, I have seen it stated that the average annual pay of university professors is about £400; and the provision for pensioning them is, I believe, negligible.

It was recently decided that dockers—who, I suppose, are at the lowest level of unskilled physical labour—ought to receive £250 per year; and a scheme is under consideration for guaranteeing them, *whilst unemployed*, pay at the rate of £200 per year at the expense of the industry.

Let it be remembered that those who become policemen and dockers have been earning wages—in these days possibly or probably more than their cost of living—since they were fourteen.

Now we will turn to the other side of the picture. In a recent issue of *Nature* the University of London advertises for two demonstrators in chemistry at a salary each of £200—equivalent in purchasing power to about £76 in 1913. I presume that such demonstrators will be graduates—*i.e.* that, instead of having earned their living during seven or eight years previously, they have been kept at school and university at very heavy expense to their parents.

I brush aside at once the myth that only rich men send their sons to the universities. In numberless cases the lads are sent there at the cost of grievous self-denial to the parents, and not even as a good pecuniary investment for the lads themselves.

If a man really wants his son to get rich, he must, in many cases, be rather a fool to send him to a university instead of putting him at sixteen into trade or finance, which will give him a living at once and a prospect of affluence later.

There are, however, strange creatures living who regard congenital brain power as not simply or primarily a potential asset for the possessor, but as a sacred trust to be nursed and developed for the good of the community; and these impracticable people, if they descry brains in their children, will seek to educate them to their utmost in order to render them essentially "useful members of society"—as the derided but hallowed old phrase had it. Whether eventually their educated but sweated and starvation-paid offspring will bless them, or otherwise, is another matter.

Now, I do not suggest that at present prices the docker's pay is at all

too much to secure a decent and sufficient livelihood for a married man with children; indeed, I doubt whether it be really enough. Of course, out of the present chaos of wages and salaries must eventually come, I think—and may it come soon!—the extension to all wage-earners and salary-earners of the new Army-system of adjusting standard pay to the needs of a single man (or woman), and making fixed additions for a wife (or dependent sister or parent), and for each child up to a limit to be fixed, as we most fervently hope, on eugenic and Galtonian principles. As things at present are, however, we must argue from the datum of a docker's or policeman's pay to that of a demonstrator or professor of the same age; and on this basis we must admit that a salary of £500 per year would be below the demonstrator's deserts, and that clearly to offer him £200 is a scandal and a crime—a crime against science and learning as well as against the man—of which any governing body of any university should be most heartily ashamed.

Of course, it will be replied that universities and demonstrators alike cannot help themselves; but I suggest that they can. If the university boldly and publicly proclaimed to the country generally, and to the Ministry of Education in particular—"We must have so many demonstrators (*e.g.*) to whom we cannot and will not under any circumstances offer less than £500: we have funds sufficient to pay only £200; unless the balance be made up, we will appoint no demonstrator at all, and the teaching of our students must more or less collapse; we wash our hands of all responsibility unless you find the money"—then I shrewdly surmise that the money would very quickly be found from public or private funds, or both.

In the next place, if the demonstrators and lecturers throughout the country would promptly organise themselves on Trade Union lines—the Association of Science-Workers now provides a machinery—would draw up a full scheme of minimum standard pay based, like the dockers' and miners' and all other manual workers', on expenses of to-day, and would bind themselves individually and collectively to resign at a certain date unless such pay were conceded; if they would collectively and individually bind themselves to regard as a blackleg, and to boycott personally and scientifically, any and every man or woman who should accept such a post at less than standard pay; if they would endeavour to sweep into their organisation all third-year science students at all the universities; I believe that the reform, big as it is, would be carried through speedily. It is nonsense to suggest that highly trained brain-workers, enjoying the advantages of practically a closely-restricted "craft"—since it would take years to train men to displace them, even were any willing to be trained arduously as blacklegs—cannot achieve what uneducated heavily-handicapped workmen have achieved.

If any such scheme of standard pay for scientific workers were so drawn as to discriminate between pay for a single man, pay for a married man, pay for a married man with one, two, three children, the draftsmen of the scheme would set an example of inestimable social value to the whole nation but I admit that this suggestion travels beyond the main record which sets forth that to-day the demonstrator is commercially "worth" 20 per cent. less than the docker. Here we have a lurid blot on the national scutcheon, and on the university quarter thereof. Let those concerned clean away the disgraceful stain at once—for very shame's sake, if from no higher motive.

Yours faithfully,
FRANK H. PERRYCOSTE.

POLPERRO, CORNWALL,
October 14, 1920.

ESSAYS

THE FUNCTION OF THE NUCLEOLUS IN THE LIFE OF THE ANIMAL CELL (J. Bronté Gatenby, B.A., B.Sc., D.Phil. (Oxon), Lecturer in Cytology, University College, London).

THAT an animal body is built up of individual cells, just as a brick house is built up of individual bricks, is a fact familiar to most people. The cells which form our bodies are differentiated in various ways for different functions, but all of the cells of the body are similarly equipped with special cell organs. In all cells one finds a nucleus or central body, containing small elements known as chromosomes, which are generally supposed to be the bearers of the factors of heredity. Outside the nucleus, within the main part or body of the cell, called the cytoplasm, one always finds two categories of cell organs—known as the Golgi apparatus and the mitochondria.

Within the nucleus of the cell, in addition to the chromosomes, there is constantly found one or more small spherical bodies called nucleoli. The latter have been classified under two heads—plasmosomes and karyosomes. The former stain red in a dye like eosin, the latter blue in a dye like hæmatoxylin, so that in the combination of the two stains hæmatoxylin and eosin, the true plasmosome alone picks up the red stain.

It can be safely said that all cells at some period of their existence contained a nucleolus within their nucleus, and, as a matter of fact, there are very few functional nuclei known in which a nucleolus is not discernible after proper staining methods have been used. The nucleolus of the cell is undoubtedly a more important cell organ than was probably before thought to be the case, as will be shown here.

It is a vital issue with geneticists to know the exact degree of relationship between the chromosomes of the nucleus and the nucleolus. The matter is still somewhat difficult to understand, and the problem in some ways unsolved. Nevertheless modern work has thrown a bright light on many of the previously existing darknesses.

Older cytologists believed that, by using dyes like methyl green, hæmatoxylin, and carmine, it was possible to distinguish between what was chromatinic and what was not chromatinic in nature. Nowadays we realise keenly our shortcomings with regard to this. Basophility, or the attraction of some cell substance for a so-called nuclear dye, means very little to modern cytologists; the whole conception of chromatin is one full of difficulties—difficulties which merely added to by a previously existent idea that chromatin or the substance of heredity and of the chromosomes was some special chemical entity which could always be expected to behave in a special manner under treatment with certain well-known fixing or killing substances and basic dyes.

There is really nothing in the literature of cytology which would enable us to conclude that the nucleolus of the undifferentiated cell was not or is not formed of chromatin; it cannot be too strongly emphasised that staining tests and digestion tests are not sufficiently satisfactory evidence on this point.

We can merely undertake a review of the whole matter after having shorn ourselves of the pernicious doctrine that staining tests give valid evidence as to whether a body is or is not chromatin.

In the ordinary body- or somatic-cell the function of the nucleolus is almost completely unknown. Schreiner[1] believes that he has been able to show that the nucleolus of fat-cells buds off small fragments which, passing into the cytoplasm, give rise to fatty granules. In such a case it is evident that the nucleolus shares largely in the fat-metabolism of the cell. We are inclined to accept Schreiner's account, especially in view of what we know happens in the case of fatty granules in certain eggs.

During the life of the cell, in some cases the nucleolus seems to act as a peculiar centre in the prophases of mitosis. Some observers have been led to look upon the nucleolus as a store of chromatin which is given up at each mitosis to the forming chromosomes, and in tissue-cultures of live cells which we have examined it seems true that the nucleolus does take some part as a centre for the aggregation of chromosomes—but not necessarily as a storehouse for chromatin.

The latter view does not commend itself to us. When, however, we come to the question of the egg cell, we find that much more is known of the functions and behaviour of the nucleolus. In the very young egg cell the nucleolus is normal, and of the same size as that of the typical connective-tissue cell. During the growth of the egg, however, most remarkable changes may overtake the original nucleolus, its contribution to the formed elements of the cytoplasm may be very considerable, and the changes undergone by the fragments of nucleolus within the cytoplasm may be of a remarkable description.

The first case which we may consider is that of the egg of Patella. The latter is the well-known limpet found on rocks about the level of the seashore when the tide is half-way out. The case of Patella has recently been worked out by R. Ludford, who has kindly allowed us to see his preparations. In the young egg of Patella, the nucleolus is oxyphil—a plasmosome, or, in other words, it stains red in eosin, and not blue or basophil in hæmatoxylin. As the cell grows the plasmosome becomes slightly fragmented, and the secondary nucleoli so found are shot out of the nucleus into the ground-cytoplasm of the cell. So far as has been ascertained these extruded elements soon degenerate, but a peculiar change gradually occurs in the original nucleolus: part of the latter loses its affinity for the red dye, and begins to stain blue in the hæmatoxylin; one then has formed a double body or amphinucleolus, part red or oxyphil, and part blue or basophil. The red part goes on budding off smaller pieces, which pass into the egg-cytoplasm, while the blue half does not seem to take much part in this process.

So far as can be stated in such a difficult matter, the portions of the nucleolus extruded into the egg-cytoplasm take no part in the formation of yolk or fat or other formed granules or deutoplasm of the Patella egg. The fragments appear to disintegrate gradually. In such a case as this, we find it very difficult to bring forward an explanation as to the purpose of these nucleolar extrusions.

When, however, we examine the oogenesis or egg-formation of a marine worm like Saccocirrus, we find a very different story. In Saccocirrus we have a nucleolus inside the young egg-nucleus just as with Patella, but during the growth of the egg, the original nucleolus buds off fragments which—passing into the cytoplasm—give rise by fission to numberless granules of a dense proteid nature; these granules form the main bulk of the reserve materials of the Saccocirrus egg, and constitute the yolk. In such a case there is no doubt as to the function of the nucleolus—it is here an organ for the production of reserve materials, which are later to be used up in the development of the embryo.

In the egg-formation of animals there is still another peculiar type of nucleolar behaviour which we may notice. In the insects, and especially in

[1] *Anat. Anzeig.* (1915).

forms like the ants, ichneumon-flies, etc., the nucleolus is to be found within the nucleus in much the same way as occurs in Patella or Saccocirrus; we find, too, that nucleolar buds pass into the egg-cytoplasm; but the fate of these buds is very different from what we have already seen. The most wonderful part about the behaviour of the extruded nucleoli in ant-eggs is that they form secondary nuclei; that is to say, each nucleolar granule expands and froths out to form a hollow sphere just like a true nucleus, and with a nucleolus, nuclear membrane, and linin-network of its own. These wonderful nucleolar nuclei—secondary or accessory nuclei, as they are known to zoologists—often appear exactly similar to an ordinary nucleus, and we have frequently deceived clever microscopists, who, on being shown such a preparation, have asserted that the secondary nuclei were true nuclei.

We have now obtained some conception of the peculiar diversity of behaviour of the nucleoli of egg-cells—later we will return to the subject of secondary nuclei, but at this stage it may be mentioned that the peculiar secondary nuclei ultimately degenerate—leaving only the true nucleus within the cell.

Up to the present we have produced no evidence as to the probable relationship between chromosomes and nucleoli. Until quite recently we really had no evidence as to this question, which is one of great importance; but recently H. M. Carleton,[1] working on the gut-cells of the frog and the cat, has succeeded in breaking some interesting new ground. Rio Hortega,[2] a Spanish worker and a pupil of the famous Cajal, had previously shown that the nucleoli of mammalian eggs contained small argentophile granules or cores, which could be demonstrated by using a formalin silver nitrate technique.

Modifying such a technique, Carleton was able to show that this core, during the division of the cell, behaved independently of the chromosomes; in cell division, as is well known, the chromosomes appear in the nucleus as formed bodies which divide into two halves, each daughter cell receiving one-half of the original chromosomes. Now, Carleton was able to stain the chromosomes red in safranin or carmine, and the intra-nucleolar core or nucleolinus, as he called it, a black colour in reduced silver; the two sets of bodies could not be confused, and the complete independence of both was demonstrated without a doubt. We have ourselves gone through Carleton's preparations.

Let us now review the facts brought forward: the nucleolus of the egg-cell may produce bodies of a proteid nature forming the egg yolk, it may form nuclear-like bodies termed secondary nuclei, or it may produce fat in the somatic cell (Schreiner). The independence of the nucleolus and the chromosomes has been demonstrated clearly in a limited number of animals. Obviously, we may say, the nucleolus is a separate and very important nuclear element, whose functions in the metabolism of the cell are of signal importance.

Keeping these facts in view, and attempting to face the issues boldly, we are led to look upon the nucleolus as a cell element, in the growth and life, if not in the reproduction, of the animal—an importance equal to that of the chromosomes themselves. The formation of secondary nuclei in ant and other hymenopterous eggs is an occurrence which to us is fraught with significance—the nucleolus, in other words, possesses the power of producing nuclei independently of the chromosomes.

The nucleolus to us represents the trophochromatin, the chromosomes the idio- or gono-chromatin. Probably the nucleolus of the cell is the homologue of the macronucleus of the protozoon like Paramœcium, the chromosomes, the homologue of the micronucleus.

[1] *Quart. Jour. Micr. Science*, 1920.
[2] *Trab. Lab. Invest. Biol. Madrid*, xi, 1913.

ESSAY-REVIEWS

EVIDENCES OF MEDIUMISTIC PHENOMENA, by O. L. BRADY, D.Sc.: on **Phenomena of Materialisation,** by BARON VON SCHRENCK-NOTZING, translated by E. E. FOURNIER D'ALBE, D.Sc. [Pp. xii + 340, with 225 Illustrations on art paper.] (London: Kegan Paul, Trench, Trubner & Co. Ltd. 1920. Price 35*s*. net.)

THIS volume is the first English translation of Dr. von Schrenck-Notzing's *Materialisations-Phænomene*, and includes besides this work the supplementary *Kampf um die Materialisations Phænomene* published in 1914 and other material collected to the present date and embodied in the forthcoming second German edition.

Dr. Fournier d'Albe states in his preface that the English revision has been prepared in consultation with the author and with Mme Bisson, the medium's protectress, so it may be taken as the final word of these investigators on the phenomena they set out to describe.

It may be stated at once that it seems regrettable that Dr. d'Albe should waste his time in translating this work or that a publisher should produce so costly a volume when so much scientific work of real value is denied the dignity of print on the plea of the cost of printing and paper. Those anxious to study psychic phenomena, if sufficiently trained in scientific method to make their investigations of value, should have no difficulty in using the German edition and the same applies to those interested in the study of superstitions and demonology.

The hysteria incident to the war has caused a great demand for emotional stimulus of this kind, and no doubt the sales will justify the expenditure. The psychological effect of this work, with its apparent scientific exactitude, on the lay mind cannot be other than harmful, and it is difficult to conceive that an English translation is in the interests of science or of national sanity.

The author, in his introduction, deprecates scepticism and claims that, though mediums are sometimes detected in fraud, this does not imply that none of the manifestations are genuine. The medium sometimes practises fraud to satisfy the audience when genuine results are not forthcoming. This is an attitude difficult to cope with, and is not likely to appeal to the trained investigator.

The phenomena dealt with are principally those produced by the medium Eva C., the experiments starting in 1909 and continuing to 1918, though the author's personal observations ended on the outbreak of war. Great stress is laid on the probity of Mme Bisson, and the fact that Eva does not earn her living by mediumship. It is claimed that there is no motive for fraud, but Dr. Schrenck-Notzing is very daring in assuming, in such an off-hand way, that money is the sole motive of dishonest action.

The description of the medium's psychology indicates an hysterical nature with great weakness of will. Her receptivity when hypnotised is made the excuse for possible trickery, a useful precaution. It is claimed that the idea of fraud in the minds of the audience might suggest chicanery to the hypnotised medium; thus are the sceptical barred from detecting trickery. For the

same reason Mme Bisson cannot surrender the medium into other hands, for experiment on Eva would be equally accessible to the new influences, presumably evil. This is particularly noteworthy as Mme Bisson was present, usually in close contact with the medium, at all the sittings of which Dr. Schrenck-Notzing has personal experience.

There follow the records of a large number of sittings, carried out in red light, with careful descriptions of the phenomena seen. At the same time a number of flash-light photographs are reproduced which corroborate these descriptions, the control of the medium is supposed to be very careful, and the descriptions of the phenomena accurate, but here Dr. Schrenck-Notzing betrays the fact that he lacks the requisite scientific spirit for his task. In no one case are all conditions controlled at the same time. The most complete examination was at the sitting of November 26, 1913, described on page 289. There was a thorough initial and final examination (including gynæcological) of the medium, and of the séance costume and cabinet. The hands and knees of the medium were apparently visible during the whole sitting; in addition, after the séance, an emetic was administered, and the contents of the stomach examined. No mention, however, is made of any examination of Mme Bisson or of her movements during the sitting. As at other sittings described this lady frequently holds the hands of the medium and sometimes enters the cabinet, such an omission should not have occurred. Various examinations were made at other sittings, both of the medium and of Mme Bisson, sometimes at their own suggestion, but it is not made clear whether they were warned of the nature of these examinations beforehand, or whether Mme Bisson was ever examined at any time other than when she herself suggested it. Further doubt is thrown on the accuracy of the author's evidence by Figs. 111 and 112. In the first case the tone values of the enlargement (*a*) differ so materially from those of the original (*b*), that one doubts if it is an untouched photographic enlargement. Possibly reproduction is at fault, though the excellent get-up of the book does not suggest this. In the second case (*b*) is certainly not an enlargement of (*a*). Such carelessness of description should not be found in a book which lays claim to the greatest scientific accuracy and care.

The most serious accusation of fraud is that published by Miss Barkley in the *Psychic Magazine*, and Dr. Schrenck-Notzing deserves great credit for the fairness with which he presents the case for and against. The charge is that some of the pictures materialised by Eva were merely cut from the illustrated paper *Le Miroir*, and partly disguised by rough pencilling. In Fig. 119 a photograph is reproduced in which some material appears on the medium's head; on this material "we can recognise the words 'Le' (small type) 'Miro' (large type). That is evidently meant to be 'Le Miroir.' . . . I cannot form any opinion on this curious result."

At the beginning of the next session the author remarks that Mme Bisson saw the proofs of the photograph only immediately before the sitting, and that Eva was not informed. He is much surprised when Eva, on going into the trance, immediately said the word "Miroir" and continued, "Elle voulait vous écrire autrefois, elle voulait vous envoyer sa pensée écrite. Vous êtes pour elle son miroir. Elle se revoit ici. Vous avez une photographie d'une pensée de Berthe. Elle a la joie de se créer un (*sic*) autre image." A very ingenious escape from a slip.

Fig. 136 shows a man's head apparently on some paper-like material; it has been suggested that this is a portrait of President Wilson cut from *Le Miroir*, and thinly disguised by pencil work. The likeness, even in the tie and collar, is so startling that it is difficult not to be suspicious. The poise of the head is different, but this could be accounted for by the obvious creasing of the paper. Similar pictures produced by the medium have been identified with ex-President Deschanel, ex-President Poincaré, and the ex-King of

Bulgaria. How far these suggestions are correct, and how far Dr. Schrenck-Notzing meets them must be left to the considered judgment of the readers of the book.

The most amazing admission is, however, made in the *Retrospect.* " Since in Munich we operated with a newly prepared cabinet, it was easy to discover pin-holes, and they were regularly found at the places where the pictures were exposed by rolling up the curtain. A pin was found after the departure of the medium on the underside of the left arm-rest." Apparently, in spite of all the elaborate searching, it was possible to introduce pins into the cabinet ; it seems that the medium was more clever than the observers. Of course these may have been materialised pins. The attenuous nature of the teleplasm, its passage through the medium's clothing without any apparent mark, and its fugitiveness in white light do not suggest that materialised pins would leave pin-holes in fabric or would remain to be discovered afterwards. Has Dr. Schrenck-Notzing preserved this precious pin, or did it vanish when brought into the cold light of day ?

Dr. Schrenck-Notzing's methods could be criticised in many other directions, but enough has been said to indicate that, in spite of his claims, they are not rigid. The manifestations may be genuine, but this book is not satisfactory evidence that they are so. Unfortunately, mediums hedge themselves round with so many precautions that it is almost impossible to eliminate fraud. The history of spiritualism records the exposure of medium after medium who for long periods were able to deceive the most acute investigators. The scientific man, therefore, is justified in maintaining an attitude of scepticism until more conclusive evidence is forthcoming.

REVIEWS

MATHEMATICS

A First Course in the Calculus. By William P. Milne, M.A., D.Sc., and G. J. B. Westcott, M.A. [Pp. xv + xxxix.] (London: G. Bell & Sons, 1920. Price 5*s.* net.)

THE first part of Dr. Milne and Mr. Westcott's book was reviewed by the late Mr. P. E. B. Jourdain in SCIENCE PROGRESS, vol. xiv, p. 152. It dealt only with differentiations and integrations of powers of x, and applications. This second volume considers trigonometrical and logarithmic functions, the differentiation of products and quotients, and of inverse trigonometrical functions, and then goes on to the differentiation and integration of logarithms, expansions and calculations, and differential equations. The historical introduction gives short biographies of Taylor and Maclaurin. Packed in between the main propositions there is also useful mention of many theorems in dynamics and physics, and geometrical and graphical methods are very largely employed. There are many examples for the student to work out. Like most modern writings, the work is loose rather than rigid; but we question whether on the whole the student gains any advantage from this looseness—whether, indeed, the older dissertations such as those of Bartholomew Price and Isaac Todhunter are not really capable of giving him the same knowledge with less effort, or at least with more satisfaction. The older, rigid, and logical expositions supposed the student to be a rational person, whereas the modern method insists upon treating him more as a child. The former reached the summit by a steep ascent, perhaps; but the latter keeps him wandering about along a gentle slope where he is very apt to lose himself without ever reaching the summit at all. On the other hand, this book often does reach particular points more quickly, or even more easily, than the rigid system used to do—as in the cases of expansions and of integrating factors, for example; and integration is put very simply, and, as it ought to be, almost *pari passu* with differentiation. The greatest gain is obtained in the examples and graphs. We should have liked to see Boole's elementary exposition of Taylor's Theorem by means of finite differences, and also some note of his symbolic methods, given in this book.

A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse. By FLORIAN CAJORI, Ph.D. [Pp. viii + 299, with portraits of Berkeley and Maclaurin.] (Chicago and London: The Open Court Publishing Co., 1919. Price 8*s.* 6*d.* net.)

THE way in which the Americans are fast out-pacing British scientific workers in certain lines is demonstrated by the issue of this much-needed little book. Although we have many teachers of mathematics, few of them trouble to write on such a fundamental matter as the history—much less to write in the detail adopted by Prof. Cajori. The book deals, however, only with part of the discovery of the Calculus, namely from Newton to Woodhouse. It gives numerous and full excerpts from various writings with translations where required, and deals fully with discussions such as the polemics between

Berkeley, Robins, and Jurin; and the reader is grateful to the author for the excellent judgment shown by him in selecting the various passages.

But the book concerns itself chiefly with the philosophical side of fluxions, the questions of infinitesimals, differentials, and Newton's opposite method of fluxions. Really this side does not appear to the reviewer to be so important. There are many ideas in philosophy and science which are almost obvious to anyone, but which it is nearly impossible to expound rigorously in words, chiefly owing to the fact that words have variable and discontinuous meanings. That acute writer Berkeley detected this difficulty at once and set himself to his attack, in the interests of his own cloth, against the philosophers. His portrait is given in the book, and the reviewer thinks that he was not quite as candid a person as Huxley and de Morgan and many others seem to think. The mere fact that he professedly set out to "counteract atheism" shows not only that he was biased in his opinions before he started, but that he was not aware of what a fatal blemish such a bias may be. His attack upon fluxions appears to have been largely in the nature of a jest; and he must have enjoyed the evident confusion which he created among the ranks of the mathematicians by his well-directed shafts, especially when these gentlemen began to fight amongst themselves after attacking him. The fact remains, however, that Newton was right and that the method of fluxions is sound; whereas, on the other hand, Berkleyism leads merely to solipsism.

We think that Prof. Cajori should have dealt not only with one part of the subject, but with the whole of it. This would, of course, have meant a much larger book, but then the reader would have had the whole matter in his hand at once. For instance, no mention is made of the recent discovery by Mr. J. M. Child of the connection of Isaac Barrow, Newton's teacher, with the Calculus (*The Geometrical Lectures of Isaac Barrow*, Open Court Publishing Co., 1916), nor is the Newton-Leibniz controversy dealt with. Really these matters are much more interesting than the philosophical points examined in the present volume. The fundamental discovery was that from a given function a second one may be derived which gives the first function's rate of change, and that, conversely, the first function gives the integration of the second one—just as set out in Newton's famous anagram to Leibniz. Opinions differ as to who originated this momentous advance in human knowledge; but, pending an exhaustive analysis, the reviewer thinks it most probable that Newton was the originator—that he not only taught his teacher, Barrow, but that he gave the whole idea to Leibniz, who merely embroidered the details while Newton was fully engaged upon applying his methods for the advance of natural knowledge in every direction. Newton built the house, though Barrow may have helped him to dig the foundations, and Leibniz to place the chimney-pots. It is even questionable whether d-ism is really better than dot-age, because, for instance, the latter is applicable in quaternions, whereas the former is not. Might we not also have had some more of Brook Taylor than the five lines given to him on p. 50—which do not even mention his great theorem. But we are grateful to Prof. Cajori for what he has given us, and hope that he may attack the rest of the history some day.

R. R.

An Introduction to Combinatory Analysis. By Major P. A. MacMahon, D.Sc., Sc.D., LL.D., F.R.S. [Pp. viii + 71.] (Cambridge: at the University Press, 1920. Price 7*s*. 6*d*. net.)

This little book is intended to be an introduction to the author's two masterly volumes on Combinatory Analysis published by the Cambridge University Press in 1915–16, and reviewed in Science Progress, vol. x, p. 601, and vol. xi, p. 695. In writing it the author designs to place the

matter in a simple way before his readers, having been reminded of the great Euler, who wrote a famous algebra addressed to his manservant. He rightly thinks that there is advantage in explaining a complicated, if not difficult, matter to untrained minds. He thinks that the subject-matter of the book shows that the algebra of symmetric functions and an important part of combinatory analysis are beautifully adapted to one another. But perhaps the most difficult kind of writing is that which aims at putting things as simply as possible; and the author's task has therefore not been as easy as the reader, who is gratified with his exposition, may think. That he has succeeded in his task will be admitted by all. The book contains six chapters: the first on the Elementary Theory of Symmetric Functions; the second Opening the Theory of Distribution; and the remaining chapters on further details of Distribution.

PHYSICS

Matter and Motion. By J. Clerk Maxwell. Reprinted with Notes and Appendices by Sir Joseph Larmor, and with a new photograph of the Author. [Pp. xv + 163.] (London: Society for Promoting Christian Knowledge; New York: The Macmillan Co., 1920. Price 5*s*. net.)

In reintroducing this classical essay on the principles of dynamics, the editor remarks justly that "as a reasoned conspectus of Newtonian dynamics, generalising gradually from simple particles of matter to physical systems which are beyond complete analyses, drawn up by one of the masters of the science, with many interesting side-lights, it must retain its power of suggestion, even though parts of the vector exposition now seem somewhat abstract." The conspectus has been made more complete by the addition of the chapter from Maxwell's treatise on *Electricity and Magnetism* "On the Equations of Motion of a Connected System," and further by the editor's appendix on the Principle of Least Action.

Reissued during the stir produced by the confirmation of Einstein's Relativity Theory, interest naturally focuses on those sections which relate to the relativity of our knowledge of space and time. One cannot help feeling that Maxwell, like most men of his day, had not resolved for himself the contradiction between two lines of thought. He is careful to urge (§ 35) that acceleration is relative and cannot be interpreted absolutely. On the other hand, he describes in detail the ideal experiment of Newton's bucket, and the practical one of Foucault's pendulum, as means of determining the absolute rotation of the earth. While the human mind confessed itself unable to conceive of permanent standards of direction, it appeared that Nature did select from all the variety of conceivable standards one which fitted best to the course of its happenings.

At first sight, the new theory of Relativity, which is the subject of the editor's first appendix, succeeds in resolving the paradox. But it may yet be that the last word has not been said. In Einstein's theory, for example, the specification of the field of gravitation is dependent upon the arbitrary choice of the system of measurement. The mathematical quantities involved serve, in fact, to define the relation of the system of measurement to the sequence of natural phenomena. From this point of view the new law of gravitation appears as a limitation upon the system of measurement which the observer may use. The law in one form consists in identifying the so-called "curvature of space-time" with the presence of matter. This is only saying that some systems of measuring space and time fit on to our observations of matter better than others. Or if we adopt the equivalent form of the law as contained in the principle of least action, we have to admit that in the long-run we have to choose our system of measuring so as to make the action a minimum.

We do well, therefore, to read over again the foundations of classical dynamics as here expounded, to ponder its limitations, and to beware lest, in criticising the older schools of thought, we may be condemning ourselves.

E. CUNNINGHAM.

CHEMISTRY

Qualitative Analysis in Theory and Practice. By P. W. ROBERTSON, M.A., Ph.D., Professor of Chemistry, Victoria University College, New Zealand, and D. H. BURLEIGH, A.R.C.S., D.I.C., B.Sc. [Pp. 63, with tables.] (London: Edward Arnold, 1920. Price 4*s*. 6*d*. net.)

THE present tendency is rather in the direction of giving up qualitative analysis and "spotting" as a means of introduction to the technique of practical chemistry. Prof. Robertson and Mr. Burleigh, however, hold the view that qualitative analysis *intelligently taught* is of very great value in forming a foundation for the general chemistry of the metals, and in illustrating the more important types of chemical reaction; they regard the practice followed by most textbooks of describing in detail the individual reactions of the metals, and writing all the equations for these reactions, as pernicious and demoralising, the student in the end simply copying into his notes what he sees in the book.

The method, therefore, which has been adopted is to make the student aware of the kind of reaction to be expected in any given case—in other words, they have endeavoured to eliminate the false antithesis between theory and practice; then, having learnt the different types of chemical reaction, the student is able to see how they are applied to the problems of systematic analysis.

The weak point in the above argument is, of course, that it depends upon the *intelligent teaching* of the subject—in other words, upon the active co-operation between teacher and student—or else the whole procedure descends to the level of the teaching of Latin grammar, the sole virtue of which seems to lie in the fact that it affords the maximum amount of pointless effort on the part of the student, with the minimum degree of exertion on the part of the teacher.

Assuming, however, the correctness of the authors' views, the present little book should be a very useful introduction to practical chemistry, and the long experience of the two authors is a guarantee that the book has been carefully thought out and put together. F. A. M.

Quantitative Analysis by Electrolysis. By ALEXANDER CLASSEN and H. CLOEREN; Revised, Rearranged, and Enlarged English Edition by WILLIAM T. HALL, Associate Professor, Massachusetts Institute of Technology. [Pp. xiii + 346, with 2 plates and diagrams.] (London: Chapman & Hall, 1920. Price 17*s*. 6*d*. net.)

CLASSEN's book on Electrochemical Analysis is too well known to need any fresh introduction to English readers. The present edition is a revision of the 1913 edition without reference to the German text, so that it is in many respects a new work.

An attempt has been made to apply the modern electronic theory a little more closely than has usually been the case with other works on the subject, so as to assist the beginner to understand exactly what takes place during electrolysis.

The first part deals with the general laws governing the method and with the types of apparatus needed. The second part covers the electro-analytical determination of various metals, including those elements, such as aluminium and the alkalies, which can only be deposited as amalgams; the third section includes the methods of electrochemical separation of

various metals; whilst the last part deals with certain special analyses which arise in actual practice. The book should be as welcome to electrochemists and analysts as its predecessor, and should, in addition, gain many fresh readers. F. A. M.

A Foundation Course in Chemistry, for Students of Agriculture and Technology. By J. W. Dodgson, B.Sc., A.I.C., Lecturer in Chemistry at University College, Reading, and J. A. Murray, B.Sc., Lecturer in Agricultural Chemistry at University College, Reading. Second Edition, thoroughly revised, with new chapters. [Pp. xii + 241, with illustrations.] (London: Hodder & Stoughton, 1920. Price, 6*s*. 6*d*. net.)

As indicated by the title, the purpose of this book is to give the student such assistance as can be obtained from books in acquiring a knowledge of those fundamental facts and general principles of Chemistry upon which the superstructure of agricultural chemistry or other technical application must necessarily rest. The attempt has been made to emphasise those aspects of the subject which are of special importance to such students, while others have been treated in sufficient detail to enable the general principle to be securely grasped.

In the present (second) edition several sections, notably those on the Fixation of Nitrogen and the Chemistry of Organic Matter, have been re-written, and a new chapter on Physical Chemistry has been added.

The treatment is simple and straightforward, so that the volume should be useful to students attending short courses in Agriculture, Horticulture, and Dairying, and to those attending courses in Hygiene, Domestic Economy, and the like. F. A. M.

A Class-book of Organic Chemistry. By O. B. Cohen, Ph.D., B.Sc., F.R.S., Professor of Organic Chemistry, Leeds University. Vol. II, for Second-year Medical Students and others. [Pp. 156 + viii.] (London: Macmillan & Co., 1919. Price 4*s*. 6*d*. net.)

One feels almost sorry for the average medical student when one realises the number of subjects with which he is expected to have at all events a nodding acquaintance, so that any endeavour to lighten his labours by presenting one of the subjects in a concentrated and readable form is to be welcomed even if the attempt tends to savour of "cramming."

Certainly there is no subject upon which the average G.P. should know more than organic chemistry, which—as indeed its very name shows—is so intimately bound up with all vital processes, but unfortunately there are few subjects in which he is usually less interested

Prof. Cohen's book should therefore be of considerable value to second-year medical students; and in addition it should be useful to those students of chemistry who are specially interested in the therapeutical application of their science.

The book follows more or less the usual arrangement and has the advantage that the descriptive text is interspersed with illustrative experiments all "guaranteed to work," so that the little volume will be a useful laboratory companion as well as a condensed textbook. F. A. M.

Organic Chemistry for Medical, Intermediate Science, and Pharmaceutical Students. By A. Killen Macbeth, M.A., D.Sc., F.I.C. [Pp. xii + 235, with diagrams.] (London: Longmans, Green & Co., 1920. Price 6*s*. 6*d*. net.)

As stated in the preface, this book is written from the point of view of the teacher who has in mind the difficulties encountered by the average student,

and an effort has been made to meet these as they normally arise. The text is divided into twenty-six chapters which, considering the small size of the book, contain in all quite a considerable amount of information. The subject-matter, which is everywhere clearly set forth, follows the conventional arrangement according to which aliphatic and aromatic compounds are segregated in different parts of the book; but for the convenience of comparison, the end of each of the earlier chapters gives a reference to a subsequent chapter in which the corresponding group of aromatic compounds is dealt with; this is quite a good idea, and facilitates the use of the book for those who prefer to study aliphatic and aromatic compounds together.

P. H.

The Physical Chemistry of the Metals. By RUDOLPH SCHENCK, Professor of Physical Chemistry in the Technischen Hochschule in Aachen. Translated and annotated by REGINALD SCOTT DEAN. [Pp. viii + 239 with 114 Figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 17*s*. 6*d*. net.)

THIS textbook, the outcome of a series of lectures by Prof. Schenck to the engineers of the Rhenish industrial district, covers an extremely wide field, ranging from the physical properties of the metals to the complex chemical reactions occurring in the blast furnace, and the roasting of ores. The wide scope of the work renders impossible the full development of all aspects of the subject within the limit of 240 pages.

A valuable feature of the book is to be found in the numerous tables of physical constants of the metals and their alloys. The vapour pressures, solidification curves and the structure of alloys are dealt with at some length, and these properties are discussed with abundant references to particular examples and widely illustrated by diagrams. Considerable attention is paid to the electronic theory of the conductivity of metals, while on the other hand the treatment from the point of view of the phase-rule is somewhat fragmentary.

The alloys of iron with carbon and of copper and other metals with their oxides and sulphides are surveyed, and many excellent microphotographs are given, illustrating the changes in the structure of steel on heat treatment. The chemical equilibria of oxidation and reduction reactions, occurring in the manufacture of metals from their ores, are treated exhaustively, and some previously unpublished data bearing on these problems are included. The theory of blast furnace reactions, which is presented, lays great stress on the effects of the composition of the gas phase on the heterogeneous equilibria between the oxides of iron, cementite, iron and the oxides of carbon. The "roasting reactions" occurring when copper and lead sulphides are heated in oxygen are treated similarly.

This interesting survey of the physical chemistry of the metals should prove especially useful to metallurgists and technical chemists.

Margarine. By WILLIAM CLAYTON, M.Sc. Monographs on Industrial Chemistry. [Pp. xi + 187, with 24 illustrations.] (London: Longmans, Green & Co. Price 14*s*. net.)

SINCE the Franco-Prussian War created the demand for a fat that should be a satisfactory substitute for butter, great advances have been made, so that to-day, as a result of the Great War, we find Margarine established as a common, almost universal, article of diet. To produce an article which should be a chemical approximation to butter would seem to have been no very difficult matter, but to produce an article equivalent in taste and nutritive value, in texture and stability, is an achievement not to be won in a day. The problem, at first one for the chemist, now demands the

attention of the physicist and the physiologist. A complex colloidal system is involved, whilst yet we are but timorously laying down the principles of the colloidal state; and revolutionary ideas in nutrition demand attention.

Sir Edward Thorpe, in his editorial introduction to the series of Monographs on Industrial Chemistry, to which this volume is a valuable addition, anticipates that the series will serve to show how fundamental and essential is the relation of principle to practice, and to indicate the bearing of the academic thought of to-day on the industrial chemistry of to-morrow. A monograph that fulfils these precepts will have a wider appeal than one that is merely an exhaustive analysis of the diversity and minutiæ of technical manufacture. Mr. Clayton's book deserves this wider audience.

The author surveys the raw materials of the margarine factory and emphasises the notable advance made by the introduction of hydrogenated fats. He gives a lucid account of the modern processes of manufacture, with descriptions of some types of plant, and describes in detail the analysis of both raw material and finished product. Chapters are devoted to butter, renovated butter, and lard compound. Throughout the text are many references to the literature and patents, and a considerable bibliography is included.

All this is required of a purely technological handbook, but the author is interested in the larger problems involved, and turns happily, in the centre of his book, to the difficult task of explaining in a few pages the position of present theories of emulsification. So brief a statement must necessarily leave much unsaid, but it will achieve its purpose if it stimulates to broader thought and to question. Again, in a chapter on Nutritional Chemistry, the author attempts a summary of the present state of knowledge of the "Accessory Food Factors" or "Vitamins," and its bearing on the nutritive value of butter and the various margarines. It is of interest to note that since the publication of this monograph the hard-and-fast line drawn between animal and vegetable fats as regards their content of Vitamin A (adopting the suggested nomenclature of Drummond) has been questioned and the anomalous position of lard explained.

Margarine will be welcomed by student and technologist, and by all workers in these fields of thought who find interest in the practical application of knowledge.

R. K. C.

The Principles of the Phase Theory: Heterogeneous Equilibria between Salts and their Aqueous Solutions. By Douglas A. Clibbens, Ph.D. [Pp. xx + 383.] (London: Macmillan & Co., 1920. Price 25*s*. net.)

Beyond a certain point the phase theory ceases to have any immediate interest, and presents no new conceptions. Its methods become severely technical, and the whole treatment tends to enter the region of pure geometry. In specialised fields of activity, such as the preparation of salts by the interaction of other salts in aqueous solutions, a knowledge of the artifices of graphical representation is indispensable to the worker, since it is only by mapping out the whole of the region of states that he can be certain that no unexpected phases have been overlooked. It is to a portion only of this, in itself very specialised, study that Dr. Clibbens has introduced his readers. He has not treated of such cases as the formation of solid solutions, which frequently arise in practice, and the general applications of the phase theory to other systems finds no place in the book. Dr. Clibbens has done the work in an exceedingly able manner, and the diagrams, on folding sheets, are especially to be commended. A good feature of the book is the stress laid on the importance of all parts of the phase diagrams, and not merely of the equilibrium curves.

Although in its restricted scope the book is in every way excellent, it is not altogether clear to the reviewer for what class of readers it is intended.

It is unsuitable for students, since it is of far too specialised a character, and the principles underlying the phase theory can be much better appreciated by a consideration of simpler cases, in which the purely arbitrary geometrical difficulties of representation are less formidable and distracting. The author, on the other hand, explicitly states that his book is not a work of reference; the absence of an index, and of any method of division of the subjects of the text, which runs from one topic to another without obvious break, confirm this decision. The general reader will find that a study of the book calls for considerable concentration. The long digressions on geometrical representation, even with the aid of the diagrams, are not easy to visualise. This, however, is the case with all advanced accounts of the phase theory, and Dr. Clibbens has succeeded in making the subject much clearer than is usually the case. The collection of the results of a large mass of scattered information, and its treatment on a uniform plan, will also lighten the work of numerous technical chemists who are employed on investigations of the kind described. Some of those who, like the reviewer, have had an opportunity of seeing the results of a careful application of the phase theory in the hands of Mr. Freeth (who contributes an introduction to the book) will wish to study the methods for themselves. Dr. Clibbens' book should prove a useful introduction to this field of applied physical chemistry.

J. R. P.

Ozone. By E. K. Rideal, M.B.E., M.A., Ph.D., Professor of Physical Chemistry, University of Illinois. A treatise of Electrochemistry, Edited by Bertram Blount, F.I.C. [Pp. x + 198.] (London: Constable & Co., 1920. Price 12*s*. net.)

The chief objection that may be raised against Prof. Rideal's work is that perhaps too much attention is paid to theory and that the practical side is therefore relatively neglected, as 142 pages are devoted to theoretical consideration, whilst the applications are dealt with somewhat shortly in 34 pages.

Yet perhaps, after all, there is much to be said for this arrangement as ozone has hardly attained to the importance in industry that might be expected from so active and clean an oxidising agent.

The problem is chiefly one of price, and if only some new and more efficient process could be discovered for preparing ozone, or some method for increasing the efficiency of existing processes, the use of ozone would increase considerably.

Even as it is, ozone is used for such diverse purposes as sterilising water, treating wounds, for bleaching and for the production of vanillin, heliotropin (the formulæ on p. 166 are incorrect, by the way), and for purifying air, though in this connection Dr. Rideal points out, however, that the value of ozone is open to doubt and that the immunity of motor-drivers on the London tubes during the 'flue epidemic was probably to be attributed to the uniformity of temperature rather than to any specific action of the ozone in the air!

The author has done a useful service in collecting together much scattered information on the subject and the book will be of great assistance to those interested in the theory and the applications of the gas.

F. A. Mason.

The Carbohydrates and Alcohol. By Samuel Rideal, D.Sc., F.I.C., and Associates. [Pp. xv + 216 with 11 Figures.] (London: Baillière, Tindall & Cox, 1920. Price 12*s*. 6*d*. net.)

In this, the latest volume of the series of books on Industrial Chemistry prepared under the general editorship of Dr. Samuel Rideal, a concise account is given of the technology of the industrially important carbohydrates and of their fermentation products.

A very brief introduction gives some account of the properties of the carbohydrates, their chemistry is but touched upon, and more information on solubilities, densities and other physical properties would have been useful. Exception must be taken to the sweeping statement that the iodine reaction, characteristic of all starches, is not produced by any other known substance, as Barger some years ago obtained blue adsorption phenomena with a number of crystalline organic compounds.

The book is divided into six parts dealing with (1) Starch and its products, (2) Sugar, (3) Beer, (4) Wine, (5) Distilled fermented liquors, (6) Vinegar. There is some indication of a general plan in each section, but this has probably suffered as the result of collaboration. An historical sketch is followed by some account of the natural sources of the various products and the conditions of cultivation. Details of manufacture, the utilisation of waste products and statistics of production are dealt with, and each part concludes with a list of references to papers, articles and textbooks. Part II, dealing with sugar, is the largest section, and contains a clear account of the cultivation of cane and beet and the various manufacturing processes by which sugar is obtained from these sources. Among the fermentation products, beer is given the pride of place and space; a short section is devoted to the problem of synthetic alcohol, now becoming so urgent in view of the fuel oil shortage, and another to the fermentation processes for the manufacture of acetone and of glycerine, which assumed great importance during the war, owing to the lack of these compounds for the preparation of explosives.

The facts are clearly presented, though occasionally there is slight carelessness of phraseology; for example, on p. 195, confusion might arise between methyl and ethyl alcohols. No attempt has been made to give a complete account of the subject, but a general outline is supplied, which indicates some of the difficulties of manufacture which have been overcome, and the problems yet to be solved. The book would have been more valuable if the theoretical side had been dealt with. The chemistry of the sugars, of the manufacturing processes and of fermentation is barely mentioned, and the many problems in physical chemistry which arise in the various processes are ignored. The book is, however, welcome in that it may divert students of chemistry from the ornamental but popular dye-stuffs to a more vital branch of industry in which, in the future, chemistry will play an even greater part than it has in the past.

O. L. B.

Chemical Fertilisers and Parasiticides. By S. Hoare Collins, M.Sc., F.I.C. [Pp. xii + 273, with 8 figures.] (London: Ballière, Tindall & Cox, 1920. Price 10*s.* 6*d.* net.)

This book is a companion volume to the author's *Plant Products and Chemical Fertilisers*, and, like it, also belongs to the Industrial Chemistry series. Whereas in the earlier volume the chemical fertilisers are only treated from the point of view of crop increment, the present volume is devoted rather to the sources and modes of manufacture of these same compounds. The book is divided into six parts, entitled respectively "The Need for Fertilisers," "The Sources of Fertilisers," "The Manufacture of Fertilisers," "The Use of Fertilisers," "The Future of Fertilisers," and "Chemical Insecticides and Fungicides." The mere recital of these headings, however, gives but a very inadequate idea of the amount of interesting and out-of-the-way information contained in this book. Very special attention is paid to the chemical processes and principles involved in the manufacture of the fertilisers, and the author, consequently, does not hesitate to draw upon the Phase Rule in describing the manufacture of commercially pure potassium fertilisers from the naturally occurring sources such as the double salts sylvine, kainite, carnallite, etc. The utilisation of atmospheric nitrogen and the manufacture of inorganic

nitrogen fertilisers are discussed at some length, and a very good summary of the synthetic ammonia and nitrate processes is given. A considerable amount of space is devoted to the phosphatic manures, as becomes their importance, and incidentally the growth of the sulphuric acid industry is commented on, together with the possible means of disposing of the excessive quantities produced as a consequence of the war; in this connection reference is also made to the use for agricultural purposes of dilute sulphuric acid as a spray for killing weeds, although, of course, it is not suggested that this would account for any considerable quantities. In a section devoted to the valuation of fertilisers will be found an interesting discussion of the economic aspect of the question from the point of view of the cost to the farmer of his fertilisers, together with some practical hints as to how he could reduce his costs by suitable storage and economy of carriage. There are, furthermore, special sections dealing with manure for special soils and climates and also manures suited for special crops. The writer is evidently fully alive to all the recent developments of the industry, as is witnessed by his inclusion of most of the processes employed during the war, and in more than one place he emphasises the importance to the country of scientific effort and the utilisation of modern appliances. His views on the possibilities of the manufacture of fertilisers may be stated in his own words: "It is one of the problems of the future to convert some of this war enterprise into useful peace industry, and to this end the fertiliser trade might take some active part." P. H.

Everyday Chemistry. By W. Robinson, B.Sc., Instructor in Chemistry and Physics, Officers' School of Education, Oxford. [Pp. viii + 138, with illustrations and diagrams.] (London: Methuen & Co., 1920. Price 3*s*. 6*d*. net.)

This little work is the outcome of the endeavour made at the end of the war to provide an outline course of chemistry for the use of those officers engaged in delivering short courses to the rank and file of the army.

Obviously the problem was no easy one, as the men could hardly be treated as children, so that the usual line of approach would not serve, and any attempt at systematic study would create an interest in the few only. Consequently a method of approach along three main lines was adopted in the school: (1) History, (2) Common Objects, (3) Industrial Interests; and this method has been embodied in the present book, which is essentially a teacher's book and is not intended to be used as a textbook by pupils working alone; it is, in fact, more or less in the nature of lecture notes.

As the author points out, adult education does not cease with the army scheme, and the hope is expressed that the book may be of use not only in continuation schools but also in Study Circles, W.E.A. classes, where teachers are troubled not only with the mere imparting of knowledge but with the greater problem of how to use this subject as an educational instrument.

F. A. M.

GEOLOGY

The Geology of India for Students. By D. N. Wadia, M.A., B.Sc. [Pp. xx + 398, with 20 plates and 37 figures.] (London: Macmillan & Co., 1919. Price 18*s*. net.)

As it is now more than a quarter of a century since Oldham's book on the *Geology of India* appeared, and as during that period a large volume of work on the subject has been published in the *Records of the Geological Survey of India* and elsewhere, there is abundant need for the publication of a textbook incorporating the recent work. The aim of the author has been to provide for the students in the Indian colleges a summary of what is known, at the present time, of the geology of the country, and thus to save them the trouble of searching

the original records. However admirable it may be to encourage students to consult the original papers wherever possible, it is generally inadvisable, in the earlier stages at least, to have these as the sole source of information, and hence there is always room for a textbook such as this.

The first chapter of the book gives a brief account of the physical features of the country, and is unsatisfactory on one account—namely, that it assumes a knowledge of all the technical terms used, and hence is only suitable for those who have already had a good grounding in the subject. The next twenty-three chapters are concerned with the stratigraphy of the country, which is treated in considerable detail. A criticism similar to that mentioned above applies to this part also, especially in the sections dealing with the Archæan rocks. Many detailed petrological descriptions of the rocks are given, often in highly technical language, and very little attempt is made even to define the names of the types peculiar to India.

Another peculiar feature is the great absence of specific names in the palæontological lists. Where so many genera persist from one period to another, it is essential that the species peculiar to each period be indicated; otherwise the lists of fossils are useless. Thus, of the hundred or so genera given as characteristic of the Cretaceous of the Peninsula, only two species are named.

Some of the stratigraphical correlations are decidedly bold, but this is probably due to the desire to give the student a lead in those cases where the published evidence is still controversial. It is somewhat unfortunate that this plan was not followed throughout, as in some cases two or three theories are very briefly stated, and, without further particulars, it is impossible to discriminate between them.

The stratigraphical part is followed by a chapter on physiography, which includes a repetition of much of the first chapter, and another on economic geology, while finally there is an appendix on the geology of Kashmir. References to literature are given at the end of each chapter, but unfortunately no uniform system of contractions for the names of journals is used.

The plates are well reproduced and have been admirably chosen to illustrate the structures, but the figures in the text are very deficient so far as the legend is concerned. For example, the figure on p. 69 is unintelligible without further explanation. The book is well written, and the descriptions on the whole are clear. There are, however, a number of misprints—*e.g.* on pp. 67, 143, 165, 198, 261, 324, and 353—which might have been noticed.

A. S.

The Natural Wealth of Britain: Its Origin and Exploitation. By J. S. Duby. [Pp. x + 319, with 130 illustrations.] (London: Hodder & Stoughton, 1919. Price 6*s*. net.)

In this book, which forms one of a series intended for the upper forms in schools, the aim of the author is to give an account, in simple and non-technical language, of the geology of Great Britain, and to indicate the relationship between the geological structure and the industrial development of the country. The first part of the book forms an introduction to general geology, and is mainly concerned with such subjects as the origin of sedimentary and other rocks, the relationship and succession of such rocks, the use of geological maps and so forth. The second part deals generally with the distribution of the various industries, while in the third, certain districts, especially the coal-fields, are discussed in greater detail.

A book on these lines should, from the educational point of view, be of general use, but it must be stated that the volume under review by no means fulfils the expectations raised by the preface. The book is marred throughout by looseness of diction and numerous inaccuracies, which are all the more

unfortunate in view of the fact that it is intended for beginners. The defective nature of the work will be evident from the following few examples: The discussion on pp. 12 and 13 gives the impression that granites are essentially the oldest rocks; on p. 14 the dark constituent of granite is stated to comprise "all the various metallic compounds"; on p. 16 we are told that "sapphire, ruby, aquamarine, and topaz are crystalline forms of clay." Many astonishing statements occur in the section dealing with the pottery industry, while in the account of the Scottish Midland Valley, the Giffrock Freestone is incorrectly described as belonging to the Calciferous Sandstone, and is correlated with the Oil-shales Series of the Lothians. Perhaps the most absurd example of carelessness in the book is the following sentence in the description of the Firth of Forth, on p. 283: "The *Clyde* is bridged from Queensferry on to such a volcanic outcrop; and on the *Fife* coast the superb position of Edinburgh is also due to a volcanic mass" (the italics are the reviewer's). Although the book contains numerous illustrations, these are very crudely drawn, while the contractions used for the place-names on the maps are often not immediately obvious.

As a whole the book has so many inaccuracies that any beginner to whom it was recommended would be liable to acquire a mass of information more curious than correct.

A. S.

The Chemical Analysis of Steel Works' Materials. By FRED IBBOTSON, B.Sc., F.R.C.Sc.I., F.I.C. [Pp. viii + 296, with 21 Diagrams.] (London: Longmans, Green & Co., 1920. Price 21s. net.)

THE author of this book is well known to all interested in the analysis of both ferrous and non-ferrous materials, owing to the fact that he is the joint author with H. Brearley of a treatise on the *Analysis of Steel Works' Materials*, and with L. Aitchison on *The Analysis of Non-Ferrous Alloys*, two books which have been well received and largely used. The present volume purports to be a new and revised edition of the portion of the first mentioned, dealing with analytical methods only, all matter referring to pyrometry and microscopy having been eliminated owing to the fact that a large number of textbooks dealing with these branches have appeared in recent years. The book is divided into eight parts dealing with, General Processes, Analysis of: Steel and Pig Iron, Steel-making alloys, Ores, Refractory Materials, Slags, Fuels and Boiler Water, etc. In Part I. the methods employed in the separation of iron from other metals are dealt with in detail, and, as this separation is so frequently used in the analysis of steel, it is undoubtedly advantageous to have these methods grouped and described together. The same remarks apply to reductions by nascent hydrogen and their application to the determination of iron, molybdenum, vanadium, etc., and to the mercurous nitrate precipitation of tungsten, chromium, molybdenum and vanadium. The "basic acetate" separation of iron is particularly well described, the important work of Brearley receiving special attention and details being given which have frequently been neglected in other books.

The question of sampling is of vital importance to the steel works' chemist, and his work may be entirely misleading, unless the material employed for the actual analysis is truly representative of the bulk from which it is taken, and, in view of this fact, it is disappointing to find that the author dismisses this subject with a little over one page of matter. It is true that the possibility of the contamination of samples by the use of unsuitable mortars is pointed out, but the precautions necessary to avoid the undue influence of segregation on the results obtained have been entirely ignored. The value of the book would have been increased had it included directions for the sampling of metalliferous materials, the methods of analysis of which are dealt with so fully.

By far the larger portion of the book describes the details of analysis, and all the methods given appear to be sound and free from unnecessary complications. The exact details of manipulation used in different laboratories vary so widely, however, that it would be impossible to find any experienced chemist who would not prefer to replace some of the methods given by others; for example, in the colorimetric determination of manganese, the description of separate weighing, solution, treatment with persulphate in the presence of silver nitrate and subsequent comparison of the colour with a known standard would undoubtedly have been an improvement on the method given. The methods for the determination of aluminium, uranium, and zirconium are dealt with in a complete manner, and the portion dealing with the analysis of steel-making alloys should prove of value to all interested in these important materials.

The matter is well arranged, and the index enables definite determinations to be referred to readily. The book may be strongly recommended to steel works' chemists and to others who have to carry out, from time to time, determinations contained within the scope of the work.

C. O. Bannister.

BOTANY AND AGRICULTURE

Heredity and Evolution in Plants. By C. Stuart Gager. [Pp. xv + 265, with 113 Illustrations. (Philadelphia: P. Blakiston's Son & Co., 1920.)

This interesting little book is one of the best elementary accounts of Heredity and Evolution that we have read. Written in simple language, it succeeds not only in conveying a great deal of information, but, what is the more important, the probable significance of our knowledge with a due appreciation of its limitations.

The first two chapters, which describe the Life History of the Fern, are adapted from the same author's *Fundamentals of Botany*, and serve as a basis for the general principles dealt with in Chapter III. Alternation of generations is used effectively to illustrate the importance of distinguishing between the inherited characteristics and their visible expression.

After considering the various theories put forward to explain evolution by Lamarck, Darwin, De Vries and others, and the recent work on Mutations and Mendelian inheritance, the author reviews the evidence from various sources, and devotes two chapters to Palæobotany and Geographical Distribution. The latter contains a very brief but adequate summary of modern views regarding distribution, means of dispersal, and the age and area hypothesis. The section on fossil plants treats of their distribution in space and time.

In the final chapters the possible origin of some of the more important groups is considered, and in summarising the divergent opinions any dogmatic attitude is avoided by an immediate exposition of the "other views" which are held.

The subdivision of the subject matter into paragraphs averaging about one and a half pages each, rather unduly breaks the continuity of the text, but that the author has attained his professed object of providing reliable and readable information of an elementary character most will, we think, agree.

E. J. S.

Practical Plant Biochemistry. By M. W. Onslow. [Pp. iii + 178.] (Cambridge: at the University Press, 1920. Price 15s. net.)

Written with the object of filling the gap between Organic Chemistry and Plant Physiology, the subject-matter of this work is divided into ten chapters, of which the first provides a short introduction, in which the chief

groups of chemical substances encountered in plants are briefly considered, together with the types of synthesis involved in their production.

The second chapter treats of the colloidal state, in illustration of which nine experiments are described. Of these five are devoted to methods of preparation of sols, gels, emulsoids, and suspensoids, and the remaining four to illustrate precipitation by electrolytes, dialysis, and Brownian movement. No experiments are, for example, suggested upon the changes in volume of gels, their relation to absorption and diffusion, and the many properties which chiefly interest the biologist. Indeed, the practical work on this important section appears to us very inadequate.

The subsequent chapters treat of Enzyme Action; Carbon Assimilation; Carbohydrates and their Hydrolysing Enzymes; the Fats and Lipases; Aromatic Compounds and Oxidising Enzymes; the Proteins and Proteases; the Glucosides and Glucoside-splitting Enzymes; and the Plant-bases.

Each chapter is followed by a bibliography, and a useful index is provided.

In all, 158 experiments are described, with instructions that are to be commended for their clarity and attention to detail.

These pages undoubtedly supply a distinct need for which teachers and students will alike be grateful. In such a work the choice of experiments is largely a matter of opinion, but we venture to think that many will agree in regretting that the authoress did not see fit to include experiments of a quantitative character. To the student of Botany, for whom these pages are primarily intended, the mere extraction and recognition of the chemical substances within the plant is a very necessary preliminary, but it is the chemical and physical properties on which his chief interest centres, and for the investigation of which he would welcome a practical guide.

E. J. S.

A Manual of the Timbers of the World; their Characteristics and Uses. By Alexander L. Howard. (Pp. xvi + 446 with 128 Illustrations.] (London: Macmillan & Co., 1920. Price 30*s.* net.)

The major part of this volume is occupied by a catalogue of timbers from all parts of the world. In all, some 350 different commercial timbers are dealt with, and it is an interesting sidelight on the need for much more work and many more workers in this field of Economic Botany, that nearly 10 per cent. of the timbers here enumerated are derived from unknown sources. When it is remembered that these are mainly a selection of the better-known woods it will be realised how deficient is our knowledge in respect to those less familiar as commercial products.

The author has aimed at completeness so far as the timbers upon the European markets are concerned. That such a restriction was necessary is obvious when we remember that Australia alone produces some four hundred timbers, whilst those of North America are even more numerous.

For most species data is furnished regarding the weight of a cubic foot of the dry wood, the country of origin, and the technical characteristics, uses and qualities. The author's forty years' experience in the timber trade gives to the last an especial value.

At several points Mr. Howard has some pertinent remarks to make on British timbers. Referring to the Austrian method of cutting oak into billets and wainscot wood, he says, "With the foreign oak the best methods to suit the particular requirements of this country have been found, and acted upon, but there have been no such wise methods employed with the 'home-grown' product."

English ash suffers in comparison with that from France owing to the more efficient forestry in the latter country, a criticism that the author might have levelled with equal justice against English oak.

A reprint of Laslett's Tables is added showing the results of tests on the tensile, crushing, and breaking strengths of various timbers. The work concludes with a chapter by S. Fitzgerald on the artificial seasoning of timber.

There are a number of excellent photographic illustrations of which those portraying the teak industry deserve especial mention.

E. J. S.

The Yeasts. By ALEXANDRE GUILLIERMOND, D.Sc., translated and revised by F. W. TANNER, M.S., PH.D. [Pp. xix + 424, with 163 Figures.] (London: Chapman & Hall, Ltd., 1920. Price 33*s.* net.)

THIS book, originally appearing in French and now translated into English and much enlarged both by the author and the translator, deals with the yeasts, not only from the economic aspects, but from the more general standpoint. The morphology, cytology, physiology and phylogeny of the yeasts and yeast-like forms are fully described, and nearly half the book is given up to a systematic survey of the group and to the treatment of the special physiology and morphology of individual species. Prof. Guilliermond's researches on the structure and relationship of these fungi are so well known that it almost goes without saying that the portions dealing with these special points are treated in an interesting and critical manner. We are provided with an account of the parasitic and symbiotic relationships of these organisms; the presence of vitamines ("accessory good substances") in yeast and the practical importance of this fact are enlarged on, and we are told that these substances seem to be essential to the existence of the yeast plant. The full and broad treatment of these subjects increases the general usefulness of the treatise.

One cannot, however, feel such confidence in the translation. There are places where, apparently, the very opposite is stated to what was intended; there are, also, innumerable occasions where the rendering is not as "free" as could be desired. On page 38 we read, "To-day the nucleus is unique. . . . Later on we shall take up nuclear division" on page 140, "At this moment each of the cells divides its nucleus." "It never produces conidia, but, on the contrary, forms a rather large number of ascs." The translator is responsible also for the revision of the section on the physiology of these organisms. Here, we find the term "hydro-carbon" continually used when carbohydrate is meant; and there is a tendency to diffuseness which at times is confusing, *e.g.* the different and irreconcilable statements made as to the possibility of yeast assimilating maltose. The book is well "got up" and fully illustrated.

E. M. C.

Weeds of Farm Land. By WINIFRED E. BRENCHLEY, D.Sc., F.L.S., Fellow of University College, London; Botanist, Rothamsted Experimental Station. [Pp. x + 329, with illustrations.] (London: Longmans, Green & Co., 1920. Price 12*s.* 6*d.* net.)

THE subject of this volume is one of great practical importance to the farmer, for it deals with plants which are responsible, in a number of ways, for a considerable reduction in the yield of his crops. Dr. Brenchley brings together a large amount of information previously scattered through the literature of agriculture and botany, and essays to make of it a collected whole, laying no claim, however, to covering completely the entire ground.

First, the methods of distribution are discussed, the means by which the weeds of farm land are brought into their position from adjoining fields or from greater distances. In the former connection such practices as the proper cleansing of farm implements are insisted on as necessary for the restricting of a quite considerable source of transport; in the latter it is pointed out that very active agents of long-distance distribution are to be

found in cargoes, ballast, and impure seeds; and that new-comers, so imported, often get a strong hold on their new terrain. A chapter on the methods of preventing and eradicating weeds follows, and the vitality of weed seeds is then discussed. Preventive measures were foreshadowed already in the consideration of the methods of spreading; while under curative measures both mechanical means—special methods of cultivation—and chemical means—treatment with sprays and manures—are critically examined. The practical importance of the length of life of seeds, if not so obvious, is none the less real; as buried seeds remain alive for considerable periods, in the case of the gorse seeds, here mentioned, for at least twenty-five years. The problem raises wider issues, and we would have wished to be presented with a fuller account of the causes which have been suggested as active in inducing dormancy. It is interesting to learn that the author's own work has substantiated the view, widely held by farmers, that charlock seeds, when buried, are long-lived; and as interesting to note that the opinion that creeping thistle seeds are abortive is contrary to the facts.

Dr. Brenchley, in considering the association of weeds with soils, largely makes use of her own work on this part of the subject. She finds herself at variance, on several points, with the other workers who do not seem to have dealt with such a variety of districts nor to have treated their results in a quantitative manner. More confidence could be placed in the results here put forward if they were accompanied by analyses, mechanical and chemical, of the soils whose weed floras were investigated. The uses and habits of weeds are treated of, and poisonous, injurious, and parasitic weeds are described, with special methods of eradication. There is only a quite incidental mention of the damage that weeds may cause through being the bearers of diseases that can also attack crops.

There are a number of illustrations to the volume—we do not seem to be told anywhere who is responsible for them—and many of them, especially some of the habit-drawings, deserve a special word of praise.

E. M. C.

ZOOLOGY

Principles of Animal Biology. By A. F. SHULL, with the collaboration of G. R. LARUE and A. G. RUTHVEN. [Pp 441 + xv, with a frontispiece and 244 illustrations.] New York and London: The Graw-Hill Book Co., 1920. Price 21*s*. net.)

SOME years ago the University of Michigan replaced its introductory course in Zoology based upon the type system by one treating mainly of principles, and the present volume is a textbook suited for such a course. It was thought, however, that even in laboratories where the type system is still adopted it would be useful to have such a book to supplement the morphological work. While it is questionable whether it is wise to entirely replace a morphological course by one dealing with principles, there can be no doubt that some such work as the present could be read with advantage in correlation with any introductory course in Zoology.

Most of the information in it is available in textbooks and other volumes to be found in any Zoological laboratory, but it is here sifted and collected in relation to the various subjects dealt with in the sixteen chapters. Thereby the junior student is saved a considerable amount of time.

One of the features of the book is a fairly extensive glossary which is very useful to a beginner, but we cannot help thinking that even in an elementary work some of the definitions could be more concise and that its value would be greatly enhanced by giving the derivations of most of the terms used.

The book is readably written and well illustrated.

C. H. O'D.

The System of Animate Nature. By J. ARTHUR THOMSON, M.A., LL.D. [2 Volumes, pp. 1–xi. and 1–687.] (London: Williams & Norgate, 1920. Price 30*s*. net.)

A BOOK by Prof. J. Arthur Thomson is always to be considered as a special event in the zoological world, but we have no hesitation in saying that these new volumes by Prof. Thomson constitute a mile-post marking the place of the summarisation of biological thought of recent years.

We have read these two comfortably sized volumes with true delight; we only sigh for more time and more students to undertake researches which Prof. Thomson's fertile thinking have brought to the mind. The author has dealt with the major philosophical problems of the universe in his own inimitable style; the central core of each problem has been stripped clear of its surrounding superfluities, and laid bare for examination. Prof. Thomson is far from being a "mechanist"; those of us who had any sneaking hidden regard for the Mechanistic Formulæ will find cold comfort in these pages.

The arrangement of the book is peculiar, and may we say, daring? The author takes such a subject as "Organism and Mechanism," or, "The Problem of Body and Mind," discusses it, and at the end of the chapter or lecture he gives a summary.

We wish Prof. Thomson were more physiological; with his gift for sifting out the wheat from the chaff, we have no doubt that he could write a book of great value on the physiological aspect of zoology. Let not this remark be interpreted in any sense as disparaging of the present volumes—it emanates merely from admiration of the way in which the author has handled philosophical problems. Prof. J. Arthur Thomson is now giving his valuable assistance to the *Journal of the Royal Microscopical Society*, and we hope that the new results which are so rapidly accumulating will be embodied by him in some future work on the physiological aspects.

No matter in what subject the reader is interested he will certainly be surprised at the complication of the by-ways to which the modern biological thought has run. It would be too much to attempt to deal here with all the material collected and discussed by the author; but we must draw attention to the great breadth of the author's knowledge, and to the skilful manner in which he has utilised the wealth of material dealt with by him.

J. BRONTË GATENBY.

Vertebrate Zoology. By Prof. H. H. NEWMAN, Ph.D. [Pp. 432 + xii, with 217 figures.] (New York and London: Macmillan & Co., 1920.)

THIS is intended as a textbook for the use of students in pre-medical classes in the United States. It could be termed more accurately a book of Chordate Zoology since it includes not only the Craniata but also the Cephalochordata, the Urochordata and the Hamichordata. The natural history of the various divisions of the Chordata are dealt with in a general way and certain forms treated in some detail from the morphological, palæontological and embryological points of view. Throughout the book the evolutionary ecological and physiological aspects are dealt with more fully than is generally the case in books of this sort.

The book is quite well illustrated and has a good index. C. H. O'D.

The Sea Fisheries: History and Administration. By JAMES TRAVIS JENKINS, D.Sc., Barrister-at-Law. [Pp. xxxi + 299, with numerous illustrations.] (London: Constable & Co., 1920. Price 24*s*. net.)

IT is an easy task to review a book on a subject in which the author is an acknowledged expert, and *The Sea Fisheries*, by J. T. Jenkins, D.Sc., Ph.D., Barrister-at-Law, Gray's Inn, is no exception to the rule. The author—who,

by the way, is Superintendent of the Lancashire and Western Sea Fisheries District—has made ample use of his opportunities, and has produced a book which is at once a help and inspiration to all who have the welfare of the fisheries at heart.

The book is divided into thirteen chapters, with appendices, bibliography, an index, and numerous excellent illustrations.

In the Introduction alternative suggestions for the control of the fisheries are ably put, and there is a well-merited but tempered criticism of the staffing of the existing Central Authority.

It is to be hoped that the preamble on "Reconstruction" will be "read, marked, and learned" by the Authorities concerned.

From the chapter on "Statistical Methods" the immensity of the trade in fish will be realised; and to take one species only, the herring is distributed all over the world, for fish was practically the only pre-war foodstuff exported from the British Isles.

Under "Methods of Fishing," the evolution of the trawl from the seine net, and the gradual introduction of the steam trawler, are described at length.

Chapter III describes the trawling grounds, drift-net fishing, methods and value of herring fishing, and also that of the most important flat-fish to the fish trade—the plaice.

In the next chapter the plaice and sole, haddock and herring, are given as types of their several zoological orders, and an interesting epitome of their life-history is set out—the metamorphosis of the plaice being of profound interest.

The rise of the herring fisheries is next ably dealt with, and the historical student will find much new and interesting material to ponder over. The herring bounty system is detailed, and the fallacious arguments of Adam Smith with regard to it are exposed.

The development of steam trawling forms the title of another chapter, and here again the author is at home in his subject.

Legislation and the Sea Fisheries is described, and many Conventions, national and international, are quoted and explained with clarity.

The effect of legislation on the inshore fisheries, which are carried on within the territorial limits by men who own their own boats, should be most helpful to those concerned in drawing up By-laws for the control of same.

In the section dealing with Public Fisheries for Shellfish, particulars of the destructiveness of the shrimp trawl are given, though, be it said, it is to the credit of the men that, where circumstances permit, they are really anxious to save as many of the small fish, taken along with the shrimps, as possible. The question of the long overdue extension of statutory protection to the berried lobster is gone into at some length. An interesting illustration of the damage done by the cockle's chief enemy is given in a full-page plate, showing the crop and stomach contents of a "sea-gull," in this instance taken from a herring gull (*Larus argentatus*). The plate shows numerous whole and fragmentary specimens of cockles. After emphasising the value of the shellfish fishery, the author goes on to detail the results of bacteriological investigations, and many specific instances of polluted shellfish being the direct cause of illness and even death are quoted.

Under the heading "Education of Fishermen," the difficulties of overcoming the conservatism of these "toilers of the sea" is enlarged upon; there is also an interesting description of the Continental Fishery Schools, which latter might with great advantage be copied here.

Then follows a chapter on "Scientific Research and the Sea Fisheries," tracing the development of the work from the efforts of Prof. Sars, and the results of the famous *Challenger* expedition. Modern methods of biological and hydrographic research are outlined, and an interesting description of the metabolism of the sea is given. Investigations into the life-history

of the freshwater eel, and the remarkable results of the plaice-marking experiments, are quoted.

In the chapter on "State Aid and Fishery Research," many startling statements with regard to official discouragement are given.

The last chapter is descriptive of foreign and colonial fishery administration, commencing with a full account of the magnificent fisheries organisation of the United States of America.

While it is obviously impossible to treat the subject fully in all its branches, we think the author has maintained the balance well. Personally we should have liked a more detailed treatment of the historical side of the subject, but possibly other readers would prefer other branches, *e.g.* the scientific to have been treated in greater detail. Students of *The Sea Fisheries* will find the bibliography of great value, since it appears to contain a reference to practically all the important works on the different branches of the subject. At a time when we are promised a "Fisheries Bill" by the Government, the appearance of a work of this kind is singularly opportune.

A. W.

The Heron of Castle Creek, and other Sketches of Bird Life. By Alfred Wellesley Rees, with a memoir of the author by J. K. Hudson, and a portrait. [Pp. 217.] (London: John Murray, 1920. Price 7*s.* 6*d.* net.)

The Heron of Castle Creek, to give the volume its brief cover title, is no easy book shortly to review, as one can approach it from three view points so distinct that it almost requires three reviewers to do it justice. While one section conveys the impression that it is a "literary effort" pure and simple, with a bird as the centre of focus, another strikes one as being purely the well-written jottings of an observant naturalist, while others again are simply stories. As a whole the book is quite inconsistent, but it is nevertheless very pleasant reading. While it will probably not tempt any reader to continue reading to the detriment of his work, it will no doubt have a wide appeal, particularly to those who have read and enjoyed *Ianto the Fisherman* by the same author.

Of the seventeen chapters which make up the volume only two are devoted to the story of the Heron of Castle Creek, but with the exception of the last chapter on the Partridge, they are easily the best. Of the five chapters given to the Partridge, the first four are a mixture of story and observations. The last, entitled "A Day with the Partridge," is the most delightful in the book. Vividly and well written, it cannot fail to charm the reader, be he sportsman, naturalist or layman.

The remaining chapters—"The Wood Wren"; "Misadventures of Bird Watching"; A Moorland Sanctuary," are some of the titles picked at random—while they will no doubt please and even fascinate many readers, are, to be frank, disappointing to the bird lover. Whether it be the style adopted or not it is difficult to say, but they are unconvincing. One is left wondering as to how much of the information is to be taken seriously. On p. 27, to take a single example, the author ascribes to the Wood Wren a nest lined with feathers, while every ornithologist knows that the lack of feathers in the Wood Wren's nest is so constant a characteristic that it makes it a safe guide for discrimination between this nest and those of the Willow Wren and the Chiff Chaff. The very meagre index supplied heightens the impression that the book is not meant for the serious naturalist.

To leave the reader with this impression is, perhaps, hardly just, for (apart from the memoir of the author by Mr. Hudson stating the fact) it is clear that Mr. Rees was a keen observer of nature, and throughout the volume he raises and comments on problems of very considerable interest to the naturalist. For this reason more than any other the book should be entitled to a place in the naturalist's library.

Wm. Rowan.

Wild Life in Canada. By CAPTAIN ANGUS BUCHANAN, M.C. [Pp. 264 + 20, with a map and 28 illustrations from photographs by the author.] (London: John Murray, 1920. Price 15*s*. net.)

THIS book is a record of a journey of nearly 2,000 miles in dog-sled and canoe undertaken by the author in 1914. It was not published earlier owing to the author's absence on military service. The route lay through the almost unknown land in Northern Saskatchewan up to the limits of the Barren Lands.

For the general reader the main part of the book provides a straightforward and interesting account of this wonderful northland, its inhabitants and the manner of journeying through it. Here and there it is marred by slight faults that might have been removed in reading the proofs: e.g. the thermometer registered 4 *per cent.* above freezing" and "a fair guide . . . are." Nevertheless it is well worth reading and conveys a very true impression of life in Northern Canada. Its pages, like the autumn wind, carry some of the magnetism of the open spaces, the tang of the biting winds over the snows and the haunting mystery of the great silence.

For the naturalist is provided a list of the birds and animals seen or collected on the journey, and this constitutes a very valuable contribution to our knowledge of the natural history of these regions and of the distribution of certain species in Canada. It is the first collection of the kind to be made over this territory.

C. H. O'D.

An Introduction to Entomology. By JOHN HENRY COMSTOCK, Professor of Entomology and general Zoology, Emeritus, in Cornell University. Part I. [Second Edition, entirely rewritten. Pp. xviii + 220, with 220 figures.] (Ithaca, N.Y.: The Comstock Publishing Co., 1920. Price $2·50.)

THE present volume is the first of two which will make up the complete work. It has been put on the market now as the result of requests from teachers and others who knew that the work was in preparation, instead of being held in till the completion of Part II. As a result it may be a very considerable time before the last half is published.

Students of entomology have every reason to be glad that this step has been taken. The second part, we understand, is to "treat of the Classification of Insects." No more information than this is vouchsafed; no suggestion is made as to the lines to be adopted. Whatever form the volume is to take, we can trust the author to produce something sound, but there are already a number of books good on taxonomic entomology and there is no doubt that the more important section is the one herewith coming under notice.

It is a pleasure to receive a book of which one can speak with nothing but approval. *An Introduction to Entomology* is such a book. There are four chapters under the following titles: "Characteristics of Insects and their Relatives," shortly describing the main features of the various classes of Arthropods; "The External Anatomy of Insects"; "The Internal Anatomy of Insects"; "The Metamorphosis of Insects." These cover an enormous field for so small a volume, but the amount of information crammed into the pages is made possible only by the terse, business-like style of the author and the omission of all unnecessary padding. In the final chapter, for instance, of the multitudinous variations possible in the metamorphosis of insects, not one of any importance is overlooked. Being "an introduction" they are briefly treated, but all entomologists will be glad to find between two covers information that they have previously had to search out from books innumerable.

Although a scientific book, *An Introduction to Entomology* is interestingly written. The language is simple and can be understood by the youngest. While the recognised standard terms for all the varied phenomena are employed, each is clearly defined when first introduced and the correct

accentation is indicated. The text is well printed on good paper, but it is a pity that it is not entirely free of typographical errors. The illustrations are excellent, and a large percentage is original, a valuable asset. One gets rather tired of seeing some of the historical drawings that appear with such monotonous regularity in so many modern scientific textbooks. There is a good bibliography and a comprehensive index. Although the volume is labelled "Part I," it is complete in itself.

In a brief preface the author says: "Two objects are kept in mind—first, to aid the student in laying a firm foundation for his entomological studies; and second, to make available, so far as possible in the limited space of a handbook, a knowledge of the varied phenomena of the insect world." That both these objects have been most successfully achieved in Part I is certain. We look forward with interest to the publication of Part II.

WM. ROWAN.

The Sea-shore. By W. P. PYCRAFT, Zoological Department, British Museum (Natural History). [Pp. vi + 156, with 11 plates, 12 figures, and 2 maps.] (London: Society for Promoting Christian Knowledge; New York: The Macmillan Company, 1920. Price 4*s*. 6*d*. net.)

MR. PYCRAFT is to be warmly congratulated on this little work, which should appeal, not only to children, but also to all adults who go to the seaside in August. The success of such works depends almost entirely upon the style of the writing, and Mr. Pycraft's style is admirable—we can always understand exactly what he means, which is not the case with many similar works. His first chapter deals with the ocean in general, and is as excellent as the similar chapters in Sir John Murray's little book, *The Ocean*, published some time ago, to which this book makes a useful adjunct. The second chapter describes the seas round England; the third chapter, the cliffs; and the remaining chapters, many interesting details. The reviewer fears that he himself was ignorant of the fact that starfishes eat oysters, and that a few of them can clear a whole bed of oysters in a night; that sea-anemones may measure two feet in diameter; and that the British Islands number no less than 5,500 islands altogether. The migrations of the salmonidæ and the eels are well described, and the book contains a coloured frontispiece, maps, figures, and some excellent photographs. For children it will be a revelation and a memory.

MEDICINE

Military Psychiatry in Peace and War. By C. STANFORD READ, M.D. [Pp. vi + 168, with 2 charts.] (London: H. K. Lewis & Co. Price 10*s*. 6*d*. net.)

IT is very creditable to the author of this treatise, who was in charge of a mental clearing hospital—"D Block, Netley"—with only 124 beds, and where the patients that came under his observation could not be detained for more than a few days at the outside, owing to the pressure for accommodation, that he should have found the time to summarise and to correlate his experience and to classify his records of the 3,000 consecutive cases of mental diseases which were admitted during the year 1917.

The author states that over 12,000 patients suffering from mental symptoms passed through Netley from overseas from the commencement of the war up to six months after the Armistice, but probably four times as many were received into the War Mental Hospitals of the United Kingdom, from among those who became affected whilst engaged in war work, or who were on leave in this country, or who became insane after reaching this country and suffered from other diseases. The experience of the various mental consultants in the different commands would probably agree that

over 40,000 persons engaged in the service of their country have suffered from some form of mental breakdown during the period of the war.

The author is a firm believer in the psycho-genetic origin of mental diseases, and is a follower of Freud. He asserts that mental conflict is at the root of all the psychoses: in civil life he states that this conflict centres round the sexual instinct, whilst in warfare it arises out of the instinct of self-preservation, and his terminology is that of his master; he refers to the human "psyche" as the equivalent to the human mind, presumably because the term "psychology," usually accepted as investigating the contents of consciousness, is not wide enough to cover the tendencies of the unconscious mind. The author suggests that fear "in disguise" acts as a stimulation to some persons, but we are inclined to think it is the instinct of curiosity, and not fear, that is at the root of adventure, as Alpine climbers well know, and that this instinct may even dominate that of self-preservation. The "herd instinct" is no new discovery, and the "crowd" psychology or the "collective will power" has always been regarded as an important factor by military commanders; indeed, Napoleon attributed much of his military success to the "resultant thought wave." The author appears to suggest that "patriotism" entered largely into the motive for joining up and conscription; but the sentiment of patriotism is a complex and not a simple emotion, and is a disposition or tendency roused by an idea which forms a part of our conceptual system, although the panoply of war, the ritual, the justice of our cause as tested by our standard of good or truth, helped to kindle it.

Coming to the record of experience, Dr. Read finds no place for "exhaustion psychosis." He states, "it is not a factor to be reckoned with in the production of the psychoses," and yet (p. 143) he admits it is a "contributory factor, and will materially aid the psychic elements at work." He regards Dementia precox to be of psycho-genetic origin, and he includes this in the category of the functional psychoses. He deems those who differ from him to be "superficial" and "shallow," and "those whose materialistic outlook prevented them from seeing their psychic origin." His views as to the alcoholic origin of the psychoses lead him to differ materially from those of other observers, inasmuch as he attributes a percentage of origin of 1·5 to this cause as against 8·5 per cent. recorded by Eager. He deprecates a "label" upon his cases, and uses a terminology of his own in classification, with the result that his statistics cannot be brought into any informing conclusion when compared with those of others who employ an accepted nosology. It is also difficult to follow his tables, as in the classification he shows 3,116 cases, in Table IV he refers to 3,453, whereas he admittedly deals with the 3,000, the original numbers presented for analysis. The author uses the term "psycho-pathic inferiority" to imply what is generally accepted as congenital mental deficiency, and the term "affect" to imply emotional disposition.

He concludes an original and informing treatise by a plea for the use of "restraint" in the treatment of mental diseases, in "suitable cases"; but the difficulty is the one shown in the thesis—viz., that there may be a wide divergence of opinion as to "suitability," with the result that an inevitable lapse into indiscriminate mechanical control of the mentally afflicted is bound to reappear, with all the intolerance of an unsympathetic treatment. It is just to the author to state that he has rèad widely and culled from the experience of others before expressing his own conclusions, which, as may be inferred, deal especially with the application of Freudian psychology to the origin of mental diseases. Apart from some misprints—"casual" for "causal" (p. 29), and "dredging" for "deluging" (p. 162)—and the use of the split infinitive, the work is an interesting contribution to the study of psychiatry.

ROBERT ARMSTRONG-JONES, M.D.

Psycho-Analysis. A Brief Account of the Freudian Theory. By BARBARA LOW, B.A., with an Introduction by ERNEST JONES, M.D. [Pp. 191.] (London: George Allen & Unwin. Price 5s. net.)

A FLOOD of literature has of late swept over the fields of neurology and psychiatry, more particularly in relation to the views of Freud, and to those acquainted with English textbooks on the subject, it is clear that the unsavoury compounds, the Œdipus myth and the Electra complex, are receiving less attention in this country than was to be found at the time of the first translations of Freud's works. Also, less importance is paid to the sexual origin of the neuroses in these textbooks than was formerly the case, and the term *libido* is attaining to quite respectable interpretations; being now regarded mainly as a wish or desire, even an ambition or a dawning hope, but originating in the so-called conflict between unconscious tendencies and the rational psychic life.

The present writer ventured to state that Freudism was dead in England, and to this he adheres; yet psycho-analysis (denuded of its offensive associations) was never more alive; so much so that a constellation of charlatans have set up to probe family relations, individual eccentricities, educational systems and social problems, and unqualified persons are to be found in any number practising upon "patients" in all directions.

The author of the book under review describes herself as a teacher and lecturer, and on p. 146 she states that psycho-analysis is a process necessarily painful, for it involves the giving up of the inmost self and desires to the analyst, the upshot of which "may cause vital changes in his whole outlook and manner of life": yet she repeatedly refers to the results of her practice upon her "patients," and it may become a serious question whether such a procedure as is implied in psycho-analysis, when carried out by unqualified persons, should not come within the purview of the General Medical Council.

It has been maintained by some that the application of psycho-analysis is not "treatment," but only a method or technique to disclose the origin of nervous and mental symptoms, but the textbook referred to devotes a whole chapter to treatment, which is summarised under three heads, viz.: (*a*) Dream Interpretation; (*b*) Free Association; and (*c*) Transference. Dream interpretation depends upon the assumption that there is during sleep a kind of personified guardian preventing thoughts, ideas, and impulses rising from the unconscious mind into consciousness. If they succeed in passing through the "Censorship," then they appear as the dream, which can be narrated, but in reality they represent latent tendencies which have been repressed, and with them certain emotions; the whole forming a complex. In this way, whatever comes into consciousness is interpreted, in accordance with a certain key, to be symbolic of tendencies in the unconscious mind; for instance, dreams of daggers or snakes are of phallic origin. There are various mechanisms in dreams, described as condensation, dramatisation, displacement, regression, and secondary elaboration, by which psychic importance is attached to the latent content or to the unconscious mind. Free association is the method of discovering the tendencies of the unconscious mind by the comparison of irrelevancies that are expressed in speech, and the "stimulus word" reaction is timed by a suitable chronoscope, which latter is, however, rarely done. A "transference" is necessary before psycho-analysis is complete—*i.e.*, emotional reactions must take place in connection with the analyst, and this is either positive or negative, depending upon the patient's "resistance." He cannot help this emotion, which is stated to disclose the condition of his unconscious tendencies, and the latter are deemed to be the fundamental dynamic processes that guide conduct. There are half a dozen terms that appear frequently in this textbook, which, in spite of quotations on almost every page, is nevertheless

interesting and well written. These are complex, transference, repression, conflict, sublimation, and resistance. In spite of paraphrasing, their meaning is yet cryptic and equivocal, although some, such as complex, have come into general use without suspicion of pedantry or arbitrariness. There is a hankering on the part of the authoress for a full discussion of sex curiosity in children, but the present writer's experience can relate more than one case in which irrevocable harm has resulted through suggestions implanted into the child's mind from the libidinous and incestuous thoughts of the analyst. The efforts to understand human motives based upon suspected unconscious tendencies does not set free energy that is said to be used in inhibitions; for it is natural and self-preservative to inhibit and to repress, although too emphatic repression has been regarded as hurtful long before psycho-analysis was taught, for the pedagogic maxim, "Don't say don't," has had a place with teachers long before Freud was known. It is well to warn teachers against the Freudian explanation of wish fulfilments, which this textbook, in common with all the Freudian literature, attributes to the instinct of sex, a view that has become an obsession with this school.

ROBERT ARMSTRONG-JONES, M.D.

Microscopy: The Construction, Theory and Use of the Microscope. By EDMUND J. SPITTA, L.R.C.P., M.R.C.S. [Pp. xxviii + 534, with 83 half-tone reproductions from original negatives and 255 text illustrations.] (London: John Murray, 1920. Price 25*s*. net.)

THE third edition of Dr. Spitta's work on Microscopy follows in its main features the two previous editions, which have proved of considerable value to workers with the microscope. The book is obviously intended more for the amateur than for the professional microscopist. It is written in an easy, sometimes almost in an unscientific style, but the main purpose in view, to impart to the worker the methods of using the microscope rather than the exact scientific principles involved, is no doubt attained. The opening chapters are devoted to elementary optics, the simple and the compound microscope and to the essential mechanical components of which the latter is built up. A number of instruments by various makers are figured and described. The description is usually laudatory rather than critical, and it is to be regretted that Dr. Spitta has not given the reader the benefit of his knowledge and wide experience in pointing out the defects and shortcomings of some much advertised and consequently much used types of microscopes. The orthodox methods of obtaining what is referred to as a critical image are described, and some illustrations are included among those at the end of the book which are intended to show the characteristics of such an image. There would appear to be no reason why any special term should be introduced to describe a series of simple operations which are essential if a correct image is to be obtained. There are two points of outstanding importance in setting up a microscope, alignment of the axes of the optical systems together with centration of the illuminating beam, and the proper utilisation of the largest possible portion of the aperture of the objective. If these two conditions are satisfied then the resulting image in the microscope will be truthful. The subject of dark-ground illumination is treated somewhat cursorily; in fact, the impression given is that this method is of little value. It is quite true that its limitations are considerable, but in modern medicine there is hardly any single microscopic process of such practical value. The difference between illumination by transmitted light and objects seen as self-luminous ones—in other words, the fundamental differences between resolution and visibility in the microscope—is not dealt with satisfactorily, although this is a difference that in practice every microscopist should fully appreciate.

A full description of the usual methods of testing objectives is given, and

this constitutes the most valuable portion of the book. It is true that a very small proportion of workers with the microscope ever test a lens at all, so that this part will appeal to but a limited audience; but it is certain that those who do go to the trouble to test their objectives will learn more of practical microscopy in a short time than can be learnt in any other way. Dr. Spitta has obviously been handicapped in producing this new edition owing to the high cost of production, so that descriptions of new apparatus and methods have been from sheer necessity dealt with as addenda.

The illustrations are excellent, and, as in previous editions, are placed at the end of the book. They are evidence, if such were needed, of the author's skill as a photo-micrographer. At the same time they are peculiarly monotonous, as they consist largely of diatoms. It is at least open to question whether these are the only test objects worth consideration. The almost universal use of the microscope now in all branches of science and industry makes it desirable that a book of this character should recognise the necessities of all microscope users.

It is, however, unquestioned that the author's honesty of purpose is in evidence all through, and that alone will ensure success.

J. E. B.

MISCELLANEOUS

Springtime and Other Essays. By SIR FRANCIS DARWIN, F.R.S. [Pp. xi + 242, with 8 Illustrations]. (London: John Murray, 1920. Price, 7*s*. 6*d*. net.)

THIS delightful little volume comprises a series of Sir Francis Darwin's essays on varied topics. The aim of the writer has partly been to place at the disposal of the intelligent reader his thoughts after having read various books dealing with Botany and Music, and with the Lives of Men and Things. The writer reveals the fact that he is a voluminous reader, for he shows an acquaintance with many topics, and a facility for presenting the most interesting of his siftings in a way which charms the reader. If for no other reason than for the essay, "Recollections," we are glad Sir Francis has published this book; in this essay he has written some further account of his illustrious father, and something of the family life of the Darwins. The author gives some interesting facts with regard to his own career, and his contact with Klein and with Sachs, the latter of whom seems to have been a rather impatient person.

Sir Francis Darwin has also discussed Francis Galpin's work on *Old English Instruments of Music*. The complete book of 242 pages is so full of facts concerning so many people and objects that the reviewer cannot fasten on any one part more worthy of mention than the rest. The literary person is sure to enjoy this pleasant volume.

J. B. G.

Beauty and the Beast. An Essay in Evolutionary Æsthetic. By STEWART A. McDOWALL, B.D. [Pp. 93.] (Cambridge: at the University Press, 1920. Price 7*s*. 6*d*. net.)

THE scientific study of beauty has probably never yet been thoroughly achieved, even by Croce. Yet the subject demands a complete exposition, if only for the fact that the explanation of certain kinds of beauty by the theory of evolution is not very obvious. Evidently things which are beautiful tend to suit us, but not all things which suit us are beautiful; while, on the other hand, it is hard to imagine what advantage we receive from many things that are admittedly beautiful, such as a sunset. We can understand why we are pleased to see a calm sunset—because it suggests pleasant weather; but a stormy sunset may be as beautiful, and there is also beauty

in many terrible things. A scientific consideration of the theme would require a scientific mode of presentment. That is, it should begin by complete definitions, with numerous examples and accurate differentiations, and, above all, the discussion should not be carried out in support of previous metaphysical speculations. Probably the whole subject can be brought fully within the natural scheme of evolution, individual and social.

The Rev. Mr. McDowall's book is a gentle book, often prettily written and without polemics; and is a book by an accomplished writer who has even condescended to study science. But perhaps we should hardly expect a scientific presentment of the subject in this work. He admits that his aim is to link up his theory of beauty with the Christian idea of God, and on p. 52 mentions that his own metaphysical system is a theocentric one. This is all very well; but scientific men would have preferred an impartial analysis to begin with. It is a pity that the author has not adopted the simple, intelligent, and intelligible style of the scientific philosophers, such, for example, as Locke and Hume, in place of the fog of words adopted by the moderns, and generally by the men who may be called the great Charlatan Philosophers. The language they use is generally such that no one can really extract any definite meaning; and they use this language on purpose, in order that no one shall be able to do so—just as the quack doctor does when purveying his pills. The author says that "love is relationship, beauty the expression of relationship," but after reading his book twice the present reviewer failed to grasp his exact meaning, although he is evidently sincere. We had hoped for more light in the chapter on Beauty in Evolution, but received none. Words are used in senses which we do not accept. Few people accept modern psycho-analysis—of which what is true is familiar, and what is not familiar is seldom true. For instance, the author says that "love is, no doubt, in origin an impulse of sex" (p. 58), that beauty has the same action (p. 64). Of course the love of children, neighbours, and many other things may be much more correctly called the Social Cement, and has really been derived by the tribal evolution forced upon us by ages of war. It is hopeless to discuss conclusions contained in books of this nature, which are really only attempts to fit facts to theories and to justify previous metaphysical conclusions or fancies. O. A. C.

The Groundwork of Modern Geography. By A. Wilmore, D.Sc., F.G.S. [Pp. xv + 396.] (London: G. Bell and Sons, 1920.)

This book is intended to provide an "introduction to the so-called 'New Geography.'" The author hopes that "not only the student, but also the intelligent general reader, may find the book of some interest." It is certainly "of some interest," but, besides that, is one of the best-arranged and most succinct books we have seen.

It is divided into three sections: the first, dealing with the structure of the earth's surface; the second, with the principles of climatic geography; and the last, with biological geography. There is also an appendix containing questions selected from the examination papers in geography set by various well-known examining bodies, a glossary, and an index.

The twenty-seven plates given are very well printed and clear, and the paper is good.

Here and there we find somewhat questionable statements, such as "Italy is Mediterranean, with some Nordic in Lombardy, and Alpine in the north-east." Surely the Alpine element is predominant in the whole of Italy north of the Appennines? There are also one or two mistakes in grammar and punctuation.

C. C. R.

BOOKS RECEIVED

(Publishers are requested to notify prices)

A First Course in Nomography. By S. Brodetsky, M.A., B.Sc., Ph.D., Reader in Applied Mathematics, Leeds University. London : G. Bell & Sons, 1920. (Pp. xii + 135.) Price 10*s*. net.

A Course of Modern Analysis. An Introduction to the General Theory of Infinite Processes and of Analytic Functions ; with an account of the Principal Transcendental Functions. By E. T. Whittaker, Sc.D., F.R.S., Professor of Mathematics in the University of Edinburgh, and G. N. Watson, Sc.D., F.R.S., Professor of Mathematics in the University of Birmingham. Third Edition. Cambridge : at the University Press, 1920. (Pp. 608.) Price 40*s*. net.

The Gyroscopic Compass. A Non-Mathematical Treatment. By T. W. Chalmers, B.Sc., A.M.I. Mech. E. London : Constable & Co., 10 Orange Street, Leicester Square, W.C., 1920. (Pp. x + 167, with 51 figures.) Price 11*s*. net.

Higher Mechanics. By Horace Lamb, Sc.D., LL.D, F.R.S., Honorary Fellow of Trinity College, Cambridge. Lately Professor of Mathematics in the Victoria University of Manchester. Cambridge : at the University Press, 1920. (Pp. x + 272.) Price 25*s*. net.

Ou En Est la Météorologie. By Alphonse Berget, Professeur a l'Institut Océanographique. Paris : Gauthier-Villars et Cie, 55 Quai des Grands Augustins. (Pp. 300.)

A Kinetic Theory of Gases and Liquids. By Richard D. Kleeman, D.Sc. (Adelaide), B.A. (Cantab.) Associate Professor of Physics, Union College, New York : John Wiley & Sons ; London : Chapman & Hall, 1920. (Pp. xvi + 272.) Price 16*s*. 6*d*. net.

From Newton to Einstein. Changing Conceptions of the Universe. By Benjamin Harrow, Ph.D. London : Constable & Co., 1920. (Pp. x + 95.) Price 2*s*. 6*d*. net.

Experimental Science. I. Physics. By S. E. Brown, M.A. (Cantab.)., B.Sc. (Lond.) Head Master of Liverpool Collegiate School, Formerly Senior Science Master at Uppingham. Section V. Light. Cambridge : at the University Press, 1920. (Pp. vii + 424.) Price 5*s*. net.

Easy Lessons in Einstein. A discussion of the non-intelligible features of the Theory of Relativity. By Edward E. Slosson, M.C., Ph.D. London : George Routledge & Sons ; New York : Harcourt, Brace & Howe, 1920. (Pp. vii + 128.) Price 5*s*. net.

Physics. The Elements. By Norman Robert Campbell, Sc.D., F.Inst. P. A Member of the Staff of the Research Laboratories of the General Electric Company, Limited, London. Cambridge : at the University Press, 1920. (Pp. ix + 565.) Price 50*s*. net.

Organic Chemistry for Medical, Intermediate Science and Pharmaceutical Students. By A. Killen Macbeth, M.A., D.Sc., F.I.C., etc. Lecturer on Organic Chemistry, Queen's University, Belfast. London : Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. + 235.) Price 6*s*. 6*d*. net.

Directions for a Practical Course in Chemical Physiology. By W. Cramer, Ph.D., D.Sc., M.R.C.Sc., L.R.C.P. Fourth Edition. London: Longmans, Green & Co., 1920. (Pp. viii + 137.) Price 4*s*. 6*d*. net., interleaved 5*s*. net.

A Treatise on Chemistry. By the Right Honourable Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. Volume I. The Non-metallic Elements. Fifth Edition, completely revised by Dr. J. C. Cain. London: MacMillan & Co., St. Martin's Street, 1920. (Pp. xv + 968, with 226 illustrations and one portrait.) Price 30*s*. net.

Practical Chemistry. Fundamental Facts and Applications to Modern Life. By N. Henry Black, A.M., Science Master, Roxbury Latin School, Boston, Mass., and James Bryant Conant, Ph.D. Assistant Professor of Chemistry, Harvard University. New York: The Macmillan Company, 1920. (Pp. x + 474.) Price 11*s*. net.

Recent Advances in Physical and Inorganic Chemistry. By Alfred W. Stewart, D.Sc. Professor of Chemistry in the Queen's University of Belfast. With an Introduction by Sir William Ramsay, K.C.B., F.R.S. Fourth Edition. London: Longmans, Green & Co. 1920. (Pp. xvi + 286, with 25 illustrations and a Curve of Atomic Volumes.) Price 18*s*. net.

The Volatile Oils. By E. Gildemeister and Fr. Hoffmann. Second Edition. By E. Gildemeister, written under the auspices of the firm of Schimmel & Co., Miltitz near Leipzig. Authorised Translation by Edward Kreemers, Madison, Wis. Second Volume. London: Longmans, Green & Co., 39 Paternoster Row, 1916. (Pp. xx + 686, with 4 maps, 3 tables and numerous illustrations.) Price 32*s*. net.

The Experimental Basis of Chemistry. Suggestions for a Series of Experiments illustrative of the Fundamental Principles of Chemistry. By Ida Freud, sometime Staff Lecturer and Associate of Newnham College, Cambridge. Edited by A. Hutchinson, M.A., Fellow of Pembroke College, University Demonstrator of Mineralogy, and M. Beatrice Thomas, Lecturer in Chemistry, Girton College, Cambridge. Cambridge: at the University Press, 1920. (Pp. xvi + 408). Price 30*s*. net.

The Manufacture of Sugar from the Cane and Beet. By T. H. P. Heriot, F.I.C. Lecturer on Sugar Technology at the Royal Technical College, Glasgow. London: Longmans, Green & Co., 1920. (Pp. x + 426, with 39 illustrations). Price 24*s*. net.

Water Plants. A Study of Aquatic Angiosperms. By Agnes Arber, D.Sc., F.L.S. Fellow of Newnham College, Cambridge, and Keddey Fletcher-Warr, Student of the University of London. Cambridge: at the University Press, 1920. (Pp. xvi + 436, with frontispiece and 171 text-figures). Price 31*s*. 6*d*.

Plantation Rubber and the Testing of Rubber. By G. Stafford Whitby, Ph.D., M.Sc., A.R.C. Sc., Assistant Professor, Department of Chemistry, McGill University. London: Longmans, Green & Co. 39 Paternoster Row, 1920. (Pp. xvi + 555, with 8 plates and 48 text figures.) Price 28*s*. net.

Text-Book of Pastoral and Agricultural Botany. For the Study of the Injurious and Useful Plants of Country and Farm. By John W. Harschberger, Ph.D. Professor of Botany, University of Pennsylvania. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. 294, xiii + with 121 illustrations.)

Studies in Fossil Botany. By Dukinfield Henry Scott, M.A., LL.D., D.Sc., Ph.D., F.R.S., F.L.S., F.G.S., F.R.M.S. Lately Honorary Keeper of the Jodrell Laboratory, Royal Botanic Gardens, Kew. Third Edition. Vol. I. Pteridophyta. London: A. & C. Black, Ltd., 4 Soho Square, W.1, 1920. (Pp. xxiii + 434, with 190 figures.) Price 25*s.* net.

A Text-book of Plant Biology. By W. Neilson Jones, M.A., F.I.S. Professor of Botany in the University of London (Bedford College) and M. C. Rayner, D.Sc. Late Lecturer in Charge, Department of Botany, University College, Reading. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 262, with 6 plates.) Price 7*s.* net.

The Influence of Man on Animal Life in Scotland. A Study in Faunal Evolution. By James Ritchie, M.A., D.Sc. (Aberdeen), F.R.S.E., Assistant Keeper in the Natural History Department of the Royal Scottish Museum (Scottish Education Department), Cambridge: at the University Press, 1920. (Pp. xvi + 550, with 90 figures and 8 maps.) Price 28*s.* net.

The Great Cycle. A Monograph on the Scheme of Creation. By Sir Evelyn de la Rue, Bart. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xii + 209.)

The System of Animate Nature. The Gifford Lectures delivered in the University of St. Andrews in the years 1915 and 1916. By J. Arthur Thomson, M.A., LL.D. Professor of Natural History in the University of Aberdeen. In two Volumes. London: Williams & Norgate, 1920. (Pp. Vol. I. xi + 347, Vol. II. v + 353–687.) Price 30*s.* net.

Initiative in Evolution. By Walter Kidd, M.D., F.R.S.E. London: H. F. and G. Witherby, 326 High Holborn, 1920. (Pp. x + 262, with 80 figures.) Price 15*s.* net.

Geology of the Mid Continent Oilfields, Kansas, Oklahoma and North Texas. By T. O. Bosworth, D.Sc. (London), M.A. (Cambridge), F.G.S., F.R.G.S. Member of the Institute of the Petroleum Technologists. New York: The Macmillan Company, 1920. (Pp. xiv + 315, with 8 photographs and 24 figures and maps.) Price 16*s.* net.

Notes on Geological Map-reading. By Alfred Harker. Cambridge: W. Heffer & Sons, 1920. (Pp. 64.) Price 2*s.* net.

A Text-book of Geology. By Philip Lake, M.A., F.G.S. Royal Geographical Society Reader in Geography in the University of Cambridge, and R. H. Rastall, M.A., F.G.S., Lecturer in Economic Geology in the University of Cambridge. Third Edition. London: Edward Arnold, 1920. (Pp. xiv + 508, with 23 plates.) Price 21*s.* net.

Industrial Colonies and Village Settlements for the Consumptive. By Sir German Woodhead, K.B.E., V.D., M.A., M.D., LL.D., and P. S. Varrier Jones, M.A., M.R.C.S., L.R.C.P., with Preface by Sir Clifford Allbutt, K.C.B., M.A., M.D., LL.D., D.Sc., F.R.C.P., F.R.S. Cambridge: at the University Press, 1920. (Pp. x + 151.) Price, Paper Covers, 9*s.* net, and cloth 10*s.* 6*d.* net.

The Elements of Practical Psycho-Analysis. By Paul Bousfield, M.R.C.S. (Eng.), L.R.C.P. (Lond.). Physician to the London Neurological Clinic (Ministry of Pensions). London: Kegan Paul, Trench, Trubner & Co.; New York: E. P. Dutton & Co., 1920. (Pp. xii + 276.) Price 10*s.* 6*d.*

Feeble-mindedness in Children of School-age. By C. Paget Lapage, M.D., M.R.C.P., with an appendix on Treatment and Training by Mary Dendy, M.A. Second Edition. Manchester: at the University Press, 12 Lime Grove, Oxford Road; London: Longmans, Green & Co., 1920. (Pp. xv + 309.) Price 10*s.* 6*d.* net.

Warfare in the Human Body. Essays on Method, Malignity, Repair and Allied Subjects. By Morley Roberts, with an Introduction by Prof. Arthur Keith, M.D., F.R.C.S., F.R.S. London: Eveleigh Nash Company. (Pp. xiii + 386) Price 18*s*. net.

Diagnosis and Treatment of Brain Injuries with and without a Fracture of the Skull. By William Sharpe, M.D. Professor of Neurologic Surgery, New York Polyclinic Medical School and Hospital. Philadelphia and London: J. B. Lippincott Company. (Pp. vii + 757, with 232 illustrations.) Price 35*s*. net.

The Serbian Epidemics of Typhus and Relapsing Fever in 1915. Their Origin, Course, and Preventive Measures employed for their arrest. By William Hunter, C.B., Colonel A.M.S. Reprinted from the Proceedings of the Royal Society of Medicine, 1919. (Vol. xiii, pp. 29 + 158.). London: John Bale & Sons & Danielsson, 83 Great Titchfield Street, Oxford Street, W.1, 1920.

Standard Nomenclature of Diseases and Pathological Conditions, Injuries and Poisonings for the United States. Department of Commerce, Bureau of the Census. Sam. L. Rogers, Director. Washington: Government Printing Office, 1920. (Pp. 247.)

Scientific and Applied Pharmacognosy. Intended for the Use of Students in Pharmacy as a Handbook for Pharmacists, and as a Reference Book for Food and Drug Analysts and Pharmacologists. By Henry Kraemer, Ph.B. (in Chemistry), Ph.M. (in Pharmacy), Ph.D. (in Botany) Second Edition, thoroughly revised. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. xxviii + 741, with 300 plates.) Price 33*s*. net.

American Civil Engineer's Handbook. Editor-in-Chief Mansfield Merriman, together with 18 Associate Editors. Fourth Edition. Thoroughly revised and enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. 1955.) Price 33*s*. net.

George Stephenson. By Ruth Maxwell, M.A. London: George G. Harrap & Co., 2 and 3 Portsmouth Street, Kingsway, W.C. (Pp. 192, with 9 illustrations.) Price 3*s*. 6*d*. net.

Optical Projection. A Treatise on the Use of the Lantern in Exhibition and Scientific Demonstration. By Lewis Wright. Fifth Edition, rewritten and brought up to date by Russell S. Wright, M.R.E.E. In two Parts: Part I, The Projection of Lantern Slides. London: Longmans, Green & Co., 1920. (Pp. viii + 87). Price 4*s*. 6*d*. net.

Geometrical Investigation of the Formation of Images in Optical Instruments. Embodying the Results of Scientific Researches conducted in German Optical Workshops. Edited by M. von Rohr (forming Vol. I. of "The Theory of Optical Instruments"). Translated by R. Kanthack. London: Printed and Published for the Department of Scientific and Industrial Research by His Majesty's Stationery Office, 1920. (Pp. xxiii + 612, with 133 text figures.) Price £2 5*s*. net.

Rudiments of Electrical Engineering. By Philip Kemp, M.Sc., Tech. M.I.E.E. London: Macmillan & Co., St. Martin's Street, 1920. (Pp. viii + 255.) Price 6*s*. net.

A Treatise on Air-screws. By Whyrell S. Park, A.R.C.Sc., Whitworth Scholar, Designer to Long Propeller Ltd., London: Chapman & Hall, 11 Henrietta St., W.C.2. 1920. (Pp. xii + 308, with figures.) Price 21*s*. net.

Opinion d'un Linguiste sur la Langue Artificielle, Extrait du Livre " Les Langues dans l'Europe Nouvelle " de A. Meillet, Professeur au College de France. Traduction en Ido avec le texte en regard par L. de Beaufront, Président de la Société Idiste Française. Paris : Imprimerie Chaix, 11 Boulevard Saint-Michel, 1919. (Pp. 52.) Price 1*s*.

William Sutherland: A Biography. By W. A. Osborne. Melbourne : Lothian Book Publishing Co.; London : The British Australasian, 51 High Holborn, W.C.1. (Pp. 102.) Price 7*s*. 6*d*. net.

The Human Atmosphere. (The Aura.) By Walter J. Kilner, M.A., M.B., M.R.C.P. etc. Late Electrician to St. Thomas's Hospital, London. London : Kegan Paul, Trench, Trubner & Co.; New York : E. P. Dutton & Co., 1920. (Pp. vii + 300, with 64 illustrations.) Price 10*s*. 6*d*. net.

The History of Social Development. By F. Muller Lyer, Translated by Elizabeth Coote Lake and H. A. Lake, B.Sc. (Econ.) F.R.A.I., with an introduction by Professors L. T. Hobhouse and E. J. Urwick. London : George Allen & Unwin. 40 Museum Street, W.C.1. (Pp. 362.) Price 18*s*. net.

Primitive Time-reckoning A Study in the Origins and First Development of the Art of Counting Time among the Primitive and Early Culture Peoples. By Martin P. Nilsson. Professor of Classical Archæology and Ancient History in the University of Lund, Secretary to the Society of Letters of Lund, Member of the R. Danish Academy. Lund : C. W. K. Gleerup; London: Oxford University Press; Paris: Édouard Champion; Leipzig: O. Harrassowtiz, 1920. (Pp. xiii + 384.) Price 21*s*. net.

A Study in Realism. By John Laird, M.A., Professor of Logic and Metaphysics in the Queen's University of Belfast. Cambridge : at the University Press, 1920. (Pp. xii + 228.) Price 14*s*. net.

Secretum Secretorum cum Glossis et Notulis. Tractatus Brevis et Utilis ad Declarandum Quedam Obscure Dicta. Fratris Rogeri, nunc primum Edidit Robert Steele. Accedunt Versio Anglicana ex Arabico Edita, per A. S. Fulton. Versio vetusta Anglo-Normanica nunc Primum Edita. Opera hactenus inedita Rogeri Baconi, Fasc. V. Oxonii: E. Typographeo Clarendoniano, 1920. (Pp. lxiv + 317). Price 28*s*. net.

The Centenary Volume of Charles Griffin & Company Ltd., Publishers 1820–1920, with Foreword by Lord Moulton, P.C., G.B.E., K.C.B., F.R.S. London: Charles Griffin & Co., 12 Exeter Street, 1920. (Pp. xx + 290, with illustrations.)

The Fringe of Immortality. By Mary E. Monteith. London : John Murray, Albemarle Street, W., 1920. (Pp. xiv + 204.) Price 6*s*. net.

Instinct and the Unconscious. A Contribution to a Biological Theory of the Psycho-Neuroses. By W. H. R. Rivers, M.D., D.Sc., LL.D., F.R.S., Fellow and Praelector in Natural Sciences, St. John's College, Cambridge. Cambridge : at the University Press, 1920. (Pp. viii + 252.) Price 16*s*. net.

Coal in Great Britain. The Composition, Structure, and Resources of the Coal-fields, Visible and Concealed, of Great Britain. By Walcot Gibson, D.Sc., F.G.S. London: Edward Arnold, 1920. (Pp. viii + 311, with 50 figures.) Price 21*s*. net.

Burke. The Burke Publishing Co., 22 Suffolk St., Pall Mall, London, S.W. (Pp. 14.)

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By Dorothy M. Wrinch, D Sc, Member of the Research Staff, University College, London, and Fellow of Girton College, Cambridge.

Some remarkable problems of a new type arise in a paper by Messrs. R. H. Fowler, E. G. Gallop, C. N. W. Lock, and H. W. Richmond, devoted to the analysis—theoretical and experimental—of the aerodynamics of a spinning shell (*Phil. Trans.*, *A*, vol. ccxxi, pp. 295–387). The subject is, by its nature, remote in certain aspects from the interests of the pure mathematician, but there are other aspects which appear to indicate a large field of future work. The general problems of dynamics have previously suggested or made necessary some large fields of modern analysis, and in our view this problem will prove to be one of the sequence. Hitherto it has been quite untractable, but the combined experimental work and theoretical suggestions of the present authors clearly succeed in paving the way to an ultimate solution, while at the same time bringing clearly into view many points which may prove the determining factor in the future lines of development of, for instance, detailed discussion of solutions of specific differential equations. Applied mathematics has usually in the past dictated the choice of the differential equations whose solutions are worthy of detailed examination, as, for instance, in classical cases such as Legendre's or Bessel's equations, which afterwards take on an important rôle in function-theory quite independently of their historical origin. The general hypergeometric function has begun recently to play a similar important part in applied mathematics, its history being thus, in a certain way, the converse of that of such functions as those of Legendre and Bessel.

We cannot here enter into the more practical side of the paper under consideration, for it is outside our legitimate province. But it deals with shells which can have a velocity greater than that of sound—a significant advance in itself—and seeks to find the forces and couples acting on the shells during their motion. The drag, in the case of symmetry, is taken as

$$R = \rho v^2 r^2 f\left(\frac{v}{a}\right),$$

where (v, a) are the velocities of the shell and of sound in air,

and ρ is the density of air, and r the radius of the shell. The function f is numerical only, and the so-called drag coefficient, determined experimentally and with some precision when v/a ranges from 0 to 3.

The shell is discussed as a rigid solid of revolution, its axis of symmetry being coincident with a principal axis of inertia. The classical theory is confirmed so far as the divergencies of the axis of the shell from the tangent to its part are concerned, but the paper also makes a determination of the magnitude and effect of these divergencies.

The angle between the axis and the path of the centre of gravity is generally known as the *yaw*. The main force components are (1) the *drag*, acting through the centre of gravity directly against the motion; (2) the *cross-wind force*, perpendicular to the drag and in the plane of the yaw; and (3) a moment in the plane of the yaw. We have given the type of formula used for (1). Similar forms are used for (2) and (3), the functions f being elucidated by experiment, partly in wind channels and partly otherwise. Complications ensue when the shell has axial spin, and into these we do not enter. In practice, the direction of the axis of the shell relative to the direction of motion has rapid changes, and needs the introduction of a new couple called the *yawing moment*, again given by a similar formula. The authors make the assumption that (1), (2), and (3) are not appreciably affected by the existence of this new couple, and they justify the assumption *à posteriori*.

It is clear that the dynamical equations of motion of a body under the influence of such force-components are of a novel type, and, in relation to their solution, interesting problems of pure mathematics arise, as we have stated already. Three types of equations are dealt with, expressed in terms of different sets of co-ordinates, and two are solved. The nature of the problem, in any set of co-ordinates, will be clear. By the use of certain complex variables, much simplicity can be introduced into the differential equations, which are of an interesting form and with coefficients dependent on quantities which in practice are very large or very small. They are of a new type, and the ultimate object is the determination of asymptotic expansions for their solutions. In this way they enter very definitely into the domain of the pure mathematician. The authors use Horn's method, which is fairly familiar now. If an equation is reduced to the form

$$\frac{d^2y}{dx^2} + k^2w^2y = 0$$

where k is large, and w is a function of x, the asymptotic solutions are

$$y = (Ae^{ckw} + Be^{-ckw})\psi$$

where ψ is readily determinate by successive approximations. We cannot recall a very practical problem in which the application of this type of analysis to a new differential equation, and its rigorous elucidation on more logical lines, is more urgently needed, or more capable of leading to results of importance not only in relation to applied mathematics. It is clear that developments on these lines may be expected, for the dynamics of a spinning shell is not, in its essence, different from the very progressive dynamics, with all its associated problems for the pure analyst, to which modern theory of aeroplanes has introduced us.

The appearance of Dr. Brodetsky's book entitled, *A First Course on Nomography* (G. Bell & Sons, 10*s*.), marks an opportune moment at which to direct the attention of mathematical readers to this subject. Hitherto it has hardly been possible to study the matter except from the larger and very comprehensive work of D'Ocagne (*Traité de Nomographie*, Gauthier Villars, Paris, 1899), to whom so many useful nomograms are due. The interest attaching to this subject is little known. It is perhaps no exaggeration to say that many pure mathematicians have never even met a nomogram, except, perhaps, the slide-rule, which, in its essence, is a collection of simple nomograms, though most of these are not quite of the more usual types to which this name is applied. Nomograms are of great utility in practice, and are in constant use in many industrial problems in which the continued solution of mathematical equations of certain types—and these by no means always of the simplest—is required. They could be made of equal service to the applied mathematician, and often even to the pure mathematician, when numerical solutions of a problem are of value.

Descartes, in inventing Co-ordinate Geometry, laid the foundations of the most powerful methods of mathematics. Buache, in the eighteenth century, followed it up by the method of contours, which made it possible for three quantities to be dealt with at once instead of two only. Further developments —stimulated by the extraordinary growth of railway systems— of graphical methods were made in the nineteenth century by Lalanne, Massau, and Lallemand. D'Ocagne, in 1884, hit on the method of using collinear points, which is used in nomography.

The essence of nomography is this. By means of one diagram we can read off the solutions of any equation of a certain type. Take a simple case : $x^2 + ax + b = o$. Children at school laboriously draw the graph of $y = x^2 + ax + b$, notice

where it cuts the axis of x, and write down the solutions. The nomogram of this type of equation enables us to read off from the diagram the roots of the equation for any suitable values of a and b. Other nomograms enable us to read off solutions of equations of types such as, for example,

$$x^3 + ax + b = 0,$$
$$a \tan x + b \sec x + 1 = 0,$$
$$bx = a^x, \ a^x = x^b$$

for various sets of values of a and b. It is evident that it is of great utility, in applied mathematics, to be able to do this ; for it means a very great deal of labour to solve even a simple equation like $a \tan x + b \sec x + 1 = 0$, when a and b have special values, and to solve a set of equations of this type is a considerable labour. It is by no means impossible that diagram sheets should be made once and for ever which would solve the various types of equation, and then they could be printed and made available for all mathematicians who need them. They would then be as much the stock-in-trade of an applied mathematician as the tables of logarithms and sines and cosines.

The theory of nomography is merely certain parts of the theory of ordinary co-ordinate geometry carefully manipulated.

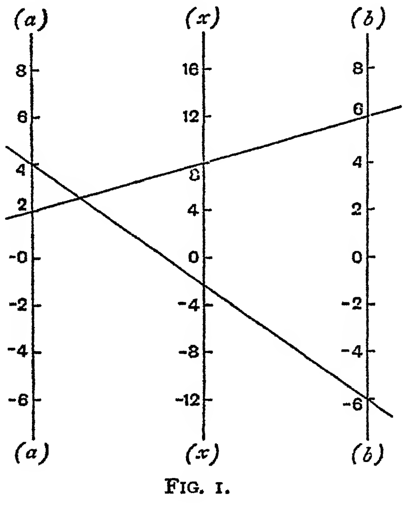

FIG. 1.

There is nothing essentially new in it beyond the application of certain well-known results to accomplish certain graphical ends. We begin, for instance, with the graphical representation of a and b. Take three scales, a, b, x (Fig. 1). Let a and b be

graduated similarly, and x graduated with half the unit used in a, b. Setting them all parallel with x equidistant from a and b, we get a nomogram for $x = a$ and b. For take (fig. 1), *e.g.*, the point $+ 6$ on (b), $+ 2$ on (a); join them; then the line cuts (x) in the point 8, giving the value of $6 + 2$. Take, again, the point $- 6$ on (b) $+ 4$ on (a); join them. This time the line cuts (x) in the point $- 2$, giving the value of $- 6 + 4$. Joining up, then, pairs of points, we may get the value of $a + b$ for various values of a and b shown on the x scale.

To get a nomographic chart for $x = a + 2b$, take scales (a) and (b), as before, graduated with the same unit, but let the third scale (x) be twice as far from a as it is from b, and let its unit of graduation be one-third of the unit used in a, b (Fig. 2).

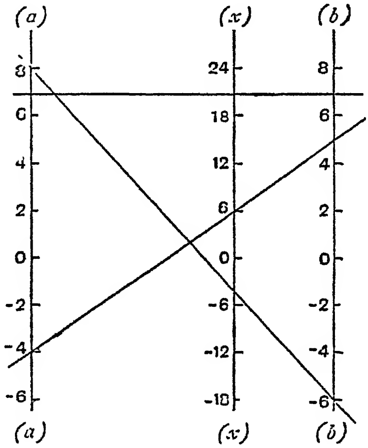

FIG. 2.

Then the collinearity of a set of points—a, b, x—on the three scales means that three times distance x = distance a + twice distance b; and, since the x-unit is one-third of the a and b units, it follows that graduation x = graduation a + twice graduation b. The lines drawn show the particular results

$$8 + 2\,(-6) = -4$$
$$-4 + 2.5 = 6$$

and the line perpendicular to the scales the result $7 + 2.7 = 21$. Obviously, very accurately drawn scales would be of value, even in such simple matters as multiplication, for by adjusting the distances between the scales, various multiplication tables could be constructed.

The generalised nomogram giving $x = la + mb$, when l and m are rational numbers, easily follows, but is, of course, only

important in practical nomography when l and m are simple numbers, such as 2 or 5 or $\frac{1}{4}$.

Suppose now we introduce another scale, y, and so graduate x and y that the two nomograms

$$2x = a + b\,;\ 3y = a + 2b$$

are represented.

To solve the equations

$$a + b = 7,\ a + 2b = 12$$

we have only to take $x = 7/2$ and $y = 12/3$, and read off the values of a and b registered by the line joining these two points. Immediate generalisations follow which enable us to solve, *e.g.*, the equations

$$3a + 2b = x,\ a + 6b = y$$

(in which the graduations used are $x/5$ and $y/7$), when x and y are the unknowns and a and b have all suitable values. Finally, we can make an elaborate chart to solve the equations $la + mb = x$, $l^1a + m^1b = y$ (l, l^1, m, m^1, being whole numbers), obtaining it merely by drawing in the various x and y scales required for the various values of l, m, l^1, m^1.

This discussion of the question of solving simultaneous equations should give a general impression of the methods used in nomography. The algebraic significance of the collinearity of points on variously placed scales is the essence of the matter, and nomograms as a whole may be defined as being the various classes of relations which result when variously placed scales are variously graduated.

Scales used are not always in the form of straight lines. The nomogram for $x^2 + ax + b = 0$ consists of two parallel straight line scales, and a symmetrically placed parabola with its vertex between the two. Another nomogram for the same type of equation consists of two similarly graduated scales and one branch of a hyperbola.

The general theory of nomograms with two parallel scales is worked out in Dr. Brodetsky's book. For the more general theory, reference should be made to the classic of the subject.

APPLIED MATHEMATICS. By S. Brodetsky, M.A., Ph.D., F.Inst.P., A.F.R.Ae.S., University, Leeds.

Profound modifications of our view of space, time, and force are called for by Einstein's Theory of Relativity, and, whether we accept the theory in its complete form or not, we must in any case review carefully the basis on which our physical science is built. It should, therefore, be of interest to the applied mathematician to realise in what way the theory affects

the fundamental conceptions and the hitherto accepted laws of dynamics. The foundations of classical mechanics, as enunciated in Newton's Laws of Motion, are laid in an unyielding soil of Euclidean space of the type we are made familiar with from the first beginnings of our geometrical experience and teaching, using what is now termed a Galilean frame of reference. Einstein's philosophical and mathematical reasoning, and the results he obtained, verified *à priori* like that referring to the perihelion of Mercury, or *à posteriori* like the bending of rays of light when passing through a strong gravitational field, can be treated in one of two ways. The counsel of caution is to accept the new facts thus established and to incorporate them in the traditional system of physical science. Thus, the bending of light is to be taken as establishing a connecting link between light and gravitation, whilst the explanation of the motion of the perihelion of Mercury is to be taken as showing that Newton's law of gravitation needs modification when great accuracy is aimed at. The bolder view is to accept the mighty structure of Einstein's theory, with the embarrassing consequence that space loses its invariable Euclidean character—in fact, Euclidean space no longer exists anywhere, since every part of what we call space is occupied by a gravitational field; there is, further, the additional consequence that the notion of force as underlying motion loses all meaning, what we have hitherto called force being merely a reflex of the properties of the space in which the motion takes place.

No doubt it seems easier for the applied mathematician to adopt the counsel of caution, for this is compatible with the survival of the classical notions of mass and force. It is, however, hardly a logical proceeding to accept new facts without working these facts into a system consistent with our former possessions. The old Newtonian view of gravitation possessed the charm of simplicity, and the advantage of fitting in with the Euclidean space that underlies classical mechanics. Moreover, the bending of light in a gravitational field presents the difficulty that the predicted and verified amount of bending is double what we would be led to expect if we merely endow visual radiation with gravitational properties.

If there are difficulties about adopting the view that the bending of light and the explanation of the motion of Mercury merely add new facts to the established system of mechanics, a revolution in our ideas is involved in the acceptance of the complete superstructure of theory that has been raised on these facts. We are concerned here with the mechanical consequences of the theory, and, put in brief, they appear to be as follows.

In studying natural phenomena we have hitherto started off with a definite notion of the properties of space, namely, the Euclidean. The various phenomena have been explained on a mechanical basis in the sense that motion and changes of motion considered vectorially have been referred to forces: a considerable fraction of scientific work has in fact consisted in the devising of systems of forces to explain the observed motions, and in using these systems in order to predict motions yet to be observed, leading to verification or modification of the original hypotheses.

The new theory reverses the procedure. We have no right to postulate *à priori* the properties of space in which phenomena take place. If we accept the fundamental notion of generalised relativity, namely, the Principle of Equivalence, then our investigations of natural phenomena, in so far as they relate to motion, must not be directed towards the devising of systems of forces which explain the observed motions in an already postulated space, but must be directed rather towards the devising of a space in which the motions observed are just as natural as uniform motion in a straight line seems to be natural in a Galilean frame of reference. We are accustomed to this kind of investigation in ordinary mechanics, where many phenomena are attributed to so-called centrifugal forces: in other words, we explain the phenomena by showing that they are what we would expect to obtain with reference to a frame which is not at rest but moving in some assigned manner, as, for example, rotating about a fixed axis. The theory of moving axes in particle dynamics, and the complicated expressions obtained for the linear and angular momenta of a rigid body when referred to axes fixed in the body, are also familiar instances of the same idea. Einstein's principle of equivalence is this idea pushed to its logical extreme as far as gravitation is concerned; the motions that we are accustomed to refer to gravitational forces are now to be explained by assuming properties of space defined by these very motions. Henceforth the motion of a body in space does not define the forces acting on the body, but the properties of the space in which the body moves.

It follows at once that the classical laws of motion need revision if we accept the principle of equivalence: we refer to the first and second laws. The second law disappears as a separate law. The principle of equivalence eliminates the objective existence of all forces, gravitational as well as centrifugal, and the first law of motion must be restated in a modified form so as to include the second law. The two laws are condensed into the single statement that the path of a particle is along a geodesic in the space in which it moves.

There is no inherent difficulty in this point of view. Although in mechanics we ordinarily deal with forces as if they were something like concrete entities, we are nevertheless conscious of the vagueness of the conception of force. The elementary student may sometimes acquire the notion that a force is a sort of poker used for poking about masses, but in reality all we can say about forces is that they explain the motions—in other words, we invoke the aid of abstract entities called forces. Why, then, should we object to invoking any other aid that enables us to explain the motions with as much ease and with even greater accuracy?

The real difficulty lies in the implication that is contained in the principle of equivalence, namely, that Euclidean space is a human myth, and that the nature and properties of space vary from place to place, whilst in any place they vary from time to time. The relativist has a perfect right to urge that physical space, as distinct from the purely abstract philosophical space, must have its properties determined by experimental evidence, and not by pure prejudice. The applied mathematician may, nevertheless, deserve some consideration if he attempts to reconstruct the Einstein theory in such a way that, while accepting the fundamental idea of relativity, he need not discard the space that has stood him in such good stead for so many centuries.

Attempts in this direction are the natural consequence of the reawakening of thought about the foundations of mechanics. At the time of writing no definite results have yet emerged, but interesting developments are to be looked for in the near future.

The applied mathematician will be interested in the following papers, among others, which deal with the dynamical aspect of generalised relativity:

Jeffreys, H., Crucial Test of Einstein's Gravitation Theory, *M.N., R.A.S.*, 1919, **80**, 138–54, where it is shown that the motion of the perihelion of Mercury cannot be explained by the presence of matter near the sun.

Crommelin, A. C. D., Comets with Small Perihelion Distance, and the Resisting Medium, *ibid.*, 1920, **80**, 475–7, where the author considers the evidence from cometary motions as to the existence of such an atmosphere round the sun as would explain the motion of the perihelion of Mercury; the result is negative.

Sampson, R. A., On the Validity of the Principles of Relativity and Equivalence, *ibid.*, 1919, **80**, 154–7, touching on the problem of rotation.

Page, L., The Einstein Deflection for High Speed Material Particles, *Phys. Rev.*, xv, **4**, 1920, 335. The author discusses the motion of a particle towards a centre of force on the Einstein theory, and deduces peculiar results, showing that, under certain conditions, the particle's velocity diminishes as it approaches the centre. A similar statement is given in *Nature*, **104**, 1920, 692–3. A comment by A. S. Eddington is to be found *ibid.*, **105**, 1920, 37.

SLATE, F., An Alternative View of Relativity, *Phil. Mag.* (vi), **39**, 1920, 433–9; the author's view is that relativists need not reject Newtonian dynamics, which can be so extended as not to contradict relativity.

MAJORANA, L., Expériences sur la gravitation, *Comptes Rendus*, **169**, 1919, 719–21, discussing the absorption of gravitation in passing through masses.

FORSYTH, A. R., Note on the Central Differential Equation in the Relativity Theory of Gravitation, *Proc. Roy. Soc.*, **97**, 1920, 145–51, where it is shown that the motion of a planet round the sun according to Einstein's theory can be solved in terms of elliptic functions.

The impetus given to hydro- and aero-dynamical research by the development of the science and art of aeronautics is producing work of importance both to the applied mathematician and to the aeronautical and marine engineer. A recent and important contribution to the study of fluid resistance and of motion in resisting media is the *Bulletin de l'Institut Aérodynamique, fasc.* vi, 1920, containing a number of papers by D. P. Riabouchinski, working at Koutchino. By the use of a function defined in a discontinuous manner, the author discusses problems on the discontinuous motion of fluids with various types of barriers. He also investigates the resistance of air at high velocities, and further work includes the autorotation of a projectile in air (due to suction caused by the formation of vortices behind the projectile), the efflux of gas from a receptacle, and air-screws.

Of equal importance, from another point of view, is *The General Theory of Blade-screws* (*First Memoir*), by George de Bothezat, published as Report No. 29 by the National Advisory Committee for Aeronautics, Washington, U.S. A., 1919. Bothezat has long been engaged in this work (see SCIENCE PROGRESS, xiii, 1918, 187); he worked in Russia till the revolution, and has continued his work in America. While one finds it difficult to assent to the author's opinion that the theory of blade-screws is an important state question for every Government, and that special laboratories should be established to study screws in the light of his theory, one cannot doubt the importance of the subject and of his theory. The theory is ambitious and includes "Propellers, Fans, Helicopter Screws, Helicoidal Pumps, Turbo Motors, and different kinds of Helicoidal Brakes." After giving the fundamental equations relating to blade-screws based on what Bothezat calls the specific function, which is a function of the relative pitch, the author proceeds to distinguish sixteen states of work of blade-screws, including the vortex ring state and whirling phenomena. The most important section of the report deals with the propulsive screw or propeller, and nomograms are given for finding solutions on the author's theory. A note of considerable interest gives a generalisation of Bernoulli's theorem to include vortex motion.

Another paper on the subject of propellers is by A. Plateau, entitled "Théorie des hélices propulsives marines et aériennes et des avions en vol rectiligne," *Comptes Rendus*, **170**, 1920, 1360–2.

The problem of fluid-resistance presents several difficulties. On the one hand, there is the difficulty of finding the pressure on any given type of barrier past which air is flowing. On the other hand, the elasticity of the air introduces effects which are no doubt considerable in the case of air-screws, whilst the viscosity too needs investigation. Theory must be combined with experiment, and several attempts to solve such problems are recorded in recent papers. The following are a selection on this and kindred topics :

Bairstow, L., Towler, R. H., and Hartree, D. R., The Pressure Distribution on the Head of a Shell moving at High Velocities, *Proc. Roy. Soc.*, **97**, 1920, 202–18.

Thurston, A. P., Distribution of Air Pressure round the Funnel of a Vessel at Sea, *Engineering*, **119**, 1920, 134–5.

Théry, R., Sur un problème hydrodynamique admettant une infinité de solutions, *Comptes Rendus*, **170**, 1920, 656–8.

Fournier, F. E., Formes de carènes de moindre résistance à leur translation à air libre à toutes les vitesses, *ibid.*, **170**, 1920, 547–52, 694–9.

Whittemore, J. K., The Starting of a Ship, *Proc. Nat. Acad. Sci., U.S.A.*, vi, 3, 1920, 182–5 ; the author discusses the motion of a particle moving under tangential forces depending only on the velocity of the particle, showing its application to marine engineering and to the study of resistance.

Noguès, P., Le vol à voile par vent horizontal de vitesse et de direction invariables, *Comptes Rendus*, **170**, 1920, 65–8, where the author tries to show how such a wind can be a source of energy to enable a bird to fly like a sailing-boat. The argument and result are controverted by Villey, J., and Volmerange, A., *ibid.*, **170**, 1920, 838–41.

Darwin, Sir H., The Static Head Turn Indicator for Aeroplanes, *Aer. Journ.*, xxiii, 1920, 617–31.

Rateau, A., Sur les plus grandes distances franchissables par les avions et les plus grandes vitesses réalisables, *Comptes Rendus*, **170**, 1920, 364–70 ; the author comes to the conclusion that a single flight of 5,000 or 6,000 kilometres is possible, but he is doubtful whether a flight of 7,000 kilometres can be attained.

Rateau, A., Sur l'altitude de vol qui correspond au minimum de consommation kilométrique, et sur le calcul de la meilleure hélice pour un avion donné, *ibid.*, **170**, 1920, 491–7.

Marvin, C. F., Flight of Aircraft and Deflective Influence of the Earth's Rotation, *Monthly Weath. Rev.*, **47**, 1919, 75–7.

Brewer, G., Some Kite Balloon Experiments, *Aer. Journ.*, xxiv, 1920, 15–36.

Cave, C. J. P., and Dynes, J. S., Further Measurements on the Rate of Ascent of Pilot Balloons, *Quart. Journ. Roy. Met. Soc.*, **45**, 1919, 277–83.

In addition there is a paper by H. Haedicke (*Zeit. Ver. Deutsch. Ing.*, **63**, 1919, 983–5), dealing with forces due to oblique air-streams ; one by F. Neeson (*Deutsch. Phys. Gesell.*, **21**, 1919, 589–93), dealing with the laws of relative motion in connection with the flight of projectiles in air ; and one by

A. Lechner (*Akad. Wiss. Wien*, 1918, 1629–42), where the author investigates the resistance to the motion of a sphere in a viscous fluid.

What we may call the traditional problems of mechanics are receiving their due share of attention, and we pick out for special mention some of the aspects of celestial mechanics. The problems of two and of three bodies are the subjects of a number of recent papers. Thus, H. C. Plummer discusses Prof. Howe's method of solving Kepler's equation, and shows how the method can be used to get very accurate results with comparatively little labour (*M.N., R.A.S.*, **80**, 1919, 207–11). The capture theory for binary stars involves the study of orbits in the problem of three bodies. L. Becker applies mechanical quadratures to examine symmetrical orbits for the case of three equal masses (*M.N., R.A.S.*, **80**, 1920, 590–7). A note by the present writer shows how the fundamental equation in the theory of central orbits can be derived from first principles (*Proc. Edin. Math. Soc.*, xxxviii, 1919–20, 51–2). Other papers are :

Strömgren, E., Solutions in the General Problem of Three Bodies, *M.N., R.A.S.*, **80**, 1919, 12–22.
Levi-Civita, T., Sur la régularisation du problème des trois corps, *Acta Math.*, **42-2**, 1919, 99–144.
Plano, J. M., El Problemo de los tres cuerpos, *Rev. Mat. Hisp.-Amer.*, **1**, 1919, 172–7.
Buchanan, D., Asymptotic Satellites near the Straight-line Equilibrium Points in the Problem of Three Bodies, *Amer. Journ. Math.*, xli, 1919, 79–110.
Buchanan, D., Asymptotic Satellites near the Equilateral-Triangle Equilibrium Points in the Problem of Three Bodies, *Trans. Camb. Phil. Soc.*, xxii, 1919, 309–40.
Chazy, J., Sur les singularités impossibles du problème des *n* corps, *Comptes Rendus*, **170**, 1920, 575–7.

In the more general theory of dynamics the following investigations should be consulted :

Ogura, K., On the Conservative Field of Force, *Tohôku Math. Journ.*, **17**, 1920, 1–6.
Andrade, J., Extension des systèmes conservatifs et généralisation d'un théorème de M. Painlévé, *Comptes Rendus*, **170**, 1920, 835–7.
Appell, P., Sur une application élémentaire d'une méthode générale donnant les équations du mouvement d'un système, *Nouv. Ann. Math.*, 1919, 121–31.
Livens, G. H., On Hamilton's Principle and the Modified Function in Analytical Dynamics, *Proc. Roy. Soc. Edin.*, xxxix, 1919, 113–19.

The mathematical theory of gravitation from the point of view of the potential function forms the subject of several notes and papers. C. Rosenblatt considers the theorem, due to Liapounoff, that of all bodies having a given volume, the sphere

has least potential energy (*Comptes Rendus*, **170**, 1920, 510–11). K. Sen investigates the potentials of elliptic cylinders and of incomplete ellipsoids and elliptic discs (*Bull. Calc. Math. Soc.*, 1919, **11-27**, 157–78), whilst G. Prasad takes the case of a surface distribution having a discontinuity (*ibid.*, 1919, 1–11), and S. C. Dhar considers Joachimstahl's Attraction Problem (*ibid.*, 1919, 151–6). Of a more theoretical nature are the note by G. Bouliyaud, " Sur le problème de Dirichlet par un domain infini " (*Comptes Rendus*, **169**, 1919, 1020–3), and the paper by P. Lévy, " Sur l'allure des fonctions de Green et de Neumann dans le voisinage du contour " (*Acta Math.*, **42-3**, 1919, 207–67).

Other recent papers, dealing with various branches of applied mathematics, are :

STATICS AND DYNAMICS

FAWDRY, R. C., The Teaching of Mechanics to Beginners, *Math. Gaz.*, x, 1920, 30–4, containing a discussion of interest to teachers of mechanics in schools.

THÉBAULT, V., Distance du centre de la sphère circonscrite au centre de gravité du tétraédre, *Nouv. Ann. Math.*, 1919, 424–6.

BIMBI, J. L., Principio de los trabajos virtuales. Applicaciones, *Cont. Estud. Cienc. fis. y mat.*, 1919, 9–71.

MARTON, W. B., and TOBIN, T. C., Note on the Construction of a Parabolic Trajectory and a Property of the Parabola used by Archimedes, *Phil. Mag.* (vi), **39**, 1920, 157–60.

HADAMARD, J., Rapport sur les travaux examinés et retenus par la Commission de Ballistique pendant la durée de la guerre, *Comptes Rendus*, **170**, 1920, 436–45 ; these investigations included the discussion of the cases where integration by quadratures is possible for the motion of a projectile in air for various laws of resistance, the actual calculation of paths and the perturbations of paths, the relation between the density of air and altitude, as well as internal ballistics and experimental work.

LE ROLLAND, P., De l'influence de la déformation du couteau et du plan de suspension sur la durée des oscillations du pendule, *ibid.*, **170**, 1920, 455–7.

JACKSON, L. C., Variably Coupled Vibrations, Gravity-Elastic Combinations, Masses and Periods Equal, *Phil. Mag.* (vi), **39**, 294–304, a continuation of Browning and Barton's work.

MILNE, J. R., A Duplex Form of Harmonic Synthetiser and its Mathematical Theory, *Proc. Roy. Soc. Edin.*, xxxix, 1920, 234–42 ; it is claimed that this obviates the obliquity error that troubled Lord Kelvin.

ANDRADE, J., Sur le controle expérimental des vibrations pendulaires doublement amorties, *Comptes Rendus*, **170**, 1920, 42–4, deals with harmonic motion in which there is a resistance varying as the speed and a constant frictional force.

ANDRADE, J., Sur le mouvement de l'axe d'un solide homogène pesant de révolution qui a un point fixe sur cet axe, *ibid.*, **170**, 1920, 1156.

BROWN, S. G., The Brown Gyro Compass, *Nature*, **105**, 1920, 44–8, 77–80.

GRAUMEL, R. (*Phys. Zeit.*, xx, 1919, 398–400), discusses the nutation of the unsymmetrical gyroscope.

DE FONTVIOLANT, B., Calcul des ponts circulaires, comportant un seul contreventement et des entretoisements dans toute leur longeur, *Comptes Rendus*, **170**, 1920, 376–9, 796–8.

RAMAN, C. V., Forced Vibrations of Strings, *Phys. Rev.*, **14**, 1919, 446–9.

RAMAN, C. V., Partial Tones of Bowed Strings, *Phil. Mag.* (vi), **38**, 1919, 573–81.

BANERJI, S., On the Forced Vibrations of a Heterogeneous String, *Bull. Calc. Math. Soc.*, ix, 1919, 43–58.

ELASTICITY

MAJUMDAR, N. K., On the Use of Ritz's Method for Finding the Vibration Frequencies of Heterogeneous Strings and Membranes, *ibid.*, x, 1920, 35–42.

NICHOLSON, J. W., The Lateral Vibrations of Sharply Pointed Bars, *Proc. Roy. Soc.*, **97**, 1920, 172–81, considers an extension of former work, the bars being of the form given by rotating $y = Ax^n$ about the axis of x, $n = 2$.

CAROTHERS, S. D., Plain Strain: the Direct Determination of Stress, *ibid.*, **97**, 1920, 110–23.

MICHELSON, A. A., The Laws of Elastico-Viscous Flow, *Proc. Nat. Acad. Sci., U.S.A.*, vi, 1920, 122–7.

RAMAN, C. V., On Some Applications of Hertz's Theory of Impact, *Phys. Rev.*, xv, 1920, 277–84, deduces a simple formula for the coefficient of restitution between a sphere and a thin plate.

RAMAN, C. V., and BANERJI, B., On Kaufman's Theory of the Impact of the Pianoforte Hammer, *Proc. Roy. Soc.*, **97**, 1920, 99–110.

MESNAGER, M., Solution élémentaire de la plaque rectangulaire encastrée, portant une charge uniformément répartie ou concentrée en son centre, *Comptes Rendus*, **169**, 1919, 1081–3.

HUBER, M.-T., Sur la généralisation d'un théorème de M. Mesnager concernant le sens des déplacements d'une plaque rectangulaire, *ibid.*, **170**, 1920, 1305–8.

AICHI, K., Forced Vibrations of a Circular Plate, *Proc. Phys. Math. Soc. Jap.*, **1**, 1919, 365–77.

ITERSON, F. K. T. v., Stresses in Thin Shells of Circular Section, *Engineering*, **105**, 1919, 640–2.

CODY, W. G., Note on the Theory of Longitudinal Vibrations in Rods having Internal Losses, *Phys. Rev.*, xv, 1920, 146–7.

PRESCOTT, J., appendix by CARRINGTON, H., The Buckling of Deep Beams (Second Paper), *Phil. Mag.* (vi), 39, 194–222, extends the work of the first paper, 1918, with experimental verification.

MESNAGER, M., Méthode de détermination des tensions existant dans un cylindre circulaire, *Comptes Rendus*, **169**, 1919, 1391–3.

KNOTT, C. G., Propagation of Earthquake Waves through the Earth, and Connected Problems, *Proc. Roy. Soc. Edin.*, xxxix, 1919, 157–208; as the result of a new determination of the laws of propagation of seismic waves, the author concludes that the earth is an elastic solid till about half-way towards the centre; the rigidity then begins to break down, and at about three-fifths of the radius towards the centre the elastic solid gives way to a non-rigid nucleus.

ROTATING FLUIDS

PLUMMER, H. C., On the Ellipticities of the Maclaurin Ellipsoids, *M.N., R.A.S.*, **80**, 1919, 26–33.

VÉRONNET, A., Figures d'équilibrium d'un liquide en rotation. Ordre de succession des figures contigues de bifurcation, *Comptes Rendus*, **170**, 1920, 1303–5.

HUMBERT, P., Les calculs de G. H. Darwin sur la stabilité de la figure piriforme, *ibid.*, **170**, 1920, 38–40, shows that Liapounoff's conclusion that the figure is unstable is correct.

PIDDUCK, B. F., The Vibration and Stability of a Rotating Cylinder, *Proc. L.M.S.*, 1920.

Hydrodynamics and Sound

Delage, Y., Sur un tube de Pitot intégrateur pour le mesure de la vitesse moyenne des courants variables, *Comptes Rendus*, **170**, 1920, 213–8.

Cook, G., An Experimental Determination of the Inertia of a Sphere moving in a Fluid, *Phil. Mag.* (vi), **39**, 1920, 350–2, obtains an addition to the inertia of 0·46 times the mass of an equal volume of fluid, the theoretical value being 0·5.

Metzner, P. (*Phys. Zeit.*, **20**, 1919, 536–42), discusses stream motion in fluids caused by rotating bodies.

Scheuerman, R. (*Ann. Phys.*, **60**, 1919, 233–59), discusses the jet of falling liquid.

Villat, H., Sur certains mouvements cycliques avec ou sans tourbillons, *Comptes Rendus*, **170**, 1920, 449–51.

Villat, H., Sur le mouvement variable d'un fluide indéfini avec sillage en présence d'un corps solide, *ibid.*, **170**, 1920, 653–5.

Hicks, W. M., Mass carried Forward by a Vortex, *Phil. Mag.* (vi), **38**, 1919, 597–612.

Nagaoka, H., Vibration of Cylinder in Fluid Flow, *Proc. Phys. Math. Soc. Jap.*, 1919, 277–83.

Cowley, W. L., Fluid Motion and Viscosity, *Engineering*, **109**, 1920, 101–2.

Dutt, B., On the Steady Motion of a Viscous Fluid due to the Rotation of two Rigid Bodies about Arbitrary Axes, *Bull. Calc. Math. Soc.*, x, 1919, 43–61.

Pol, B., On the Motion of an Ellipsoid of Revolution in a Viscous Fluid, *ibid.*, x, 1919, 81–94.

Harrison, W. J., The Hydrodynamical Theory of the Lubrication of a Cylindrical Bearing under variable Load, and of a Pivot Bearing, *Trans. Camb. Phil. Soc.*, xxii, 1920, 373–88.

Dasgupta, S., Some Cases of Tidal Oscillations in Canals of Variable Section, *Bull. Calc. Math. Soc.*, x, 1919, 105–16.

Banerji, S., On Surface Waves and Tidal Waves near a Promontory, *ibid.*, x, 1919, 1–10.

Raman, C. V., and Dey, A., On the Sounds of Splashes, *Phil. Mag.* (vi), **39**, 1920, 145–7.

Raman, C. V., and Kumar, S., Musical Drums with Harmonic Overtones, *Nature*, **104**, 1920, 500.

Rayleigh, Lord (the late), On Resonant Reflexion of Sound from a Perforated Wall, *Phil. Mag.* (vi), **39**, 1920, 225–33.

Wilson, H. A., The Theory of Receivers for Sound in Water, *Phys. Rev.*, xv, 1920, 178–205.

Waetzmann, E., Theoretisches über den Schalldruck, *Phys. Zeit.*, xxi, 1920, 122–8.

ASTRONOMY. By H. Spencer-Jones, M.A., B.Sc., Royal Observatory, Greenwich.

The Angular Diameters of the Stars.—In these notes recently (Science Progress, **15**, 180, 1920) reference was made to the application of interference methods, at the Mount Wilson Observatory, to the measurement—with the aid of the 100-inch telescope—of the separation of the components of Capella. It was also shown that the same principles are adapted to the measurement of the angular diameters of small satellites or asteroids, and the application of the method to the determination of the angular diameters of stars was foreshadowed. This

has now actually come to pass, and we have to place on record the first direct measurement of the angular diameter of a star.

Before dealing with the actual observation, it is of interest to note that the possibility of measuring the angular diameter of a star in this way was first pointed out by the French physicist Fizeau about fifty years ago. It provides a good illustration of an incidental suggestion, thrown out more or less casually, subsequently bearing fruit. Fizeau's remark occurs towards the end of a report on the award of the Bordin Prize of the Académie des Sciences, and is as follows :

" Il existe, pour la plupart des phénomènes d'interférence, tels que les franges d'Young, celles des miroirs de Fresnel, et celles qui donnent lieu à la scintillation d'après Arago, une relation remarquable et nécessaire entre la dimension des franges et celles de la source lumineuse ; en sorte que les franges, d'une ténuité extrême, ne peuvent prendre naissance que lorsque la source lumineuse n'a plus que des dimensions angulaires presque insensibles ; d'où pour le dire en passant, il est peut-être permis d'espérer qu'en s'appuyant sur ce principe et en formant, par exemple au moyen de deux larges fentes très-écartées, des franges d'interférence au foyer des grands instruments destinés à observer les étoiles, il deviendra possible d'obtenir quelques données nouvelles sur les diamètres angulaires de ces astres " (*Comptes Rendus*, **66**, 934, 1868).

The suggestion of Fizeau was investigated practically by Stéphan, who worked out an approximate theory of the phenomenon. He extended the suggestion by proving that, instead of narrow slits, it was permissible to use extended apertures, provided that the two apertures were equal and possessed two axes of symmetry and that their width was small compared with their distance apart. In this way much more light could be obtained than with narrow slits, making the observations easier. Stéphan used the 80-cm. refractor at the Marseilles Observatory and examined Sirius and other stars, which he anticipated would have a relatively large apparent diameter. Diameters not less than $0''\cdot16$ should have been measurable with the instrument, but Stéphan could not find any star for which the fringes would vanish, and from their appearance he concluded in a paper entitled " Sur l'extrême petitesse du diamètre apparent des étoiles fixes " (*Comptes Rendus*, **78**, 1008, 1874) that " les expériences citées ne prouvent pas seulement que le diamètre apparent des étoiles examinées est inférieur à $0''\cdot16$. Elles montrent encore que ce diamètre est une très faible fraction du nombre précédent." We now know from theoretical considerations that for few, if any, stars do the apparent diameters exceed $0''\cdot05$, and for most are much inferior to this limit, so that Stéphan's conclusion was correct

and formed a substantial advance on the knowledge then available as to the angular diameter of a star.

The method recently employed with success at Mount Wilson was indicated by Michelson in a paper in the *Phil. Mag.* in 1890. By the ordinary interference method, already explained in these notes, in which two slits are placed in front of the telescope, the limiting angular diameter which can be measured is exactly equal to the theoretical resolving power of the telescope. In the case of the 100-inch telescope this is about $0''\cdot05$, so that the angular diameters of the stars are just beyond its reach. Michelson's suggestion was to use a refractometer in which the two mirrors could be separated by any desired amount, which is in effect equivalent to using a telescope whose aperture is equal to the distance between the mirrors. Full details of the methods employed at Mount Wilson have not yet been published, but from the information available at the time of writing the following would appear to be correct in principle: a steel girder, 20 feet in length, is fixed across the upper end of the tube of the 100-inch telescope in such a manner that it can be rotated about the axis of the tube. On the girder are fixed two mirrors, equidistant from its centre, and each inclined at an angle of 45° both to the girder and to the axis of the tube. These mirrors can be moved along the girder in such a manner that they always remain equidistant from its centre. The light from a star is reflected by them towards the centre of the girder, where two other mirrors reflect the two beams down the tube of the telescope on to the 100-inch mirror, which reflects them and brings them together in the focal plane of the eyepiece, where they interfere with one another and produce fringes. The distance apart of the mirrors is varied until the fringes vanish. The girder is then rotated into a different position, and, if the fringes still vanish, it is evidence that their vanishing is not due to the star under observation being a double star, in which case the measures would merely give the separation of the components. From the distance apart of the mirrors when the fringes vanish (d), it is possible to compute the angular diameter of the star from the formula $a = 1\cdot22\lambda/d$, λ being the mean wave-length of the light employed. There are practical difficulties incidental to the use of so large an interferometer which make the observation a delicate and difficult one: it is stated that, after the mirrors are shifted, it requires about one hour to rediscover the fringes. The observers at Mount Wilson are therefore to be congratulated on having succeeded in measuring the angular diameter of α Orionis (Betelgeuse), the value obtained being $0''\cdot042$. The least angular diameter measurable with the present arrangement is about $0''\cdot02$, though it is probable that before long it

will be found possible to measure diameters even smaller than this. Several other stars have been examined, which have been found to have diameters less than this amount.

Betelgeuse is a giant red star, and the above angular diameter corresponds (on the best information available as to the parallax of the star) to an actual diameter of about 300 times the sun's diameter. Since the masses of the stars do not vary very greatly, its density must be extremely small. It is, in fact, amongst the giant red stars of low density and large actual diameter that the stars of measurable angular diameter may be expected to be found. The probable diameters of the stars have been theoretically discussed recently by H. N. Russell (*Pub. Ast. Soc. Pacific*, **32**, 307, 1920). Russell points out that the angular diameter of a star can readily be determined if its surface brightness is known. In fact, if d is the apparent diameter of a star, m its visual apparent magnitude, and J its surface brightness, it can easily be shown that—

$$d \propto 10^{-0\cdot 2m} J^{-\frac{1}{2}}.$$

The constant of the proportionality can be found from the data for the sun, if the sun's surface brightness is taken as unity. Then, putting $j = -2\cdot 5 \log J$, so that j represents the change in stellar magnitude corresponding to a change in surface brghtness in the ratio J to 1, we obtain—

$$d = 0''\cdot 0087\,(0\cdot 631)^{m-j}.$$

The angular diameter can be determined if j can be found. This can only be done indirectly, the basic assumption involved being that the star radiates as a black body. The difference of surface brightness of two stars (when expressed in stellar magnitudes) is proportional to the difference of colour indices: if i is the colour index,

$$j_2 - j_1 = k\,(i_2 - i_1)$$

k is the same for all stars. If the colour indices are standardised to a system in which the index for type *Ao* is zero and for type *Ko* is $+ 1\cdot 00$, Russell shows, from various lines of argument, that the value of k can be approximately taken as $4\cdot 0$; j can then be taken for any star as equal to four times its colour index on the standard scale, and the angular diameters can then be determined from the formula given above. If the parallax of the star is known, its linear diameter can be calculated.

The results obtained are at first sight rather surprising: thus Sirius, the brightest star in the sky, whose angular diameter Stéphan attempted to measure, presumably in the belief that its diameter would be relatively large, is found to

have an angular diameter of only 0″·007, and a linear diameter only twice that of the sun. Similarly, Canopus and α Centauri have small angular diameters. In fact, if the diameter of a star exceeds 0″·01, we must have $m - j < - 0{\cdot}3$, and no star of solar or earlier spectral type satisfies this relationship. All the stars with the largest apparent diameters must therefore be sought amongst the red stars, which therefore become objects of considerable interest. Russell's calculations indicate that the stars which should have the largest angular diameters are α Orionis (Betelgeuse), α Scorpii (Antares), γ Crucis, α Tauri (Aldebaran), and β Gruis. For Betelgeuse he finds 0″·031, which is of the same order of magnitude, though smaller, than the 0″·045 measured at Mount Wilson.

Similar conclusions have been reached by Wilsing at Potsdam, who has determined the temperatures of a number of stars from the energy distribution in their spectra and so been able to derive their surface brightness. Eddington, in his Presidential Address to Section B of the British Association (" The Internal Constitution of the Stars," *Observatory*, **43**, 341, 1920), gave a theoretical determination of the angular diameter of several stars, based on somewhat similar considerations; the following table summarises his values for the probable angular diameters of stars of various spectral types and visual magnitudes :

Visual Magnitude	A	F	G	K	M
0·0	″·0034	″·0054	″·0098	″·0219	″·0859
2·0	″·0014	″·0022	″·0039	″·0087	″·0342
4·0	″·0005	″·0009	″·0016	″·0035	″·0136

The large values for the bright red stars (type M) will be noticed. Eddington agrees with Russell in placing Betelgeuse and Antares at the top of the list, finding ″·051 for the former, a somewhat larger value than given by Russell. Wilsing finds for Betelgeuse ″·042, the determination being based upon a measurement of its effective temperature. The values are in very close agreement with the observed value.

The first result to be obtained confirms the theroetical basis on which the calculated values are based, and is therefore of the greatest value. It is probable that further results will be announced before long, and it is to be hoped that the progress in our knowledge in this direction will be as rapid as the recent progress in our knowledge of the distances of stars.

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PHYSICS. By L. F. Bates, B.Sc., The University, Bristol.

Prof. W. Lawrence Bragg publishes a paper on the arrangement of atoms in crystals in the *Phil. Mag.* for August 1920. He shows that, if we imagine crystals to be formed by the assembling of the atoms in the form of spheres of suitable diameters, each sphere held in contact with its neighbours, then certain useful empirical relations can be deduced. Thus, in any crystalline structure a certain volume must be attributed to each atom, and no two atoms can approach closer than the distance representing the sum of their radii. The conception is only to be considered as a "working approximation," for differences of about 10 per cent. between the calculated and observed distances are known to exist in some cases; but it greatly assists in the interpretation of the diffraction patterns obtained by the passage of X-rays through the more complex crystal structures. Starting with Hull's determination of the structure of metallic iron, the diameter of the sphere representing the iron atom is taken as 2·47 Ångström units (2·47 Å), and, from his own determination of the structure of iron pyrites, Prof. Bragg calculates the diameter of the sulphur sphere to be 2·05 Å. In this way the diameters of other atoms are calculated, and a curve with "atomic diameter" as ordinates and atomic number as abscissæ is plotted. The periodic curve obtained is similar to the Lothar Meyer curve of atomic volumes; the alkaline metals and alkaline earths possess the greatest diameters, and the electronegative elements the least. It is shown that the large diameters of the electropositive atoms are consistent with the Lewis-Langmuir theory that the electropositive atom does not share electrons with neighbouring atoms, but is surrounded by a stable shell of electrons, which repels the shells of neighbouring atoms, and thus keeps them at a fixed distance. Similarly, the small atomic diameters accredited to the electronegative elements correspond to Langmuir's conception that these elements share electrons, and hence their centres approach each other more closely. From the apparent approach to a lower limiting value for the atomic diameters of the electronegative elements in each period of his

curve, Prof. Bragg gives an estimate of the diameters of the outer electron shells of these elements. Since these shells correspond, on Langmuir's theory, to the outer shells of Neon, Argon, Krypton, and Xenon, the values 1·30 Å, 2·05 Å, 2·35 Å, and 2·70 Å are given for the outer shells of these respective gases. The relations hold most accurately for compounds and electronegative elements; they do not hold accurately for crystals of the electropositive elements; this is consistent with Langmuir's conception that a metallic crystal consists of a number of positive ions bound together by electrons whose positions are not fixed, whereas crystals of electronegative elements consist of atoms bound together by sharing electrons.

In continuation of his work on the influence of molecular onstitution and temperature on magnetic susceptibility, of which a résumé is to be found in SCIENCE PROGRESS, No. 56 (March 1920), Dr. A. E. Oxley publishes a paper on magnetism and atomic structure in the *Proc. Royal Society*, No. A, 692, in which he gives a conception of the electron structure of matter in the light of recent experiments on magnetic effects. A crystalline plate of naphthalene sets with its principal cleavage perpendicular to the lines of a uniform magnetic field, and experiments on piles of naphthalene plates—each pile being constructed of selected plates cemented together with Canada balsam at different orientations, but with the principal cleavages of the plates parallel to one another—show that a pile always sets with the principal cleavage perpendicular to the magnetic field, however thick the pile may be. The diamagnetic effect is, therefore, a maximum in a direction parallel to the principal cleavage. Now Dr. Oxley attributes the cohesive force, which is a maximum parallel to the same cleavage, to the highly localised mutual induction between a pair of electron orbits, and he gives diagrams which illustrate the nature of this form of coupling which would account for the diamagnetic and paramagnetic properties of molecules and the cohesive force of non-ionised compounds. In particular, he suggests a model of the diamagnetic hydrogen molecule. Crystallisation is imagined to take place under the influence of the Laplace intrinsic pressure, which Dr. Oxley suggests may arise from electrostatic doublets of atomic dimensions, and the directive magnetic couplings which are responsible for the crystalline symmetry of a given molecule and the rigidity of the crystalline structure. Thus, the electron coupling is a maximum parallel to the principal cleavage, so that paramagnetic and diamagnetic crystals should set with this cleavage parallel and perpendicular to the field respectively; and this is verified in the case of the naphthalene piles. Simple cubic crystals, which we know from X-ray analysis to possess an ionised atomic structure, possess

three mutually perpendicular and equally defined planes of cleavage, and show no traces of diamagnetism or paramagnetism in accordance with these views. The presence of several planes of cleavage in a crystal represents that the areas of the electron orbits can be projected parallel as well as perpendicular to the principal cleavage, and therefore the necessary spacial arrangement of electrons rotating in small circles or ellipses at various distances from the atomic nucleus is similar to that suggested by the Lewis-Langmuir cubical atom theory. On this theory one of the stable arrangements of electrons takes the form of eight electrons situated at the corners of a cube, and it therefore offers a more reasonable explanation of magnetic phenomena than the Bohr-Sommerfeld theory.

In the *Physical Review* for November 1920 Messrs. A. H. Compton and O. Rognley describe an experiment designed to test the dependence of the intensity of a beam of X-rays on the magnetic state of a ferromagnetic crystal from which it is reflected. A crystal of magnetite is magnetised (*a*) parallel and (*b*) perpendicular to the III face, and the intensity of a beam of X-rays reflected from this face is investigated in the first three orders of reflection in case (*a*) and in the first four orders in case (*b*). No detectable difference is found in the beams reflected from the crystal in the magnetised and unmagnetised states, although a sensitive null method capable of recording a 1 per cent. difference of intensity is used, and the crystal is magnetised to slightly over one-third its saturation value perpendicular to the reflecting face. If magnetisation is due to the rotation of groups of atoms, or of single iron atoms of the Rutherford type, or of the type suggested by Hull, then a large difference in intensity should result. Hence the conclusion previously formed by A. K. Compton and A. E. Trousdale (*Phys. Rev.* 5, 1915) that the elementary magnet is not a group of atoms is confirmed; and this is consistent with the results of A. P. Chattock and F. B. Fawcett (*Phil. Mag.*, Nov. 1894). The authors also conclude that, in all probability, the elementary magnet is not the atom in its entirety. The experimental results are, however, in agreement with the view that the electron itself is the ultimate magnetic particle, although they are not in themselves a definite proof that this is the case.

The investigations of Mr. J. Chadwick on the charge on the atomic nucleus, to which reference is made in the Bakerian Lecture, 1920, is published in the *Phil. Mag.* for December 1920. Geiger and Marsden, by means of experiments on the scattering of α particles, determined the charge on the atomic nucleus to be $\frac{1}{2}\,Ae$, where A is the atomic wieght, and e the electronic charge, within an error of about 20 per cent., the inaccuracy being due to the employment of different methods

for measuring the very diverse intensities of the direct and scattered beams. Chadwick uses the scintillation method of measurement, but he employs an arrangement whereby the number of scattered particles reaching the fluorescent screen is increased; and, further, he employs a device suggested by Sir E. Rutherford, which permits the measurement of the intensities of the original and scattered beams on the same screen. The respective atomic numbers of platinum, silver, and copper are 78, 47, and 29, and it is found that the charges on the nuclei of these elements are respectively $77 \cdot 4\, e$, $46 \cdot 3\, e$, and $29 \cdot 3\, e$, within an error of about 1 to $1\frac{1}{2}$ per cent., the accuracy of the experiments being limited by the probability fluctuations. It is further shown that in the neighbourhood of 10^{-11} cms. from the platinum nucleus the law of force varies as $1/r^p$, where p lies between $1 \cdot 97$ and $2 \cdot 03$.

Prof. T. R. Merton (*Proc. Royal Society*, No. A, 691) describes some interesting experiments designed to investigate the effect of concentration on the spectra of luminous gases. It was suggested by Lord Rayleigh that the broadening of the spectral lines of an element in a Bunsen flame is due to temporary association of the atoms or to the influence of neighbouring atoms on the radiating particles. If a large quantity of sodium is added to a flame containing a small quantity of lithium, or a large quantity of lithium to a flame containing a small amount of sodium, it might be expected, from the chemical similarity of lithium and sodium, that temporary associations of lithium and sodium atoms take place, with resulting broadening of the lithium or sodium lines respectively; but Prof. Merton finds no trace of this effect in experiments conducted on flames or arcs *in vacuo*. In the case of helium and hydrogen lines produced by vacuum tubes, the broadening of the lines is produced by electrical resolution due to the electric fields of charged atoms in the immediate neighbourhood of the radiating particles. A vacuum tube filled with pure helium under high pressure (over 50 mm. of mercury), and excited by a condensed discharge, produces broadened spectral lines; if small quantities of hydrogen are now passed into the tube the helium lines appear broadened as before, and the hydrogen lines appear quite sharp but faint; but, on the addition of more hydrogen, the latter become diffuse, and eventually definitely broadened when the quantity of added hydrogen is large. It therefore appears that charged atoms of the same kind affect the radiating atoms and cause resolution to a marked extent, but the effect of a charged particle of another kind in close proximity is small. When a high-pressure helium vacuum tube containing a little hydrogen is excited by an uncondensed discharge the hydrogen lines as seen in a direct vision spectroscope appear of uniform

intensity throughout the capillary; when, however, a condenser and spark gap are placed in parallel with the terminals of the induction-coil, the hydrogen lines appear faint inside the capillary, and much stronger in the bright glows which appear just beyond the ends of the capillary. On cutting out the condenser these bright glows gradually extend into the capillary in a manner suggestive of gaseous diffusion, until the Balmer lines are once more of uniform brightness throughout the capillary. Prof. Merton suggests that this change in the relative intensity of the lines in the different parts of the tube is due to an alteration of the relative proportions of helium and hydrogen —the latter being driven out the capillary in some way when the condensed discharge is applied—and that this may be the explanation of many phenomena in vacuum tubes containing two gases, which have formerly been attributed to changes in the nature of the electrical discharge itself.

PHYSICAL CHEMISTRY. By W. E. GARNER, M.Sc., University College, London.

The Radiation Hypothesis.—This hypothesis, as developed by Trautz, Lewis, Perrin, and others, has been criticised recently by Lindemann (*Phil. Mag.*, 1920, **40**, 671) and Langmuir (*J.A.C.S.*, 1920, **42**, 2290). This theory assumes that the velocity of a chemical reaction is determined by the energy density of radiation of a characteristic frequency, which is emitted and absorbed by the chemical system. A possible check on this theory is to establish some relation between the value of ν derived from the equation

$$\frac{d \log k}{d\mathrm{T}} = \frac{\mathrm{N}h\nu}{\mathrm{RT}^2}$$

and the value obtained from the optical properties of the system. Lewis has done this for the inversion of sucrose by hydrochloric acid, and obtained satisfactory agreement for the two values of the characteristic frequency ($\lambda = 1{\cdot}05\mu$)

Lindemann, however, raises the following objection to the radiation hypothesis. The energy density of $\lambda = 1{\cdot}05\mu$ in the light from the sun is 10^{13} times that occurring in the dark, in the case of Lewis's experiments. This radiation should materially modify the velocity of the chemical reaction, whereas the reaction proceeds at the same rate whether exposed to sunlight or not.

Langmuir points out that in the cases of the gases—PH_3, As_4, P_4, $COCl_2$, and NO—no absorption band is to be found

which agrees with the frequency calculated from the equation $\frac{Q}{N} = h\nu$. The values for the monochromatic radiation of these gases should be $390\mu\mu$ for PH_3, $630\mu\mu$ for As_4, $570\mu\mu$ for P_4, $467\mu\mu$ for $COCl_2$, and $437\mu\mu$ for NO; and all of these wavelengths lie in the visible region. Since the gases in question are colourless, they cannot absorb appreciable amounts of the necessary radiation. There are also considerable discrepancies between the amount of monochromatic energy radiated (black body radiation) and the energy required for the activation of the gaseous molecules. The amount of radiant energy available is many times smaller than the heat absorbed in activation. This discrepancy is very marked in the case of phosphine, where the radiant energy is one million times too small. The rate at which heat is dissipated from a tungsten filament in hydrogen gas is also not in agreement with the hypothesis.

Perrin pointed out that the velocity of a monomolecular reaction is independent of the pressure and the number of collisions between the molecules, and hence the energy of collision could not be solely responsible for the chemical reaction. Langmuir shows that the collision-energy is too small to supply the heat of activation. It thus appears that neither the radiant energy nor the energy from collisions is sufficient to activate the molecules and to account for the velocity of chemical reaction. He proposes a modification to the radiation hypothesis, and suggests that chemical reaction is in many ways analogous with the photoelectric effect, and that by a kind of trigger action, energy at a low intensity would be able to control the disposition of energy of a high intensity. (This action would be presumably analogous to that of infra-red radiation on the phosphorescent light emitted by a phosphoroid.) Baly (*Phil. Mag.*, 1920, [vi], **40**, 15) suggests a very similar mechanism to explain the discrepancy occurring between the velocity of decomposition of phosphine and that calculated on the quantum theory. The absorption of infra-red radiation by the phosphine molecule may be regarded as initiating a change, which is itself the origin of radiation of an absorbable type. The decomposition of phosphine, being slightly exothermic, could thus be brought about by a low density of infra-red radiation. The reaction is compared with that investigated by Henri and Wurmser, where 180 molecules of H_2O_2 were decomposed by one quantum at the phase frequency.

Lewis (*Trans. Chem. Soc.*, 1916, **109**, 806) indicates that it is possible for catalysts to increase the natural radiation of a system (black body radiation), but it is not clear whether the density of such radiation would be sufficient to supply the whole of the energy of activation.

Rideal (*Trans. Chem. Soc.*, 1920, **117**, 1288) has shown, in a preliminary series of experiments, that the infra-red radiation from the sun or from a hot nichrome wire accelerates the rate of hydrolysis of methyl acetate. The radiation hypothesis is supported by the work of Cox (*Trans. Chem. Soc.*, 1921, **119**, 142) and Dhar (*Proc. K. Akad. Wetensch., Amsterdam*, 1920, **23**, 308). These two authors have investigated the temperature coefficients of certain reactions. Cox finds that the temperature coefficient of a reaction in the various solvents is inversely proportional to the velocity in the solvent—a condition which is required by the radiation hypothesis.

The "Cyanogen" Bands.—Barratt (*Proc. Roy. Soc.*, 1920, A 98, 40) has investigated the problem of the source of these bands. It was formerly considered that the cyanogen bands were produced by a compound of carbon and nitrogen, but Grotrian and Runge in 1914 threw doubt on this conclusion. In experiments with Schonherr elongated arcs they found that the cyanogen spectrum was produced with metal electrodes and nitrogen alone. On the other hand, Hemsaleck obtained evidence that nitrogen is not essential to their formation. These contradictions are readily explained, since the presence of carbon and nitrogen in very small quantities suffices to produce the bands. Barratt has examined the spectrum of a number of flames, and has found that the cyanogen spectrum is only present in those containing carbon, nitrogen, and hydrogen. The nitrous oxide-coal gas flame yielded these bands, whereas the nitrous oxide-hydrogen flame did not. Elementary nitrogen is not so efficient as combined nitrogen, and the presence of oxygen is unfavourable to the production of these bands. This spectrum offers a very sensitive test for the presence of carbon and nitrogen in gases, and in the case of carbon is much more sensitive than any other carbon lines.

Luminescence.—Schmidt (*Ann. Phys.*, 1920, [iv], **63**, 2 4) continuing the work of Lenard and others on the phosphorescence of the alkaline earth substances, has investigated the oxide phosphoroids. Lenard showed that a phosphoroid should be of the type sulphide—easily fusible salt—heavy metal. The quantity of the heavy metal present should not exceed 10^{-4} times that of the sulphide. Pauli and Hirsch have shown that sulphides may be replaced by oxides or selenides without the mixtures losing their phosphorescent properties when exposed to light. Schmidt has examined the luminescence and phosphorescence of the oxide class of substances when exposed to light of wave-lengths ranging up to $220\mu\mu$. Certain wave-lengths in the blue and ultra-violet were shown to excite phosphorescence to the maximum extent. The wave-length of both the exciting and incited radiation was found to be

shifted towards the longer wave-lengths, with increase in the atomic weight of the alkaline earth metal. Thus, the phosphorescence of CaO,Cu was blue; that of SrO,Cu green, and of BaO,Cu yellow.

The oxygen compounds require to be heated to a high temperature before showing the maximum intensity and duration of phosphorescence, whereas sulphur compounds phosphoresce best at ordinary temperatures, and selenium compounds at lower temperatures. Some of the oxide mixtures, which did not phosphoresce at ordinary temperatures, stored the exciting radiation, and emitted this only when heated to 300–400° C. There are many regularities to be observed in the behaviour of the sulphur, selenium, and oxygen compounds. With increase in atomic weight, both the exciting bands and the phosphorescence are shifted to the longer wave-lengths.

Tiede and Schliede (*Ber.*, 1920, **53** [B], 1721), studying the phosphorescence of zinc sulphide, conclude the crystalline forms of this substance do not show phosphorescence. Neither wurtzite (hexagonal) nor sphalerite (in the pure condition) is capable of this phenomenon. The fused salt and the crystalline forms in the presence of a flux may, however, be excited by light. Pure barium sulphide also phosphoresces in the fused condition without the presence of a binding material.

The fluorescence of iodine is apparently a property of the molecule and not of the atom (Landau and Stenz, *Phil. Mag.*, 1920, [vi], **40**, 189). The dissociation of the iodine destroys both the fluorescence and the resonance spectra. The complicated vibrating spectra, corresponding with thousands of lines in the visible part of the spectrum, is not inherent in the atom, but in the molecule. Thus, the iodine atom will possess a simple structure.

Other papers on this subject are:

"The X-ray Fluorescence of certain Organic Compounds," Newcomer, *J.A.C.S.*, 1920, **42**, 1997; "Existence of Intermediate States in the Phosphorescence of Calcium Sulphide, deduced from a Study of its Conductivity," P. Vaillant (*C.R.*, 1920, **171**, 713); "Fluorescence, Dissociation, and Ionisation in Iodine Vapour," Compton and Smyth (*Science*, 1920, **51**, 571); "Fluorescence of the Uranyl Salts," Nichols, Howes, Merritt, Wilber, and Wick (*Carnegie Inst. Washington Publication*, 1919, **298**, 1); "Bioluminescence. III, The Production of Light by *Luciola* vitticollis is an Oxidation Process," Sakyo Kanda (*Amer. J. Physiol.*, 1920, **53**, 137).

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., University College, London.

Two papers dealing with the constitution of polysaccharides deserve attention. In the first one, on "The Relationship of Inulin to Fructose" (Irvine and Steele, *J. Chem. Soc.*, 1920,

118, 1474), it is pointed out that the systematic investigation on methylated sugars, carried out during the last twenty years in the St. Andrews laboratory, were intended as a groundwork to the study of the problems presented by the complex carbohydrates. The principle involved in these investigations is to substitute all free hydroxyl groups in a carbohydrate by stable methoxyl groups, and so on hydrolysis to obtain a methylated sugar or sugars. Determination of the number and position of the alkyl groups in each of the hydrolytic products thus gives direct evidence as to the linkage of the constituents in the parent complex. The inulin employed in this investigation was obtained from dahlia tubers; after repeated alternate solution in water and freezing out and washing, it was still found to yield rather less than 0·2 per cent. of ash on ignition, from which it was concluded that the organic constituents form a minute but definite part of the molecular complex. The purified inulin had a specific rotation of $-35°$ on methylation with methyl sulphate and alkali, and yielded a lævorotatory dimethyl inulin which, on subsequent treatment with methyl iodide and silver oxide, gave a dextrorotatory trimethyl inulin having $[\alpha]_D^{15} + 55{\cdot}6°$; the latter compound being appreciably volatile at 196°/·15 mm., it may be concluded that the molecular weight of the polysaccharide is much smaller than is generally supposed. The authors suggest alternative formulæ for inulin in which, to account for the behaviour of the substance towards methylating agents, two of the three hydroxyls in each C_6 complex are differently situated from the third. Hydrolysis of the trimethyl inulin with 1 per cent. oxalic acid produces a very good yield of trimethyl γ-fructose, which can be converted into a tetramethyl fructose identical with that previously isolated from sucrose, and it is argued that consequently the structural relationship between sucrose and inulin is a close one. In the second paper, dealing with "The Conversion of Cellulose into Glucose" (Irvine and Soutar, *J. Chem. Soc.*, 1920, **118**, 1489), attention is drawn to the fact that the confident statements made from time to time by various authors to the effect that practically quantitative yields of glucose had been obtained by the hydrolysis of cellulose were based entirely on observations of optical activity or reducing power, and not on the isolation of a pure material. Moreover, Willstätter and Zechmeister's claim (*Berichte*, 1913, **46**, 2401) to have obtained 96·3 per cent. of the theoretically possible yield of the hexose cannot be substantiated owing to the action of hydrochloric acid in promoting the auto-condensation of glucose to iso-maltose.

In the present instance, however, the authors have adopted the principle that the yield of hexose obtained in their hydrolysis

experiments should be calculated from the weight of crystalline compound isolated in a state of analytical purity. The method adopted was to hydrolyse and simultaneously condense the sugar liberated with methyl alcohol so as to give a stable derivative which would thereafter remain unaffected, and so protect the glucose from the destructive effect of the hydrolytic agents. To this end pure cotton cellulose was treated with a large excess of acetic anhydride containing acetic and sulphuric acids, and when the fibrous structure had disappeared the mixture was poured into water. The resulting polysaccharide acetates were then heated in an autoclave at 100° with methyl alcohol and 0·5 per cent. of dry hydrogen chloride; this treatment removed the acetyl groups as methyl acetate, whereupon simultaneous hydrolysis and condensation with the solvent ensued, resulting in the formation of methyl glucoside, which was weighed. From this material no difficulty was experienced in obtaining pure crystalline glucose by hydrolysis. In this way it has been shown that the yield of glucose obtained by the hydrolysis of cellulose is at a minimum 85 per cent. of the theoretically possible. The unexplained margin of 15 per cent. may possibly be accounted for by the presence of a nucleus in the cellulose molecule which is resolved into a ketose on hydrolysis.

The methylation of polysaccharides is also attracting the attention of other workers. Thus, Denham (*J. Chem. Soc.*, 1920, 119), acting on cellulose with methyl sulphate in presence of sodium hydroxide, obtained a product containing 44·6 per cent. of methoxyl, which closely approximates to the 45·6 per cent. required by theory for a trimethyl cellulose. On the other hand, Karrer (*Helv. Chem. Acta*, 1920, **3**, 620), by methylating starch, has obtained three compounds containing respectively one, one and a half, and two methoxyl groups to each C_6 complex.

A new synthetic method for the preparation of *α*-monoglycerides is described by Fischer, Bergmann, and Bärwind (*Berichte*, 1920, **53** [B], 1589). The method depends on the fact that "acetone glycerol," or *iso*-propylidene glycerol,

$$(CH_3)_2C\begin{cases} O \cdot CH_2 \\ \quad\;\; | \\ O \cdot CH \cdot CH_2OH \end{cases}$$

reacts readily with acid chlorides in presence of quinoline, yielding products from which the acetone may be readily removed, leaving *α*-monoglycerides.

The decomposition of Tartaric Acid by Heat has been reinvestigated by Chattaway and Ray (*J. Chem. Soc.*, 1921, **119**,

34). The decomposition has been carried out at the lowest possible temperature and under diminished pressure, and the primary products were at once removed, thus avoiding or reducing to a minimum, any side reactions ; it was found that tartaric acid at first loses one molecule of water, producing a colourless lactide ; this is followed at a somewhat higher temperature by an intra-molecular rearrangement whereby hydrogen and hydroxyl change places, and the resulting compound thereupon breaks down in one of two ways to produce, in the one case acetic acid and carbon monoxide and carbon dioxide, and in the other carbon dioxide and pyruvic acid.

CRYSTALLOGRAPHY. By ALEXANDER SCOTT, M.A., D.Sc.

A. W. HULL has published a preliminary note on his investigations of the structure of certain metals (*Science*, **52**, 227, 1920 ; *Jour. Chem. Soc.*, **120**, ii, 38, 1921). Calcium is composed of face-centred cubic lattices, so that each atom has twelve others equidistant from it. Some of the elements of the eighth group, such as platinum, palladium, and iridium are similar to calcium, but ruthenium has a structure like that of zinc and cadmium. The latter two elements have structures which conform to close-packed hexagonal aggregates, the elongation of the unique axis being 14 per cent. and 16 per cent. respectively. Titanium and tantalum belong to the centred cubic type. Iridium is composed of a face-centred tetragonal lattice, the elongation of the unique axis being 6 per cent.

By the examination of X-radiograms of graphite, from grey pig-iron, and temper carbon, from annealed white cast-iron, K. Jokibé (*Sci. Rep. Tôhoku Univ.*, **9**, 275, 1920) deduces that the structure of each of these forms of carbon is the same as that of natural graphite. Hence, temper carbon cannot be regarded as amorphous.

The structure of antimony has been investigated by R. W. James and N. Tunstall (*Phil. Mag.* [vi], **40**, 233, 1920), and the crystals have been found to consist of two interpenetrating face-centred lattices. The arrangement of the latter is such that the atoms constituting one of the lattices lie on the "trigonal" diagonals of the rhombohedral cells which form the unit of the other lattice. In order to reconcile the structure with the observed results, it is necessary to assume that the atoms of the second lattice do not occupy the vacant corners of the first, but are displaced a short distance along the diagonal.

According to W. L. Bragg (*ibid.* [vi], **39**, 647, 1920), zinc oxide consists, so far as the crystalline form is concerned, of two interpenetrating hexagonal lattices, with zinc as the constituent particles, and two similar ones composed of the oxygen atoms.

The latter are regarded as translated parallel to the C-axis in such a way that each oxygen is approximately in the centre of a tetrahedron formed by four zinc atoms.

One of the most interesting investigations of crystal structure ever carried out is that described by G. Bartlett and I. Langmuir (*Jour. Amer. Chem. Soc.*, **43**, 84, 1921). The work of W. H. and W. L. Bragg (*X-Rays and Crystal Structure*, London, 158, 1916) on the alkali halides showed that, in the case of the potassium and sodium salts, each metal atom was equidistant from six halogen atoms, and, correspondingly, each halogen equidistant from six atoms of the metal. So far as ammonium chloride is concerned, each ammonium radicle is symmetrically placed with reference to eight halogen atoms, and conversely. The structure in the latter instance agrees, as a whole, with the centred cube, compared with the simple cube of the former. This has been interpreted as indicating a tetrahedral symmetry for the ammonium ion, while the symmetry of the alkali ions is cubical. In order to test this interpretation, Bartlett and Langmuir, by means of the method devised by Hull (*Phys. Rev.*, **10**, 661, 1917; *Jour. Amer. Chem. Soc.*, **41**, 1168, 1919), have examined the structures of the ammonium halides, both at ordinary and higher temperatures. Ammonium chloride and bromide each exist in two polymorphous modifications, the transition temperatures being 184·3° and 137·8° respectively, and hence the crystal structures determined at 20° and 250° might be expected to be different. This has proved to be the case, as in each salt the high-temperature form has a simple cubic arrangement of the ions analogous to that of sodium chloride, while the low-temperature form has the more complex centred-cubic form. The transition temperature for ammonium iodide is − 17·6°, so that the form stable at 20° corresponds to the high-temperature form of the chloride and bromide, and has a simple cubic structure.

In the high-temperature forms, owing to the greater distance between like ions, the influence of the ammonium ion becomes less, and hence there is a greater tendency for the ammonium salts to be isomorphous with the corresponding salts of the alkale metals. A similar isomorphism can also occur where the anion has a comparatively large volume, as in certain sulphates, the influence of the ammonium ion being negatived by that of the anion.

The structure of cæsium di-chloroiodide has been investigated by R. W. G. Wyckoff (*Jour. Amer. Chem. Soc.*, **42**, 1100, 1920). Although two polymorphic forms—hexagonal and rhombic—of this salt have been described (Wells and Penfield, *Amer. Jour. Sci.* (3), **43**, 29, 1892), there is apparently only one form, the "rhombic" nature of one modification being due to

twinning. Examination of the X-ray spectrum, supplemented by Laue radiograms, shows the rhombohedral modification to have as its structural unit a centred rhombohedron consisting of one cæsium, one iodine, and two chlorine atoms. The chlorine atoms are situated on the long diagonal of the rhombohedron about one-third way from the corners, while the cæsium and iodine atoms occupy the corner and centre, the discrimination of the latter two not being possible so far. The structure is considered to be analogous to that of the simple halides, the deformation of the cube into a rhombohedron being due to the replacement of the simple halogen by the group ICl_2.

The same writer (*Phys. Rev.*, **16**, 149, 1920) has subjected sodium nitrate to X-ray examination, and finds the structure to be composed of interpenetrating rhombohedral lattices very similar to those in calcite. Bragg's suggestions as to the structure of sodium nitrate are thus verified. R. G. Dickenson (*Jour. Amer. Chem. Soc.*, **42**, 85, 1920) has examined the X-ray spectra of wulfenite (lead molybdate) and scheelite (calcium tungstate). Although the elucidation of the structure is incomplete, the results show that the heavier atoms are arranged in face-centred lattices.

The general results of X-ray work on crystals is discussed in several recent papers. From considerations of the spatial relationships of the atoms in crystals, W. L. Bragg (*Phil. Mag.* [vi], **40**, 169, 1920) concludes that each type of atom has a characteristic constant, and the space which separates it from an atom of another type is a function of such constants of the two atoms. By graphing the constants of a number of elements a periodic curve analogous to the atomic volume curve is obtained. In the case of electronegative atoms, certain electrons are common to both, and hence a closer packing than is obtained in the case of electropositive atoms is possible. The paper includes calculated data for the diameter of the outer " electron shell " of many elements. H. Tertsch, in a paper entitled, " Crystallographic Observations on Atomic Structure " (*Sitzber. Akad. Wien*, **129**, 24, *Jour. Chem. Soc.*, **120**, ii, 24, 1921) discusses the relationship between crystal and atomic symmetry, and concludes that the latter has an important influence on the crystal form. In a mathematical paper on the atomic constitution of crystal surfaces, E. Madelung (*Physikal. Zeit.*, **20**, 494, 1919) shows that in binary compounds there is a displacement of one type of atom relative to the other so far as the surface layers are concerned.

The question of the existence of molecules in crystalline compounds is treated by A. Reis (*Zeit. Physik*, **1**, 204, 1920), who deduces, on geometrical grounds, an essential difference between those compounds which are of the nature of salts and

those which are not. In the latter, the lattice is molecular, and certain physical constants, such as heat of transition, are less than in the case of those compounds which have a lattice composed of ions. A. Quartaroli (*Gazzetta*, **50**, ii, 60, 1920; *Jour. Chem. Soc.*, **118**, ii, 602, 1920) discusses the bearing of X-ray work on crystals on the Law of Definite Proportions, and comes to the conclusion that in minute crystals, where the number of atoms is small, the composition may vary from the recognised formula. As such "embryo crystals" with only a few atoms would be submicroscopic, it seems better not to class them as true crystals, particularly as they cannot be directly examined.

F. M. Jaeger (*Proc. Akad. Amsterdam*, **22**, 815, 1920) has investigated the patterns obtained by passing a beam of X-rays through lamellæ of mica symmetrically superimposed, and finds that dextro- and lævo-rotatory aggregates give the same pattern. L. Vegard (*Ann. Physik* (4), **63**, 753, 1920) reiterates his views on the rôle of the water of crystallisation in the structure of the alums. The relations between cleavage and the lattice structure as determined by X-ray methods is discussed by R. Scharizer (*Zeit. Kryst. Min.*, **55**, 440, 1920).

BOTANY By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Ecology.—An interesting study of the conditions above and immediately below the tree limit on Mount Marcey, New York, has furnished data showing the effectiveness of the forest shelter (Adams, Barnes, Hankinson, Moor, and Taylor, *Ecology*, 1920). The timber line is situated at 4,900 ft., or 444 ft. below the summit. The solar radiation, at the ground-level, in the dense dwarf forest of *Abies balsamea* just below the timber-line, was only 4 per cent. of that above, whilst in the tree crowns it reached 89 per cent. At the lower limit of the forest (4,250 ft.) the radiation was 51 per cent. The evaporation exhibited a similar minimum just below the timber-line, despite the general increase with altitude. The temperature gradient showed an abrupt fall above the tree limit, and it is suggested that this is one of the most important factors causing the abrupt change in vegetation. It is significant that the rate of growth, which, like the longevity of the trees, diminishes with altitude, falls abruptly at the upper tree limit.

A. Tengwall has written an extensive account of the vegetation of the Sarek region of Lapland (*Naturwiss. unt. des Sarekgebirges im Schwedische-Lappland*, 1920) mostly of a descriptive character. The chief type of woodland is dominated by birch, which attains an altitude of from 533–832 m. on southern aspects, and from 590–760 m. on northern. The

duration of the active period of the birch is stated to be uniform at the tree limit, and the factors influencing this and the minimal summer temperature are considered to determine the altitude of this zone. Many of the birch woods are heathy in character, with *Vaccinium myrtillus*, *Deschampsia flexuosa*, *Solidago-virgaurea*, etc., and mosses, especially *Dicranum* spp., are often abundant, their prevalence being associated with a poverty of lichens. *Empetrum nigrum* replaces *Vaccinium* where the snow melts earlier, as around the birch trunks, or even throughout the wood in situations which are very dry and exposed, with thin humus. These woods occur in the subalpine region with a vegetative period of about three months (mid-May —August). In the alpine zone, this lasts only about 2½ months, and the vegetation is mainly scrub, dominated by willows and *Betula nana*, meadows on thin soil consisting of either tall herbs, such as *Geranium sylvaticum*, *Mulgedium alpinum*, *Angelica*, etc., or low-growing species, including *Dryas*, *Sibbaldia*, *Viola biflora*, etc. Heaths also occur in which *Empetrum*, *Vaccinium*, *Bryanthus*, *Diapensia*, *Loisleuria*, etc., form growths comparable to our *Vaccinium-Calluna* heaths, or they are dominated by grasses, amongst which *Anthoxanthum* and *Nardus stricta* are prominent. The moorlands are chiefly *Carex* moors or *Eriophorum* moors. Numerous subtypes are described, and statistical data show the comparative areas occupied by each.

Miss Lyle, writing of the marine algæ of Guernsey (*Jour. Bot.*), recognises three zones, viz. the upper littoral, mid-littoral, and sublittoral, in which the predominating species vary according to the exposure. Thus, in the uppermost zone, species of Cyanophyceæ are associated with exposed conditions and *Pelvetia* with moderate shelter. This latter condition also favours *Enteromorpha* and *Cladophora rupestris*, whilst *Porphyra* is associated with extreme shelter. In the second zone *Fucus* spp. dominate with *F. vesiculosus* and *F. serratus* in shelter, and *F. vesiculosus v. evesiculosus* where exposed. In the sublittoral zone, *Laminaria digitata*, *Saccorhiza polyschides*, and *L. saccharina* are a series indicative of increasing shelter. On the more exposed rocks *Corallina squamata* and *Lithothamnion polymorphum* form an undergrowth to the larger species. The entire algal flora comprises 428 species, of which 53 spp. are Northern and 20 Arctic; 382 species are common to Guernsey and the South of England, and 356 are also met with on the coasts of Northern and Western France, showing a greater British than continental affinity.

Anatomy and Cytology.—Some years ago Bailey and Tupper examined the lengths of vessels and tracheids in a number of vascular plants, and found that the average for the tracheids

of Gymnosperms was $3{\cdot}53 \pm {\cdot}07$ mm., for the fibre-tracheids and vessel segments of Dicotyledons the values were $0{\cdot}61 - 1{\cdot}20 \pm 0{\cdot}02$ mm. In a recent paper Bailey (*Amer. Jour. Bot.*) gives data respecting the comparative lengths of the cambial initials and the conducting elements formed from them. The results appear to indicate that the size of the tracheids in Ginkgo and the Coniferæ closely resemble that of the cambial cells from which they are formed, the adult elements being slightly longer. In Dicotyledons, however, the fibre tracheids are considerably longer, though the vessel segments approximate closely in length to that of the cambial elements. The ultimate size of tracheary cells thus appears to be determined by the size of the cambial initials as well as by the conditions of their differentiation.

From a study of the somatic chromosomes in *Tradescantia*, and from that of his earlier work on Vicia (*Bot. Gaz.*, 1913), Sharp has come to the conclusion that in these two genera, at least, the chromosome split takes place during the prophase, and not during the telophase (*Amer. Jour. Bot.*). Moreover, he finds no evidence to support the contention that the longitudinal split of the chromosome as a whole is in any way due to the division of smaller component units.

An extensive examination of the anatomy of the peduncle in the Liliaceæ by Gatin (*Rev. Gen. Bot.*) has shown that the vascular supply varies not only in different genera, but is often distinctive for the different species of the same genus. The vascular strands usually form alternating rings, of three members each, which pass either direct into the floral parts or anastomose in a ring at the base of the flower. In the simplest cases there are three bundles only (e.g. *Ruscus aculeatus*, *Polygonatum verticillatum*, *Scilla autumnalis*). In *Scilla verna*, *Convallaria maialis*, *Allium cepa*, *Endymion patulus*, etc., there are two rings of three bundles each. In many cases these two sets of three bundles each can be recognised, but they are followed by an outer ring, consisting of a varying number of bundles, viz. 2 in *Allium ursinum* and *Endymion nutans*; 3 in *Allium moly*; 6 in *Agapanthus*, *Allium nigrum*, *Uropetalum*, etc.; 9 in *Ornithogalum umbellatum*; 15 in *Galtania condicans*, *Erythronium-dens-canis*. In *Mianthemum*, 2 bundles only are present, whilst at the other extreme we have considerable complexity, as, for example, in *Tulipa sylvestris*, where three whorls of three bundles each are followed by from 36–45 small bundles. The variation within the genus is well illustrated by *Scilla*, where the following arrangements occur: *S. autumnalis*, 3; *S. verna*, 3 + 3 *S. lilio-hyacinthus*, 3 + 3 + 3; *S. obtusifolia*, 3 + 3 + (2 − 3); *S. italica*, 3 + 3 + (4 − 6).

Bower has considered the effect of size on stelar structure (*Proc. Roy. Soc.*, Edinb.), and lays stress on the relation of surface of the stelar tissue to its bulk. Both in the ontogeny and phylogeny of various ferns, he points out, various devices are seen (e.g. solenostely, dictyostely, etc.) by which the relation of surface to bulk is increased. This feature is well exemplified by the enlargement and disintegration of the stele in fern tubers, and the " polystely " of the roots of palms is regarded as an illustration of the same influence at work.

Church, in the *Journal of Botany*, contributes an interesting theoretical paper on the lichen symbiosis.

Taxonomy.—The nature of the buff-coloured film present on certain whales (*Balænoptera* spp.) has been investigated by Bennett (*Proc. Roy. Soc.*, B. 91), who finds that it consists of diatoms, generally a species of *Cocconeis*, to which Nelson, in an appendix, gives the name of *C. seticola.*

Spencer Moore has described a number of new species from Australia (*Linn. Jour. Bot.*) belonging to the following genera: *Hibbertia, Candollea, Calandrinia, Commersonia, Boronia, Phebalium, Eriostemon, Oxylobium, Burtonia, Daviesia, Gastrolobium, Cassia, Acacia, Calythrix, Baeckea, Melaleuca, Siebera, Olearia, Helipterum, Calocephalus, Stylidium, Symphyobasis, Calogyne, Dampiera, Leucopogon, Hemigenia, Kochia, Anisacantha, Gyrostemon, Grevillea, Monotaxis, Casuarina, Distichostemon, Euroschinus, Micromertus, Leptospermom, Kunzea, Calothamnus, Eugenia, Trianthema, Mitrasacme, Dicrastyles, Grevillea, Dryandra, Euphorbia, Phyllanthus, Tritoxis, Croton,* and *Codiæum.*

The same author, in the *Journal of Botany*, describes new species of Euphorbiaceæ, Asclepiadaceæ, and Aristolochiaceæ.

Mr. Ridley records *Plantago cynops* from Kent, and T. and T. A. Stephenson give an account of the British palmate orchids, viz. *O. incarnata* L., *O. purpurella* Stephenson, *O. ericetorum* Linton, *O. prætermissa* Druce, *O. latifolia* L. and *O. fuchsii* Druce. The paper is illustrated by a useful plate in colours showing the flower types of these species and some hybrids.

PALÆOBOTANY. By Marie Carmichael Stopes, D.Sc., Ph.D.

The influence of the war is still felt, both in the relative smallness of the output in Palæobotany and in the slow distribution of those papers which have been published. But few memoirs dealing with the details of individual species or families have appeared in the years under review. Of these the following should be noted:

Conifers have been dealt with by Rudolf Florin (" Über cuticularstrukturen der Blätter bei einigen rezenten und fossilen

Coniferen," *Ark. f. botanik k. svensk. vetenskapsakad.*, vol. xvi, no. 6, pp. 1–32, pl. i, 1920), who has added to the now rapidly increasing data derived from careful study of the cuticular preparations. Florin endeavoured to find essential characters in the stomata of several recent genera, and then examined carefully cuticle preparations of the fossils *Sequoia Langsdorfii* Heer, and *Taxodium distichum* Rich, *miocenum* Heer, which he illustrated with good photographs. He supported the earlier diagnoses of the fossils as belonging to the genera to which they were allotted by older workers.

Earlier Gymnosperms are represented by the detailed anatomical work of Scott on the fertile shoot of *Mesoxylon* (*Ann. Bot.*, vol. xxxiii, pp. 1–21, pls. i–iii, 1919) and on *Ginkgo* by Nathorst (*Geol. Fören. forh.*, vol. xli, pp. 234–48, text figs., 1919). The fertile shoots of *Mesoxylon multirame* were found to be identical with the previously known axillary shoots; and that they were what would be described as *Cordaianthus* from impression-material was also evident. The shoot was bi-laterally symmetrical, and the vascular strands were individually mesarch. The place of *Mesoxylon* within the family of Cordaiteæ is thus finally and completely established. Nathorst's contribution dealt with the unique genus *Ginkgo*, as represented by Tertiary forms in Spitzbergen, in particular by the very widely distributed *G. adiantoides*.

The Cycadophyta were considered in general by Wieland ("Classification of the Cycadophyta," *Amer. Jour. Sci.*, vol. xlvii, 1919, pp. 391–406), who re-discussed a number of early theories, and who gave a tabulated list of all the genera of this great group arranged according to his current views. Florin (*Arkiv. f. botanik, K. svensk. vetensk.*, vol. xvi, 1920, pp. 1–10, pl. i) described of the leaf of *Nilssonia polymorpha* (the genus Wieland placed in his Holophytæ) the cuticle with stomates. He then entered into a consideration of the classification and systematic position of the genus *Nilssonia*; the omission of all reference to the only member of the genus of which the *internal* anatomy is known (*N. orientalis*, see Stopes, *Ann. Bot.*, vol. xxiv, pp. 389–93, pl. i, 1910) renders this consideration incomplete. The first "baby" of the group to be described from Europe was named by Stopes *Bennettites Scottii* (*Jour. Linn. Soc. Bot.*, vol. xliv, pp. 483–96, pls. xix, xx, text figs., 1920). The stem (of exceedingly small size) and its still enfolded young leaves, were well preserved anatomically, and described in full from a series of sections. This specimen elucidated a point long rather obscure in the mature American forms, and showed that there was a thick weft of hairs on the under surface of the leaves.

A great gap, both in phylogenetic and geological sequence,

exists between all the above and the next individual memoirs to be considered. The Old Red Sandstone cherts of Scotland are still yielding beautiful material, and two very important memoirs by Drs. Kidston and Lang (Parts II and III, *Trans. Roy. Soc. Edin.*, vol. lxii, 1920, pp. 603–627, pls. i–x, and pp. 643–80, pls. i–xvii) dealt with *Rhynia major*, *Hornea Lignieri*, and *Asteroxylon Mackiei*. These papers established the existence of four archaic types of early vascular Cryptogams, and demonstrated their internal structure clearly. Like all Kidston's palæobotanical work, they were illustrated by magnificent microphotographs. *Rhynia* and *Hornea* were extraordinarily primitive, leafless but spore-bearing land plants; *Asteroxylon*, somewhat more distinctly Pteridophytic in character, had simple leaves on its aerial stems. Their detailed studies of these plants led the authors to consider that one may possibly see " the convergence of Pteridophyta and Bryophyta backwards to an Algal stock." These papers read in conjunction with Halle's " Lower Devonian Plants " (*K. Svensk. vetensk. Handl.*, vol. lvii) places current knowledge of the Devonian flora on a totally different footing from that available a decade ago.

The year 1920 was notable, also, in seeing a new (the 3rd) edition of Scott's famous " Studies in Fossil Botany," the textbook which is used by all students of the science. Vol. I only has yet appeared, and is greatly enlarged. It will be reviewed in a separate article.

In Stratigraphic Palæobotany, the Tertiary was represented by a short paper on a new palm and other plants from Japan by Kryshtofovich, who is still a refugee in Tokio. Kryshtofovich pointed out the lack of knowledge about the true sequence of the various Tertiary plant-beds in the country, and the sources of error in comparing them with distant European and other floras, partly owing to the fact that the changes of climate appear to have differed from, and not tallied with, those in better-known regions. After studying a new palm and various Dicotyledons, the conclusion was drawn that the Sendai palm can be held to be Miocene or older. Florin also dealt with Japanese Tertiary in his paper on the fossil species of the genus *Salvinia* (*Bull. Geol. Inst. Upsala*, vol. xvi, pp. 243–60, pl. xi, 1919). He described *S. formosa* Heer from Japan, with good photographic illustrations, and gave a critical summary of other species included in the genus. The same author also described some Chinese Tertiary plants (*Svensk. Bot. Tidskrift*, vol. xiv, pp. 239–43), including *Pinus* and three Dicotyledons. From Peru a Miocene flora represented by a considerable variety of leaf impressions was described by Berry (*Proc. U.S. Nat. Mus.*, vol. lv, pp. 279–94, pls. xiv–xvii, 1919).

Berry also published an important memoir on the Upper Cretaceous of the Eastern Gulf (*U.S. Geol. Surv. Profess.*, paper cxii, pp. 1–141, pls. i–xxxiii, 1919), in which not only a large number of new species of Angiospermic leaves (and others) were described and finely illustrated, but the distribution in time and space and the ecology of the deposits were dealt with in the thorough way which we are learinng to expect from Berry.

The Lower Cretaceous is represented by further work from Walkom, who has for some time past been publishing on the too-long-neglected fossil floras of Australia. In "The Floras of the Burrum and Styx River Series" Walkom described many species, and from their nature clearly demonstrated that the deposits were of early Cretaceous, probably Neocomian age, and not Triassic, as they had been long considered. As the Burrum deposits contain commercially valuable coal, the determination of their correct stratigraphic position is not only of scientific, but of practical importance. In his presidential address on "Queensland Fossil Floras" (*Proc. Roy. Soc. Queensl.*, vol. xxxi, no. 1, 1919) Walkom emphasises the value of accurate palæobotanical work in clearing up doubtful points in stratigraphy.

The Hör sandstone of the Lower Lias of Sweden, rendered so famous by Nathorst's wonderful memoirs on *Dictyophyllum*, *Clathropteris*, *Nilssonia*, and other plants, has been discussed further by Antevs, who gave a monographic account of all the further remains collected at various times, and complete lists of the species and their distribution elsewhere (*K. Svensk. vetensk. Handl.*, vol. lix, no. 8, 1919, pp. 1–17, pls. i–vi).

The Carboniferous has received the attention of various authors. Renier dealt with the coal measure deposits of Belgium (*Ann. d. Mines Belg.*, 1920, vol. xxi), and published a comprehensive and valuable bibliography of the entire literature on the subject (*Ann. d. Mines Belg.*, 1920, vol. xxi, pp. 421–680). The same author published also a short note on discoveries on the rare Lepidodendroid genus *Omphalophloios* (*Ann. Soc. Sci. Bruxelles*, 1920, pp. 1–7). Bonnier, in "Notes Paléophytologiques sur le Carbonifère du Bassin de la Basse-Loire" (*Rev. gén. bot.*, vol. xxxi, 1919, pp. 1–15, pl. iii) gave descriptions of a small number of species, and some notes of general interest. He dealt in the following year (*Rev. gén. bot.*, vol. xxxii, 1920, pp. 1–13, pls. v and vi) with the fructifications of the Culm of Mouzeil, where the deposits are rich in impressions of a variety of small seeds.

Carpentier announced the discovery of the fructifications (hitherto unknown) of *Sphenopteris herbacea*; the fertile pinnules having reduced lobes not unlike those of the Archæopterideæ

(see *Compt. rend. Acad. Sci. Paris*, vol. clix, 1919, pp. 1–2). The same author recorded several fresh species of which the anatomical structure was preserved in silica, from the famous locality in the Vosges (*Compt. rend. Acad. Sci. Paris*, 1920, pp. 250–52).

A new genus of the Lepidodendreæ was described by Hörich as *Protasolanus Wieprechti* (*Jahrb. Preuss. Geol. Landesanst*, 1919, vol. xl, pp. 434–59, pls. xvi and xvii). A detailed study of the flora of the Gilfach Goch region of South Wales has long been on hand by David Davies, and one part of the results was published in the *Trans. Inst. Mining Engin.*, vol. lix, pt. 3, pp. 183–221, 1920.

COAL.—Much work has been done in recent years, both in this country and abroad, on the structure of coal, but Walcot Gibson, in his book "Coal in Great Britain," lost a golden opportunity of writing useful chapters on the structure of coal, for he reiterated several old loose or inaccurate statements on the subject, and omitted reference to the more recent and careful chemical and palæobotanical work. A paper "On the Four Visible Ingredients in Banded Bituminous Coal," by Stopes (*Proc. Roy. Soc. Lond.*, vol. xc, 1919, pp. 470–87, pls. xi–xii), described and differentiated four ordinarily visible portions in typical coal. These were Fusain (or mother of coal) and the three others to which new names were given, viz. Durain, Clarain, and Vitrain. The differences in characteristics between these were shown to exist also in chemical, physical, and microscopic details. This paper has led to a great deal of work by others on the further study of the four ingredients, notably that by Tideswell and Wheeler on the chemical constitution (*Jour. Chem. Soc.*, 1919, vol. cxv) and by Lessing on "The Behaviour of the Constituents of Banded Bituminous Coal in Coking" (*Trans. Chem. Soc.*, 1920, vol. cxvii, pp. 247–56) and on their Mineral constituents (same volume, pp. 256–65). Advance proof is to hand of more work on the theme, but it is not yet published. The considerable differences in plant-content, coking properties, water-content, and mineral ash, etc., which all the above work reveals as existing in visible zones in coal within a few inches or millimetres of each other necessitate a fundamental revision of our conceptions of the nature of coal as hitherto based on analyses and other results obtained from "bulk" samples.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, M.A., University College, Reading. (Plant Physiology Committee.)

Carbon Assimilation.—During the last two years the literature of photosynthesis has grown considerably. Investigations on the relations of the various conditions of carbon assimilation

and the rate of the process have continued, and in the main support the conclusions already drawn. With regard to temperature, the generally held opinion, resulting from the work of Miss Matthaei, that the rate of carbon assimilation is a little more than doubled by an increase of temperature of 10° C., has been called in question by W. H. Brown and G. W. Heise, who subjected the published results of previous investigators of this subject to a searching criticism. A. M. Smith ("The Temperature Coefficient of Photosynthesis: A Reply to Criticism," *Ann. of Bot.*, **35**, 517–36, 1919) has now replied in vindication of the generally held opinion, and defends the conclusions previously drawn by Miss Matthaei, F. F. Blackman, and himself. In an experiment on the influence of temperature on the rate of photosynthesis of *Ulva rigida*, W. J. V. Osterhout and A. R. C. Haas ("The Temperature Coefficient of Photosynthesis," *Journ. Gen. Physiol.*, **1**, 295–8, 1919) found that the rate of carbon assimilation in this alga at 27° C. is 1·81 times the rate of the process at 17° C. This value of the temperature coefficient, though somewhat smaller than the values found for other species by Miss Matthaei and Blackman and Smith, is yet of the same order, and is confirmatory of the earlier findings. Osterhout and Haas suppose that the assimilatory process involves two reactions, a light reaction with a low temperature coefficient and an ordinary chemical reaction with a high temperature coefficient. There is little that is new in this supposition, and among recent writers a somewhat similar suggestion has been made by Warburg, whose work is cited below, and also earlier by Willstätter and Stoll.

In the experiments of O. Warburg (*Biochem. Zeitsch.*, **100**, 230–70, 1919) on the influence of external conditions on the rate of photosynthesis, the experimental plant was the unicellular alga *Chlorella*, the determination of the carbon dioxide assimilated being made by the method of gas analysis of Haldane and Barcroft. With regard to the influence of carbon dioxide concentration, the rate of assimilation increases nearly proportionally to the concentration from zero up to about a concentration of 2×10^{-6}. Above this the rate of assimilation does not increase at so rapid a rate with increasing concentration of carbon dioxide until a concentration of about 10×10^{-6} further increase in concentration of carbon dioxide produces little further change. The relation of light intensity ("concentration of light energy") to the rate of assimilation is similar to that of carbon dioxide concentration. From these results the author assumes that at low concentrations of carbon dioxide the rate of assimilation is determined by a chemical action and not by diffusion. From the relation of light intensity to rate of assimilation it is supposed that each light intensity

corresponds to a particular concentration of a primary photochemical product, which is active in the chemical reaction according to the law of mass action.

Experiments were also carried out at different temperatures under different intensities of illumination. When the latter is high, and the concentration of carbon dioxide high also, the temperature coefficient is not constant, but decreases with increasing temperature from about 4·3 at 5° C. to 1·6 at 32° C. When the light intensity is low the temperature coefficient is about unity. This result forms very definite support to Blackman's Theory of Limiting Factors, which has lately been subjected to adverse criticism by W. H. Brown and G. W. Heise ("The Theory of Limiting Factors," *Philippine Journ. of Science*, C, **13**, 345–51, 1918). Warburg further observes that, if the light intensity is low, the same amount of light energy can bring about the utilisation of the same amount of carbon dioxide, whether the light is continuous or intermittent, whereas if the light intensity is high, more carbon dioxide is utilised by the same amount of light energy if the illumination is intermittent than if it is continuous. This result is exactly what would be expected on Blackman's view of the "time factor" in assimilation applied to the action of light.

The investigation of the internal factors controlling photosynthesis obviously presents considerably more difficulty than the examination of the influence of external conditions. Willstätter and Stoll ("Untersuchungen über Kohlensäureassimilation," Berlin, J. Springer, 1918) disputed the conclusion of Miss Irving that, in the development of the seedling, chlorophyll content does not act as a limiting factor, but that some other internal factor undergoing development during the development of the seedling limits the assimilation. This question has now been re-examined by G. E. Briggs ("The Development of Photosynthetic Activity during Germination," *Proc. Roy. Soc.*, B, **91**, 249–68, 1920), who, working with a new method of measuring the rate of assimilation depending on combining the oxygen evolved in assimilation with hydrogen at a surface of palladium black, has followed the increase of the rate of photosynthesis in seedling leaves of Phaseolus and other species from zero to the stage when the photosynthetic activity is nearly complete. In the experiments any increase in chlorophyll content was prevented, and so Briggs was able to confirm the conclusion drawn earlier by Miss Irving, that some internal factor other than chlorophyll is necessary for photosynthesis, and that this other factor develops with increasing age of the seedling. Briggs was also able to reconcile the apparently contradictory results of Willstätter and Stoll on the one hand and of Miss Irving on the other.

The energy relations of the photosynthetic process have formed the subject of a series of investigations by A. Ursprung. In the first place, the absorption curve of the green leaf was determined by means of a large spectrometer with a glass prism, a Hilger linear thermopile, and moving coil galvanometer. ("Über die Absorptionskurve des grünen Farbstoffes lebender Blätter," *Ber. deut. bot. Ges.*, **36**, 73–85, 1918.) A Nernst lamp was used as source of light and the leaves of *Phalaris arundinacea* var. *picta* used as experimental object. This variety has variegated leaves, and the amount of light absorbed by the green pigments of the leaf was determined by comparison of the absorption of green and white parts of the leaf otherwise similar. The absorbing power of a weak alcoholic extract of the leaf pigments was also examined. Both in the extract and in the living leaf absorption was found to take place throughout the green region of the spectrum. The maximum absorption in the red end of the spectrum was found between the B and C lines, but it appears that infra red rays can also be absorbed by chlorophyll. In the second paper ("Über die Bedeutung der Wellenlänge für die Stärkebildung," *Ber. deut. bot. Ges.*, **36**, 86–100, 1918) the influence of light of different wave-lengths, but of the same intensity, on assimilation as indicated by starch formation, was examined. The curve representing the relation between wave-length and power to form starch runs roughly parallel to the curve of absorption obtained by the author and described in his previous paper, but by no means absolutely so. The lower assimilation in the violet end of the spectrum, where, nevertheless, there is a high degree of absorption, may perhaps be explained as due to the fact that the stomata are less widely open in light of this end of the spectrum than in the red.

The discovery of the steps in the assimilatory process is clearly one of the most difficult problems of plant physiology; but this has not deterred many writers from putting forward theories of the chemistry of photosynthesis, and fresh theories, or rather variants of the existing theories, are still being propounded. It will suffice here to give references to papers by G. Woker ("Zum Assimilationsproblem," *Pfluger's Archiv f. ges. Physiologie*, **176**, 11–38, 1919) and P. R. Kögel ("Uber die Photosynthese des Formaldehyds und des Zuckers," *Biochem. Zeitsch.*, **95**, 313–16, 1919), in which hypotheses on the chemistry of the assimilatory processes are put forward. Of much greater interest, because they are the results of actual scientific investigation, are the conclusions of H. A. Spoehr with regard to the carbohydrate equilibrium in plants. From work on cacti Spoehr ("The Carbohydrate Economy of Cacti, Washington," Carnegie Institution, 1919) shows that low temperature and

high water content lead to the increase of simple sugars in relation to the more complex carbohydrates, while high temperature and low water content are associated with relative increase of polysaccharides, decrease of monosaccharides, and increase of pentosans. These results are highly suggestive with regard to the later stages of carbon assimilation, but a very great deal of work will have to be done before the materials are at all adequate for a lucid understanding of the chemistry of photosynthesis.

In a long paper Stoklasa (" Über die Radioaktivität des Kaliums und ihre Bedeutung in der chlorophylllosen und chlorophyllhaltigen Zelle," *Biochem. Zeitsch.*, **108**, 109–84, 1920) accounts for the greater concentration of potassium in the green parts of plants by the radioactivity of compounds of that metal, on account of which radioactivity these substances are concerned in the transformations of energy in the assimilatory processes. The evidence for this view will probably require considerably strengthening before it obtains anything like general acceptance.

An attempt has been made by H. H. Dixon and H. H. Poole (" Photosynthesis and the Electronic Theory," *Sci. Proc. Roy. Dublin Soc.*, **16**, 63–77, 1920) to determine whether there is any basis for a photoelectric theory of the activity of chlorophyll. The method used by the authors consists in connecting a film of chlorophyll with an electrometer in suitable fashion and measuring the ionisation current developed when the film was in the dark and when it was illuminated. An increase of current in the latter case might be regarded as evidence of an electronic theory of assimilation, electrons being driven out of the chlorophyll molecule, to which action might be attributed the transference of energy from sunlight to carbon dioxide in the green leaf. Although such an increase of current was noticed in the case of ultra violet light, no current of sufficient magnitude to support the hypothesis was observable with intense illumination of the visible rays.

ZOOLOGY. By Prof. Chas. H. O'Donoghue, D.Sc., F.L.S., Manitoba University, Winnipeg, Canada.

Protozoa.—Jameson has discussed " The Chromosome Cycle of Gregarines with Special Reference to *Diplocystis schneideri*, Kunstler " (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 2, Jan. 1920). The nuclear stages in all the various phases of the life-history have been fully investigated and discussed. The karyosome of this species results from a " Micronucleus " making its way inside a nucleolus, so that it is obviously composed of two distinctly differentiated parts, and thus the entire Gregarine

nucleus must be different from the nucleus in the higher forms. The reduction division is found to be the first division of the sporoblast in *D. schneideri*, and probably in other forms also. In none of the so-called reduction divisions has true reduction been demonstrated. Particular emphasis is laid on the fact of the difference between the Protozoa and the Metazoa, and it is pointed out that the practice of interpreting the Protozoa in terms of the metazoan cell has led to serious error in the past. "The relations between nuclear number, chromatin mass, cytoplasmic mass, and shell characteristics in four species of the genus *Arcella*" (*Jour. Exp. Zool.*, vol. xxx, no. 1, 1920) have been dealt with by Hegner. The problem of the investigation is fairly indicated by the title, and *Arcella* is very favourable for this type of work, since cytoplasmic and nuclear characteristics can be drawn easily at the same time from living examples; further, they are easy to keep, and offer certain measurable characters that are not modified by growth. The two chromatin masses in one specimen were found in general to be nearly equal, but those from different specimens varied considerably in size, and the mean quantity of chromatin in specimens of one family was much greater than in those of another. One line of *A. dentata* died out apparently because of the small amount of chromatin as compared with the cytoplasm. The paper finishes with a discussion on various problems arising out of the work.

Other papers include:

Cutler, "Protozoa Parasitic in Termites. Pt. II, *Joenopsis polytricha*, n. gen. n. spec. with Brief Notes on two New Species, *Joenopsis cephalotricha* and *Microjoenia axostylis*" (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 3, March 1920); Dawson, "An Experimental Study of an Amicronucleate *Oxytricha*. II, The Formation of Double-animals, or Twins" (*Jour. Exp. Zool.*, vol. xxx, no. 1, Jan. 1920); Hausman, "A Contribution to the Life-history of *Amœba proteus* Leidy" (*Biol. Bull.*, vol. xxxviii, no. 5, May 1920); Huxley, "Note on an Amœba-like Parasite from *Clavellina*" (*Quart. Jour. Micro. Sci.*, vol. xxxvi, pt. 3, March 1920); Kofoid, "A Critical Review of the Nomenclature of Human Intestinal Flagellates, *Cercomonas, Chilomastix, Trichomonas, Tetratrichomonas*, and *Giardia*" (*Univ. California Pub., Zool.*, vol. xx, no. 6, June 1920); Kofoid and Swezy, "On the Morphology and Mitosis of *Chilomastix mesnili* (Wenyon). A Common Flagellate of the Human Intestine" (*ibid.*, vol. xx, no. 5, April 1920); Kudo, "On the Structure of some Microsporidian Spores" (*Jour. Parasit.*, vol. vi, no. 4, June 1920); Todd, "*Spirochæta recurrentis*: a Filter Passer" (*ibid.*, vol. vi, no. 3, March 1920); Tyzzer, "The Flagellate Character and Reclassification of the Parasite producing 'Blackhead' in Turkeys—*Histomonas*, gen. nov. *meleagridis*, Smith" (*ibid.*); Wight and Lucke, "A New Bi-flagellate Protozoon of Man" (*ibid.*).

Invertebrata.—Papers include:

Hyman, "The Axial Gradients in Hydrozoa III, Experiments on the Gradient of Tubularia" (*Biol. Bull.*, vol. xxxviii, no. 6, June 1920).

Child has continued his studies on the phenomena of experimental reproduction in "Studies on the Dynamics of Morphogenesis and Inheritance in Experimental Reproduction. X, Head-frequency in *Planarai dorotocephalia* in Relation to Age, Nutrition, and Motor Activity" (*Jour. Exp. Zool.*, vol. xxx, no. 3, April 1920). It is found that the frequency with which heads are produced in regenerating pieces is lower in pieces of younger animals, in pieces of starved animals, and in pieces that are not frequently stimulated to activity after the operation. The range of head-forms is the same in all cases, showing that these stimuli are simply quantitative, and not qualitative, in their effect.

Other papers include:

Ciurea, "Sur la Source d'Infection du Chien et du Chat avec *l'Echinochasmus perfoliatus* (v. Ratz) et la Question d'Infection de l'Homme avec les Diatomes de la Famille des Echinostomides" (*Jour. Parasit.*, vol. vi, no. 4, June 1920); Cobb, "A Newly-discovered Parasitic Nematode (*Tylenchus mahogani*, n. sp.) connected with a Disease of the Mahogany Tree" (*ibid.*); Duncan, "On Acari from the Lungs of *Macacus rhesus*" (*Jour. Roy. Micro. Soc.*, pt. 2, June 1920); Faust, "Criteria for the Differentiation of Schistome Larvæ" (*Jour. Parasit.*, vol. vi, no. 4, June 1920); Kepner and Helvestine, "Pharynx of *Microstoma caudatum*" (*Jour. Morph.*, vol. 33, no. 2, March 1920); Kobayashi, "On a New Species of Rhabitoid Worms found in the Human Intestines" (*Jour. Parasit.*, vol. vi, no. 3); Magath, "Leuchloridium problematicum, n. sp." (*ibid.*); Schwarz, "The Biological Relationships of Ascarids" (*ibid.*); Scott, "Notes and Experiments on *Sarcosystis tenella* Raillet" (*ibid.*, vol. vi, no. 4, June 1920); van Cleave, "Notes on the Life Cycle of Two Species of Acanthocephalia from Freshwater Fishes" (*ibid.*). Yokogawa and Susumu Suyemori, "Observations on Abnormal Courses of Infection of *Paragonimus ringeri*" (*ibid.*); and Yoshida, "On the Resistance of Ascaris Eggs" (*ibid.*, vol. vi, no. 3, March 1920); Tannreuther, "The Development of *Asplanchnia ebbesbornii* (Rotifer)" (*Jour. Morph.*, vol. 33, no. 2, March 1920).

"The Development of the Starfish *Crossaster papposus*, Müller and Troschel," is described in considerable detail by Gemmill (*Quart. Jour. Micro. Sci.*, vol. lxiv, no. 2, Jan. 1920). There is a decided resemblance between the development of this species and that of *Solaster endeca*, which has been described previously by the same author. The archenteron early exhibits three divisions, of which the anterior gives rise to the cavity of the præoral lobe, hydroporic canal, hydrocoel, epigastric cœlom, axial sinus, stone canal, axial organ, internal oral perihæmal ring, and oral hæmal ring; the middle to all portions of the digestive cavity of the adult; and the posterior to the hypogastric cœlom, the pharangeal or perioral cœlom, the external oral perihæmal ring, the "genital rachis," and the aboral perihæmal sinus. The larva is not a feeding form, and consequently has no mouth, œsophagus, or anus.

Other papers include :

Gilchrist, "*Plancto-thuria diaphana*, g. et sp. n." (*Quart. Jour. Micro. Sci.*, vol. lxiv, no. 3, March 1920) ; and Heilbrunn, "An Experimental Study of Cell-division. 1, The Physical Conditions which determine the Appearance of the Spindle in Sea-urchin Eggs" (*Jour. Exp. Zool.*, vol. xxx, no. 2, Feb. 1920) ; Edmonson, "The Reformation of the Crystalline Style in *Mya arenaria* after Extraction" (*Jour. Exp. Zool.*, vol. xxx, no. 3, April 1920) ; Heilbrunn, "Studies in Artificial Parthenogenesis. III, Cortical Change and the Initiation of Maturation in the Egg of *Cumingia*" (*Biol. Bull.*, vol. xxxviii, no. 5, May 1920) ; Latchford, "Canadian Sphæridæ" (*Canadian Field Nat.*, vol. xxxiv, nos. 2 and 4, Feb. and April 1920) ; and McLearn, "Three New Pelecypods from the Coloradoan of the Peace and Smoky Valleys, Alta" (*ibid.*, no. 3, March 1920).

Gatenby has continued his series of papers on "The Cytoplasmic Inclusions of the Germ-cells. Part VI, On the Origin and Probable Constitution of the Germ-cell Determinant of *Apanteles glomeratus*, with a Note on the Secondary Nuclei" (*Quart. Jour. Micro. Sci.*, vol. lxiv, part 2, Jan. 1920). This determinant is almost entirely composed of an albuminous proteid, very dense and definitely basophil, and it is partly in the form of fine granules. It is not soluble in solvents that disintegrate yolk spheres, nor does it appear to contain fat or glycogen, and it is not destroyed by the fixations that remove the mitochondria. It takes origin as a concentrated area at the posterior pole of the young oöcyte, at first simply denser than the surrounding protoplasm, but later more clearly marked off. With "Some Habit Responses of the large Water-strider, *Gerris remigis*, Say. III" (*Amer. Nat.*, vol. liv, Jan. 1920), Riley concludes a series of investigations on the behaviour of this and other allied forms. They respond more readily to moving variations in light intensity than they do to stationary ones, and possibly also to variations in the moisture content of the atmosphere. The Gerrids being mainly apterous forms, were unable to migrate by flight, and so their actions were more easily observed. Apparently in finding their way back to a pool from which they had been moved, or in migrating to another pool when the one they are inhabiting dries up or becomes foul, they are mainly guided by the sense of sight, although moisture also may play a subsidiary part.

Other papers include :

Doncaster and Cannon, "On the Spermatogenesis of the Louse (*Pediculus corporis* and *P. capitis*), with some Observations on the Maturation of the Egg" (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 3, March 1920) ; Ewing, "A Gamasid Mite annoying to Man" (*Jour. Parasit.*, vol. vi, no. 4, June 1920) ; Leon, "Quelques Observations sur les Pediculides" (*ibid.*, no. 3, Mar. 1920) ; McIndoo, "The Olfactory Sense of Orthoptera" (*Jour. Comp. Neur.*, vol. xxxi, no. 5, June 1920) ; Shaffer, "The Germ-cells of *Cicada* (*Tibicen*) *septemdecim*" (*Biol. Bull.*, vol. xxxviii, no. 6, June 1920) ; Weidman, "Variation of the

Ovum (*Sarcoptes scabiei*) under Cover-glass Pressure" (*Jour. Parasit.*, vol. vi, no. 3, March 1920); and Yuasa, "The Anatomy of the Head and Mouth Parts of Orthoptera and Euplexoptera" (*Jour. Morph.*, vol. xxxiii, no. 2, March 1920).

Jordan has added a further paper to his series on "Studies on Striped Muscle Structure. VI, The Comparative Histology of the Leg and Wing Muscle of the Wasp, with Special Reference to the Phenomenon of Stripe Reversal during Contraction, and to the Genetic Relation between Contraction Bands and Intercallated Discs" (*Amer. Jour. Anat.*, vol. xxvii, no. 1, March 1920). "Contraction in striped muscle is associated with a genuine reversal of striations as regards a deeply staining substance of the dark disc of the sarco-style. This reversal of striations results in the formation of contraction bands in the contracted fibre." The beaded condition of the fibre is an artifact, and not the result of contraction. Anisotropy and the deep staining character of the dark disc are distinct phenomena, and reversal of striations concerns only the deeply staining substance of the dark disc. The contraction band is a new structure, and not an optical illusion, and the simplest type of intercallated disc is identical with this band.

Other papers include:

Bridges, "White-ocelli—an Example of a Slight Mutant Character of Normal Viability" (*Biol. Bull.*, vol. xxxviii, no. 4, April 1920); Hyman, "The Development of *Gelasimus* after Hatching" (*Jour. Morph.*, vol. xxxiii, no. 2, March 1920); MacDowell, "Bristle Inheritance in Drosophila. III. Correlation" (*Jour. Exp. Zool.*, vol. xxx, no. 4, May 1920); Nonidez, "The Meiotic Phenomena in the Spermatogenesis of Blaps, with Special Reference to the X-complex" (*Jour. Morph.*, vol. xxxiv, no. 1, June 1920); Pixell-Goodrich, "Determination of Age in Honey Bees" (*Quart. Jour. Micro. Sci.*, vol. lxiv., no. 2, Jan. 1920); and Zeleny, "A Change in the Bar Gene of *Drosophila melanogaster* involving further Decrease in Facet Number and Increase in Dominance" (*Jour. Exp. Zool.*, vol. xxx, no. 3, April 1920).

Chordata.—Norris and Hughes, in "The Cranial, Occipital, and Anterior Spinal Nerves of the Dogfish, *Squalus acanthias*" (*Jour. Comp. Neur.*, vol. xxxi, no. 5, June 1920), have provided a much-needed detailed account of the anterior nerves of this species. The work was done in the main upon a wax model reconstruction of the heads of the species in the pup stage. Useful diagrams are provided. Tracey has contributed two papers on the anatomical relations of the membranous labyrinth in fishes. "The Clupeoid Cranium in its Relation to the Swim-bladder Diverticulum and the Membranous Labyrinth" (*Jour. Morph.*, vol. xxxii, no. 2, March 1920), and as the Membranous Labyrinth and its Relation to the Procœlomic Diverticulum of the Swim-bladder in Clupeoids" (*Jour. Comp. Neur.*, vol. xxxi, no. 4, April 1920). Both papers present

a fully illustrated description of the relations between the parts indicated in their titles. The procœlomic diverticulum divides into two tubes, which pass forwards in a cartilaginous tube to the ex-occipital bone. They enter canals in the bones, and each divides into two, one passing to the pterotic and the other to the pro-otic bone, and each subdivision enlarges to form a vesicle.

Other papers include:

Grave, "*Amaroucium pellucidum* (Leidy) form *constellatum* (Verrill). I, The Activities and Reactions of the Tadpole Larva" (*Jour. Exp. Zool.*, vol. xxx, no. 2, Feb. 1920); Hosford, "A Note on the Hyobranchial Skeleton of *Squalus acanthias*" (*Anat. Rec.*, vol. xviii, no. 3, April 1920); Moodie, "The Nature of the Primitive Haversian System" (*ibid.*, vol. xix, no. 1, June 1920); Norris and Hughes, "The Spiracular Sense Organ in Elasmobranchs, Ganoids, and Dipnoans" (*ibid.*, vol. xviii, no. 2, March 1920); and Olmsted, "The Nerve as a Formative Influence in the Development of Taste-buds" (*Jour. Comp. Neur.*, vol. xxxi, no. 5, June 1920).

Reed has investigated in detail in a number of forms "The Morphology of the Sound-transmitting Apparatus in Caudate Amphibia, and its Phylogenetic Significance" (*Jour. Morph.*, vol. xxxiii, no. 2, March 1920). There are always two elements present, columella and operculum, and in the generalised condition they are independent. In a large number of families, however, they fuse to form a single plate. "The nature and relations of the sound-transmitting apparatus indicate that these structures came into their present state in a terrestrial environment. Results gained from this study, combined with others, tend to confirm the view that the present-day aquatic species are secondarily so; that some species give evidence of just beginning a return to an aquatic abode; that others are still strictly terrestrial, and likely to remain so."

Other papers include:

Allen, "The Results of Earliest Removal of the Thymus Glands in *Rana pipiens* Tadpoles" and "The Parathyroid Glands of Thyroidless *Bufo* Larvæ" (both *Jour. Exp. Zool.*, vol. xxx, no. 2, Feb. 1920); Burr, "The Transplantation of the Cerebral Hemispheres of *Amblystoma*" (*ibid.*, no. 1, Jan. 1920); Cummins, "The Role of Voice and Coloration in Spring Migration and Sex Recognition in Frogs" (*ibid.*, no. 3, April 1920); Jewell, "The Effects of Hydrogen Ion Concentration and Oxygen Content of Water upon Regeneration and Metabolism of Tadpoles" (*ibid.*, no. 4, May 1920); Larsell, "The Cerebellum of *Amblystoma*" (*Jour. Comp. Neur.*, vol. xxxi, no. 4, April 1920); Latimer, "The Weights of the Viscera of the Common Frog" (*Anat. Rec.*, vol. xviii, no. 1, Feb. 1920); Sagouchi, "Studies on the Glandular Cells of the Frog's Pancreas" (*Amer. Jour. Anat.*, vol. xxvi, no. 3, Jan. 1920); Smith, "The Hyobranchial Apparatus of *Spelerpes bislineatus*" (*Jour. Morph.*, vol. xxxiii, no. 2, March 1920); Vincent and Cameron, "A Note on an Inhibitory Respiratory Reflex in the Frog and some other Animals" (*Jour. Comp. Neur.*, vol. xxxi, no. 4, April 1920); and Wilson and Markham, "Asymmetrical Regulation in Anuran Embryos with Spi n

Bifida Defect" (*Jour. Exp. Zool.*, vol. xxx, no. 2, Feb. 1920); Rice, "The Development of the Skull in the Skink, *Emueces quinquelineatus* L. I, The Chondrocranium" (*Jour. Morph.*, vol. xxxiv, no. 1, June 1920); Pohlman, "A Consideration of the Branchial Arcades in the Chick, based on the Anomalous Persistence of the 4th Left Arch in a Sixteen-day Stage" (*Anat. Rec.*, vol. xviii, no. 2, March 1920).

"The Origin, Growth, and Fate of Osteoclasts, and their Relation to Bone Resorbtion" forms the subject of a paper by Arey (*Amer. Jour. Anat.*, vol. xxvi, no. 3, Jan. 1920). He concludes that the multinucleate giant cells usually termed osteoclasts include elements morphologically similar but nevertheless developmentally distinct. Osteoclasts appear to arise from the confluence of mesenchymal cells and the connective tissue of marrow, but their chief source is from the old osteoblasts and bone cells. They undergo retrograde changes and ultimately disappear. Only indirect and insufficient evidence points to osteoclasts as active agents of bone resorbtion, and it is probably better to regard them as degenerating and fused osteoblasts.

Other papers include:

Addison, "Histological Study of the Spleen of the Rabbit under Heightened Phagocytic Activity" (*Amer. Jour. Anat.*, vol. xxvi, no. 3, Jan. 1920); Adelmann, "An extreme Case of Spina Bifida with Dorsal Hernia in a Calf" (*Anat. Rec.*, vol. xix, no. 1, June 1920); Anthony, "New Rodents and New Bats from Neotropical Regions" (*Jour. Mammal.*, vol. i, no. 2, Feb. 1920); Asami, "Observations on the Follicular Atresia in the Rabbit's Ovary" (*Anat. Rec.*, vol. xviii, no. 4, May 1920); Badertscher, "Eosinophilic Leucocytes in the Thymus of Postnatal Pigs" (*ibid.*, no. 1, 1920); Baumgartner, Nelsen, and Dock, "Development of the Uterine Glands in Man" (*Amer. Jour. Anat.*, vol. xxvii, no. 2, May 1920); Beckwith, "Note on a Peculiar Pancreatic Bladder in the Cat" (*Anat. Rec.*, vol. xviii, no. 4, May 1920); Bowles, "The California Grey Squirrel an Enemy to the Douglas Fir" (*Amer. Forest.*, vol. xxvi, Jan. 1920); Congdon, "The Distribution and Mode of Origin of Septa and Walls of the Sphenoid Sinus" (*Anat. Rec.*, vol. xviii, no. 2, March 1920); Cooper, "A Case of the Inferior Vena Cava uniting with the Azygos Vein in the Dog" (*ibid.*, vol. xvii, no. 5, Jan. 1920); Craigie, "On the Relative Vascularity of Various Parts of the Central Nervous System of the Albino Rat" (*Jour. Comp. Neur.*, vol. xxxi, no. 5, June 1920); Draper, "The Prenatal Growth of the Guinea-pig" (*Anat. Rec.*, vol. xviii, no. 4, May 1920); Hanna, "Mammals of the St. Matthew Islands, Bering Sea" (*Jour. Mam.*, vol. i, no. 3, May 1920); Hanson, "The Development of the Shoulder-girdle of Sus Scrofa" (*Anat. Rec.*, vol. xviii, no. 1, Feb. 1920); Hausmann, "The Microscopic Identification of Commercial Fur Hairs" (*Sci. Month.*, Jan. 1920); Hinton, "The Subspecies of *Paraxus flavivittis*, Peters" and "Three New Species of *Spalax monticola*" (both in *Ann. and Mag. Nat. Hist.*, March 1920); Hollister, "Two New East African Primates" (*Smithsonian Misc. Coll.*, vol. lxxii, no. 2, Jan. 1920); Hornaday, "The Birth of a Pigmy Hippopotamus" (*Zool. Soc. Bull.*, vol. xxiii, no. 1, Jan. 1920); Horsley, "A Description of a Six-legged Dog" (*Anat. Rec.*, vol. xix, no. 1, June 1920); Howell, A. B., "A Study of the California Jumping Mice of the Genus *Zapus*" (*Univ. Cal. Pub. Zool.*, vol. xxi, no. 5, May 1920); Howell, A. H., "A New Race of the Florida Water-rat" (*Jour. Mamm.*, vol. i,

no. 2, Feb. 1920); Huntington, "A Critique of the Theories of Pulmonary Evolution in the Mammalia" (*Amer. Jour. Anat.*, vol. xxvii, no. 2, May 1920); Ibsen, "Linkage in Rats" (*Amer. Nat.*, vol. liv, Jan. 1920); Jackson and Stewart, "The Effects of Inanition in the Young upon the Ultimate Size of the Body and of the various Organs in the Albino Rat" (*Jour. Exp. Zool.*, vol. xxx, Jan. 1920); Kittelson, "Effects of Inanition and Refeeding upon the Growth of the Kidney in the Albino Rat" (*Anat. Rec.*, vol. xvii, no. 5, Jan. 1920); Larsell, "Pancreatic Bladders" (*ibid.* vol. xviii, no. 4, May 1920); Lewis, "The Course of the Wolffian Tubules in Mammalian Embryos" (*Amer. Jour. Anat.*, vol. xxvi, no. 3, Jan. 1920); Mann, "A Comparative Study of the Anatomy of the Sphincter at the Duodenal End of the Common Bile-duct, with Special Reference to Species of Animals without a Gall-bladder" (*Anat. Rec.*, vol. xviii, no. 4, May 1920); Mann, Brimhall, and Foster, "The Extrahepatic Biliary Tract in Common Domestic and Laboratory Animals" (*ibid.*, no. i, Feb. 1920); Meyer, "The Case and Problem Method in Anatomic Neurology" (*ibid.*, no. 4, May 1920); Miller and Gidley, "A New Fossil Rodent from the Oligocene of South Dacota" (*Jour. Mamm.*, vol. i, no. 2, Feb. 1920); Senior, "The Development of the Human Femoral Artery, a Correction" (*Anat. Rec.*, vol. xvii, no. 5, Jan. 1920); Sheppard, "Hermaphroditism in Man" (*ibid*, vol. xix, no. 1, June 1920); Smith, "A Study of the Lipoid Content of the Kidney Tubule" (*Amer. Jour. Anat.*, vol. xxvii, no. 1, March 1920); Stewart, "The Development of the Cranial Sympathetic Ganglia in the Rat" (*Jour. Comp. Neur.*, vol. xxxi, no. 3, Feb. 1920); Sowerby, "A New Three-toed Jerboa from China" (*Ann. and Mag. Nat. Hist.*, vol. v, Mar. 1920); Strong, "An Inexpensive Model on the Principal Spinal-cord and Brain-stem Tracts" (*Anat. Rec.*, vol. xix, no. 1, June 1920); Sutton, "The Fascia of the Human Orbit" (*ibid.*, vol. xviii, no. 2, March 1920); Thomas, "Two New Species of *Sylvilagus* from Columbia"; "A New Shrew and two New Foxes from Asia Minor and Palestine"; The Generic Positions of '*Mus*' *nigricauda*, Thos., *woosnami*, Schwann"; "A New *Taphozous* from the Soudan"; "A New Marmoset from the Peruvian Amazons" (all from *Ann. and Mag. Nat. Hist.*, vol. v, Jan. 1920); "Some Notes on *Babirussa*," "A Further Collection of Mammals from Jujuy" (both *ibid.*, Feb. 1920); "Two New Asiatic Bats of the Genera *Tadarida* and *Dyacopterus*," "Four New Squirrels of the Genus *Tamiops*" (both *ibid.*, March 1920); Vincent and Arnason, "The Relationship between the Thyroid and Parathyroids" (*Endicrino*, vol. iv, no. 2, June 1920); Wilhelmi, "A Case of Double Ureter in Man with Failure of Development of the Kidney about the Aberrant Ureter" (*Anat. Rec.*, vol. xviii, no. 2, March 1920); Wodsedalek, "Studies on the Cells of Cattle, with Special Reference to Spermatogenesis, Oögonia, and Sex-determination" (*Biol. Bull.*, vol. xxxviii, no. 5, May 1920); and Wroughton and Cheesman, "A New Species of *Mellivora* from Somaliland" (*Ann. and Mag. Nat. Hist.*, vol. v, Feb. 1920).

One of the most striking lines of cytological research in recent years has been in the direction of studying the special extra-nuclear bodies included within the cytoplasm, and overlooked for a considerable time owing to inadequate fixation. They have formed the basis of series of papers by Gatenby, who has added a general survey in "The Cytological Inclusions of the Germ-cells. Part VII, The Modern Technique of Cytology" (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 3, March 1920). The paper provides a classification of the cell inclusions, and discusses their nature and the problems they present. This is followed by an account of the various modern methods of

fixation and staining that not merely show these bodies, but also enable them to be differentiated from one another. The same author, in conjunction with Woodger, deals with an allied subject in a paper " On the Relationship between the Formation of Yolk and the Mitochondria and Golgi Apparatus during Oögenesis " (*Jour. Roy. Micro. Soc.*, June 1920). The origin and development of these bodies are here studied more particularly as they occur in vertebrates. The same journal also contains a paper on " Methods for the Demonstration of the Golgi Apparatus in Nervous and other Tissues," by Da Fano.

Harvey, in " A Review of the Chromosome Numbers in the Metazoa II " (*Jour. Morph.*, vol. xxxiv, no. 1, June 1920). provides a list of all the chromosome numbers recorded from Annelida, Arthropoda, and Coelenterata from January 1916—December 1918, thus bringing a previous list up to date. It also tabulates those of all the other Metazoa from 1878—December 1918.

Other papers include:

Allen, " A Quantitative and Statistical Study of the Plankton of the San Joaquin River and its Tributaries in and near Stockton, California, in 1913 " (*Univ. Cal. Pub., Zool.*, vol. xxii, June 1920); Carleton, " Observations on an Intra-nucleolar Body in Columnar Epithelium Cells of the Intestine " (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 3, March 1920); Cowdry, " Anatomy in Japan " (*Anat. Rec.*, vol. xviii, no. 2, March 1920); Firket, " On the Origin of the Germ Cells in Higher Vertebrates " (*ibid.*, no. 3, April 1920); Gamble and Hitchcock, " The Use of Stereoscopic Roentgenograms in studying the Circulatory System of Vertebrates " (*ibid.*, no. 2, March 1920); Gough, " A Method of Injecting the Blood-vessels for Roentgenological Studies and simultaneously Embalming " (*ibid.*); Grey, " The Effects of Ions upon Ciliary Movement " (*Quart. Jour. Micro. Sci.*, vol. lxiv, March 1920); Loeb, " On the Recreation of Tissues towards Synogenesio- Homo- and Hetero-toxins, and on the Power of Tissues to discern between Different Degrees of Family Relationship," and " The Individuality-differential and its Mode of Inheritance " (both in *Amer. Nat.*, vol. liv, Jan. 1920); Pearl, " Certain Evolutionary Aspects of Human Mortality Rates " (*ibid.*); Reighard, " The Storage and Handling of Wall Charts " (*Anat. Rec.*, vol. xix, no. 1, June 1920); Souchon, " A New Permanent Solution for the Preservation of Anatomic Preparations, the Souchon Solution of Calcium Chloride " (*ibid.*, no. 4, May 1920); and Sumner, " Geographic Variation and Mendelian Inheritance " (*Jour. Exp. Zool.*, vol. xxx, no. 3, April 1920).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc., Zoological Laboratory, Cambridge.

In the last number of Science Progress I referred to an article by Mr. M. C. Burkitt, which had appeared in the *Proceedings of the Prehistoric Society of East Anglia* (vol. iii, pt. 2, 1919–20), and dealt with the highly interesting and important subject of the correlation of the Glacial and Interglacial strata of Great Britain, and especially of East Anglia, with the Glacial and Interglacial strata on the Continent; and more particularly

with the correlation of English results with Prof. Penck's well-known scheme of four great Glacial Periods. The subject is far from being new, and I have referred to it more than once before in these notes. I had omitted to notice, however, that at the end of an article entitled, "A Series of Humanly-fashioned Flints from Mundesley," in the same number of the above-mentioned *Proceedings*, Mr. Reid Moir had discussed this very subject, and had published a correlation of the East Anglian results with the Penck scheme, his correlation being as detailed as, but totally different from, that of Mr. Burkitt. Since we have here two different interpretations of this most important problem, one requires no excuse for returning to the matter. Mr. Moir's scheme was published earlier than that of Mr. Burkitt, but his point of view is less original than that of the latter writer.

Readers of SCIENCE PROGRESS will probably remember that James Geikie published an elaborate scheme of British Glacial periods, which he was able to bring into close accord with Penck's ideas. Geikie made a beautiful correlation in the Munro Lectures of 1913. He had four great Glacial Periods, which appeared to fit in well with those of Penck, though he called them by different names. Penck had three subsequent minor cold phases: Geikie only two. But this discrepancy was only apparent. Penck's first minor cold phase was Pleistocene; his other two were Recent. Both Geikie's were Recent. Hence Geikie's corresponded with Penck's second and third (or sixth and seventh, if the reader prefers). Penck's cold periods are called respectively Günz, Mindel, Riss, Würm, Bühl, Gschnitz, and Daun. Only the first four are usually referred to as "Glacial Periods," but Geikie called them all "Glacial Periods." This, however, is mainly a question of words. It remains to be explained that there is a dispute about the correlation of the Palæolithic Ages with the Glacial Periods—quite a distinct correlation-problem from that which we are discussing, be it noted. Penck placed the Older Palæolithic in the Mindel-Riss Interglacial, and the Aurignacian in the Riss-Würm Interglacial. Most French writers place the Older Palæolithic in the Riss-Würm Interglacial, and the Aurignacian in the interval between Würm and Bühl, which interval is known as the "Achen Recession." Geikie was in accord with Penck.

We turn now to the East Anglian series more particularly. The key-problem here is, as Mr. Burkitt shows, the age of the Chalky Boulder Clay. Geikie regarded it as Mindel. Other writers, including Prof. Sollas (see *Ancient Hunters*, p. 560), have suggested that it is Riss. Mr. Burkitt thinks that it is Würm. In reference to previous writers, I may mention that

there are sentences in Mr. Moir's article which might, I think, be unintentionally misleading to uninformed readers. He speaks of "making a beginning" with this correlation. But a "beginning was made" years ago, as we have seen. Mr. Moir, however, gives a detailed and most interesting scheme. He adopts the view that the Chalky Boulder Clay is Riss. The other beds then fall into line. A hill-wash near Ipswich represents the Würm, and contains Solutréan implements. The Middle Glacial Gravel represents the Mindel-Riss Interglacial Period. The Cromer Till is Riss. The Cromer Forest Bed is Günz-Mindel. He does not make it clear where he places the Günz itself, as he cannot mean (what one phrase seems to imply) that the Günz preceded the Coralline Crag. Except, therefore, for the doubt about the Günz, Mr. Moir suggested, earlier and in much greater detail, the same scheme as that which I said appeared to be indicated by Prof. Marr's work, when I was reviewing the latter in the last number of this journal. The Günz is very possibly represented by Marr's cold period of the Chillesford Beds. It cannot have anything to do with the Coralline Crag.

Now, it will be seen that the Moir scheme is not fundamentally new, since it turns on the dating of the Chalky Boulder Clay as Riss, which is not a new suggestion. On the other hand, so far as I know, Mr. Burkitt is the first to suggest that it is Würm. We have, therefore, these two schemes from which to choose. Both Mr. Burkitt and Mr. Moir accept the conclusion that the Older Palæolithic is earlier than the Chalky Boulder Clay. The dispute, therefore, has a highly important bearing on that other correlation problem—of the Palæolithic Ages with the Glacial Periods—to which I have referred. Now, Mr. Burkitt believes that the French time-table (the Older Palæolithic post-Riss and the Aurignacian post-Würm) is well founded. But, if the Moir scheme is correct, it goes ill with the French time-table in this country, and, doubtless, abroad also.

I am exceeding my space, but there are two suggestions of a general character which I would make. The Achen Recession was much less warm than the real interglacial periods, the snow-line being at least 700 metres below its present level. Hence, in these northern latitudes, it may well be impossible to distinguish Bühl from Würm. If there is a cold phase missing as a separate entity in Britain, it is likely to be Bühl. And a second point. On the Continent, Würm was much less severe than Mindel or Riss. It was probably also less severe in Britain. But the Chalky Boulder Clay is close to the extreme southern limit of any glaciation in England. Hence the Chalky Boulder Clay would appear to be more likely to be Riss (or even Mindel) than Würm. These considerations appear to favour the Moir correlation.

MEDICINE By R. M. Wilson, M.B., Ch.B.

A New Law in Medicine.—The most interesting medical announcement during the last few months has come from the new clinic established by Sir James Mackenzie at St. Andrews. It will be recalled that Sir James retired from consulting practice in London with the avowed object of devoting the rest of his working life to the study of the early symptoms of disease. He held that, so far as the study of disease in organs is concerned, we have reached a point beyond which progress must necessarily be very slow. On the other hand, he believed that, before organs begin to break down under the stress of disease, there is a period of infection or intoxication referable to the whole system, but not definitely located.

This is the period of early signs and symptoms. The symptoms are present, but, because they are not yet referable to any one system or organ, are largely discounted, with the result that opportunities which can never recur are missed.

Necessarily the study of this vast and vague field demands tireless energy. A beginning, however, was made in certain directions, notably in connection with pain experienced over the heart. This form of pain had been related to disease of the heart by many observers. Investigation, however, soon showed that it is met with in young and apparently strong persons who have no signs of heart disease and who present no symptoms of heart failure. It was manifest, therefore, that, while the pain could arise from actual disease of the organ, it could arise also in the absence of organic disease.

There was a problem here of great difficulty. Happily, some progress towards its solution had been made by a previous observation that pain is not located in an organ, but in the skin and muscle covering the organ. This presupposes a nervous connection between the organ lying deeply and the skin covering it. In the case of a diseased organ, the skin expresses or signals the difficulty experienced. Thus, when the possessor of a diseased heart attempts effort, pain comes on at once, and the skin of the left breast becomes tender to touch.

We have in this picture three factors : an organ in distress ; a nervous reflex arc connecting the organ with the surface ; and, finally, the painful area of the skin. Clearly enough, alteration of any one of these factors may produce disturbances in the others. For example, strenuous exertion will cause the heart to overwork, and so cause pain in a healthy man. A smaller degree of exertion in the case of disease of the heart will cause pain. There remains the third case—pain caused by a small degree of exertion when the heart is healthy and the skin area normal.

This third case can be explained only by supposing that the nervous system has become unduly excitable, so that a small effort on the part of the heart sets up a great effect. And we can only explain the increase in nervous excitability by supposing that toxins of disease are acting upon the nerves. We thus reach the position that the presence of a toxin in the body may so raise nervous excitability that a healthy heart, in its normal responses to effort, sets up a symptom—pain—which is identical with the pain occasioned by a diseased heart.

We have thus found an earlier period of disease than that which can be recognised by examination of organs. We have also disentangled a law—that some of the symptoms of disease are exaggerations of normal reflexes. Because the normal heart, under great effort or strain, gives rise to the same kind of pain.

Sir James Mackenzie and his co-workers have already found that what is true of the symptom—pain—is true also of many other symptoms ; for example, breathlessness and exhaustion. Thus, a new position in regard to medical research is being taken up, and the study of early symptoms entering upon a practical stage.

Some fascinating possibilities open before the mind. If, for example, the poisons of disease act on various portions of the nervous system and so disorganise the nervous control of organs, it may well be that, in process of time, those organs will break down from this reason alone. Thus, destruction of the lung in tuberculosis may not be so much the cause of the disease as one of its effects. The cause may be remote, in a general poisoning of the system, and the local focus of trouble may arise owing to this general poisoning. Thus a new world comes into view.

We wrote last month of the third partner in disease—the factor which prepares the soil for the seed. It will be seen that the search for these third partners cannot leave out of account the work at St. Andrews, which indeed offers a field for pioneer endeavour of the most fascinating description.

ARTICLES

THE PHYSICAL INVESTIGATION OF SOIL

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Now that the methods and principles of many branches of science are being applied to agriculture, a worker engaged on a problem apparently lying in one of these divisions frequently finds that the investigation extends into other divisions as well. There are no definite boundaries within agricultural science, although, for convenience in organisation, certain broad lines of research can be marked off and attacked in separate institutions and laboratories. It must, therefore, be borne in mind that the physical problems presented by soil cannot always be clearly defined without reference to other branches of science. For instance, some of these properties depend very much on the type of soil, for information on which recourse must be had to geology and physical geography. The moisture conditions in the soil are largely controlled by the climate, and hence meteorology must be considered in this respect. Again, those soluble nutrient salts in the soil moisture which are taken up by the plant roots have to be investigated by chemical and physico-chemical, as well as physical methods.

In general, the physical study of soil may be expected to give results which are capable of application in two directions: firstly, to elucidate the reasons for the comparative advantages of the many forms of cultivation operations employed in farming practice; and, secondly, to supply information to the biologist on the physical environment in which the plant roots, insects, and micro-organisms exist in the soil.

Historically, soil physics has grown up in two definite periods. When the scientist first turned his attention seriously to agricultural problems there were many obvious phenomena, the investigation of which promised definite information about the various factors controlling plant growth. There were, for instance, questions of water supply, of the amount of

available water in different types of soil, of the manner of its distribution over the minute soil grains, and so on. Early in the nineteenth century, Davy, in the course of a series of lectures under the auspices of the newly formed Board of Agriculture, laid stress on the importance of the physical properties of soil, while Schübler in Germany, and Boussingault in France, laid the foundations of the subject in a series of classical experiments.

They regarded the soil as a framework of mineral particles of all shapes and sizes, something like a heap of sand and considered that the soil moisture was spread over these particles in the form of a thin film. The movement of the water in the soil was controlled by the force of gravity, capillary attraction in the minute pore spaces between the grains, evaporation from the soil surface, imbibition by plant roots, and so on. The well-established principles of pure physics were directly applicable to a soil of this type, and it was rightly concluded, from the methods and evidence then available, that a fairly complete survey of soil physics had been made. This was the first historical period, which closed about the year 1850.

At this time the famous researches of Lawes and Gilbert, and Warington at Rothamsted, and Liebig in Germany, directed world-wide attention to agricultural chemistry, and soil physics fell into the background. A small amount of work was still done, however, by agricultural chemists and others, who found—as we have already mentioned—that the solution of a particular problem demanded some knowledge of the physical environment in the soil.

The opening of the second historical period may be dated from 1900, when Warington published a monograph on the physical properties of soil. He discussed in detail the knowledge obtained from the old water-film and sand-grain hypothesis, and indicated certain soil characteristics whose explanation demanded at least an extension of this hypothesis. This extension was first given, apparently, by J. Dumont in France, who assumed the soil particles to be coated, more or less completely, with a film of colloidal materials derived, probably, both from the mineral material of the soil—zeolites, etc.—and from the organic residues of plants. On this view, soil should show more or less marked colloidal properties such as those demonstrated earlier, in the famous researches of Van Bemmelen at Leiden, and hence, considerable advances should be possible in our knowledge of its physical properties, by applying to it the powerful technique and theories of the colloidal state of matter. We are hardly more than at the beginning of this second historical period, but sufficient results have already been obtained—first of all in Germany, later in

this country and in America—to show quite definitely that the colloidal theory, which has extended into so many different branches of science, will be of the greatest use to the soil physicist in his study of the very complicated physical constitution of soil.

Soil Physics may be divided into four broad sections:

1. The dimensions of the individual particles and the manner of their arrangement.
2. Soil moisture.
3. Soil temperature.
4. Soil atmosphere.

Discussion of each of these sections, of which the first two are the most important, follows immediately below.

(1) Dimensions and Arrangement of Soil Particles

Since soils contain particles of all sizes down to ultra-microscopic dimensions, it is possible to classify soils by sorting the particles into groups or fractions, each of which contains all particles within certain specified limits of diameter. This process is known as mechanical analysis, and, by suitably choosing the various diameters, the characteristics of the soil can be estimated with reasonable accuracy from the proportions of each soil fraction present. The details of the process vary in different countries; the customary division in British practice is as follows:

Conventional name of fraction.	Average diameter of particles, mm.
Fine gravel	above 1·0
Coarse sand	1·0 to 0·2
Fine sand	0·2 to 0·04
Silt	0·04 to 0·01
Fine silt	0·01 to 0·002
Clay	below 0·002

The separation, which is based on the assumption that the individual soil particles follow Stokes's well-known law for the fall of a sphere in a viscous liquid, may be done either by sedimentation or elutriation. In the former case the soil fractions are obtained by repeated settling in beakers through a fixed depth of water for a definite time, and in the latter, by means of upward currents of water of known velocity, which carry off all particles below a certain size.

A separation of the soil particles into six fractions must be regarded as of only qualitative value, although for most practical

purposes it is sufficient. It would be possible, in cases where more exact divisions were required, largely to increase the number of the fractions, but the experimental work would become very laborious, whether sedimentation or elutriation was used. Odén, in Upsala, has recently described an ingenious and compact apparatus which will effect this detailed subdivision. A soil suspension falls through water on to a flat counterpoised plate and the time intervals for successive small equal increments of weight are automatically recorded. These data, when analysed mathematically, enable a distribution curve to be constructed, with radii as abscissæ, and the proportion of particles lying between any two radii is given in the usual way, by the area under the curve between these specified radii. Odén's method will be of the utmost value to soil physicists: it may be mentioned here that he has already demonstrated that two samples of Globerina Ooze, one from the Atlantic and the other from the Pacific, contained no particles whatever between certain limits of radii—between 12 and 20μ in the former, and 16 and 30μ in the latter. This obviously important deduction, which follows easily from Odén's treatment, could not be obtained with any certainty by other methods.

Of the various fractions separated by ordinary mechanical analysis, clay is the most important from the physical point of view. It shows Brownian movement in water, it is flocculated by acids and salts, deflocculated by dilute ammonia, and it possesses considerable plasticity. In fact, the physical properties of the vast majority of soils depend largely on the percentage of the clay fraction present, which imposes its colloidal characteristics on the soil mass. The earliest attempt to estimate what proportion of the clay fraction itself was in the colloidal condition was made by Schloesing in 1874. By evaporating the water which had stood for many weeks in contact with clay, he obtained a very plastic material of a horny nature, which was translucent in layers up to 1 mm. in thickness. This substance constituted only about 2 per cent. by weight of the original clay. Recently Tempany, by measuring the shrinkage of blocks of moist compacted soil, has shown that the percentage of colloidal clay is in excess of 2 per cent., and that its amount varies from one clay to another. It is probable that the methods of Tempany and Schloesing give respectively the upper and lower limits, and that the actual percentage lies between them. Certain of the soils used by Tempany were very difficult to keep in good cultivation, as they became sticky in wet weather, and dried to hard clods in the summer. These soils did not necessarily contain the greatest amount of clay, but, of the clay present, a greater percentage was in the colloidal

condition than was the case in the more easily cultivated soils.

Generally speaking, sandy soils can be subjected with impunity to cultivation at times when clay soils would be seriously injured by similar treatment. This injury shows itself in a destruction of the soil tilth: the friable and porous surface, characteristic of good tilth, gives place to one which is sticky and practically impervious to water or air, and which can be brought back only slowly into a better condition.

Although these two states of clay present obvious differences to visual inspection, and can be readily controlled by the farmer, very little is known about them. The grains in a soil in good tilth are loosely attached to each other, forming small porous aggregates, or compound particles, which may be likened in some ways to sponges. The formation of these aggregates from the individual particles is closely parallel to the flocculation, by acids and salts, of clay suspended in a bulk of water. Lime is used in practice to improve the physical condition of soils, and also produces flocculation of a clay suspension in the laboratory, and, although the proportion of water in natural soils is many times less than that used in flocculation experiments in the laboratory, the effects are so similar that one must conclude that they are produced by the same causes.

It will, therefore, be seen that the colloidal theory is of great importance in soil problems. One of the most important of its immediate applications is in the investigation of the jelly-like material supposed to exist on the surface of the soil particles, because of the above-mentioned statement that it is the forces associated with this coating which are concerned in the production of good tilth. At present it is nearly always necessary to examine the physical effects produced by the coating as a whole, as its composition is exceedingly complex, and the necessary chemical and physico-chemical data relating to its constitution are not yet complete. Physical measurements carried out by the writer indicate that, as far as the relationship of this material to the soil moisture are concerned, the inorganic colloids derived from the mineral material of the soil are of more importance than the organic colloids contained in the decomposed plant products, or humus. In the production of good tilth, however, it is probable that these humus colloids have increased importance. They will effect a certain mechanical separation of adjacent soil aggregates, and thus keep the soil more open, and they may also have a weak cementing effect which would assist the formation and maintenance of soil aggregates from the individual particles. Recently Comber has published some interesting results bearing

on the formation of compound particles. He suggests that the clay particles are protected by a coating of an emulsoid character, and thus partake of the properties of an emulsoid. The larger soil particles, such as silt, by themselves are suspensoid in character. Soil aggregates are supposed to be built up around a large nucleus, the outer layers of the compound particles being composed of clay particles, which impose their emulsoid nature on the whole aggregate, and therefore, normally, on the whole soil. One practical application of this view lies in the fact that it may be possible to devise an ameliorative treatment for a class of soils known as " silt soils." Mechanical analysis shows the presence of a large proportion of the " fine silt " fraction, and very little clay. The difficulty of working these soils in practice is ascribed by Comber to the inability of the small amount of clay to impose its emulsoid character on the large suspensoid surface exposed by the fine silt, and one possible remedy advanced is to apply emulsoid materials before liming.

It is now necessary to consider briefly some other consequences arising from the loose packing together of the soil-grains. There is a certain amount of space not taken up by soil particles, and this will be occupied by the soil water and soil atmosphere, whose proportions will vary with the soil conditions. The volume of this pore space varies roughly from 30 per cent. to 50 per cent., according to the type of soil, and is usually least in sandy soils. The physical conditions within it are of vital importance, since they will control the available water supply to the plant roots, the environment of the micro-organisms engaged in producing plant food, and so on. In order to form a mental picture of the pore space, it may be regarded as a bundle of capillary tubes, irregular in length, width, and direction, along which the soil moisture and soil atmosphere circulate. For mathematical purposes it is necessary to introduce certain assumptions : we may take the soil-grains as spheres all of the same radius and packed together in a symmetrical manner. We must, in fact, consider an " ideal," rather than a natural, soil. Under these conditions the dimensions of the capillary tubes can be expressed in terms of the common radius of the spheres, and calculations may be made of, for instance, the rate at which water will percolate through a given depth of such a soil. King, in America, has used this conception of the ideal soil for the purpose of defining various actual soils. The percolation of a fluid through tubes containing soil was measured, and compared with the percolation through tubes of sand, all the sand-grains in any one tube being approximately of the same diameter. The diameter of the sand particles in a tube which gave the same percolation as a

tube of soil could then be used as a specification of the soil, so that different soils could be classified simply by comparing the numerical values of the particle radii of the equivalent "ideal" soils. This treatment had the obvious advantage that a single figure could be regarded as specifying a natural soil, but it also possessed the disadvantage that the sand-grains constituting the "ideal" soil with which the comparison was made were not spherical, nor were they all of the same diameter.

Green and Ampt, in Australia, were fortunate in finding a material which did not suffer from these defects. This is the so-called "Glistening dew" of the Christmas-card artist, which is composed of almost perfectly spherical glass grains of diameter ·25 mm. upwards, and is an excellent material for an "ideal" soil. Green and Ampt measured the permeability (i.e. the volume of fluid passing per second through a soil column of 1 sq. cm. cross sectional area, and 1 cm. in length, under 1 cm. head of pressure) of the "ideal" soil both for air and water, and made similar determinations for actual soils. Now, it can be shown that the actual permeability (P) varies inversely as the viscosity (μ) of the fluid used, provided there is no action of the fluid on the material; so that for air (a) and water (ω) the value of the ratio $\frac{\mu_a P_a}{\mu_\omega P_\omega}$ should be unity. With the glass bead "ideal" soils, this relation was obeyed, but for the actual soils the value of the ratio ranged from 2 in a sandy soil up to 14 for a clay soil. This is due to the imbibition of water by the colloidal material, which increases in volume and thus decreases the size of the capillary spaces. Low values are therefore obtained for P_ω, which are reflected in the high value of the ratio $\frac{\mu_a P_a}{\mu_\omega P_\omega}$. These experiments of Green and Ampt emphasise the important difference between the constitution of actual soil and a mass of sand-grains.

We have mentioned above that the tilth of a clay soil may be damaged by cultivation when wet. The structure of the compound particles is broken down, so that the individual grains are forced closer together and the whole soil becomes sticky and plastic; if it subsequently dries the soil contracts considerably into hard coherent masses, separated by cracks, which may be several inches wide and of considerable depth. The plasticity of wet soil and the cohesion of dry soil, therefore, are properties which must be studied in their relation to the formation of compound particles.

There is great scope in this direction for further work, especially in view of the relations of these phenomena to the various methods of cultivation used in modern farming. For convenience of discussion, consideration of cultivation imple-

ments is reserved for the end of this article, while plasticity is dealt with immediately below.

The plastic properties of clay are of great importance in ceramics as well as in agriculture, and the possible causes of the phenomenon have given rise to much speculation. They have been summarised by Hancock under the following four headings.

(*a*) *Chemically Combined Water.*—This is removed by heating to 500° C., and clay so treated is no longer plastic when worked up with water. But against the view that the combined water determines the plasticity is the fact that clays with the highest percentage of such water are not necessarily the most plastic (e.g. China Clay).

(*b*) *Size and Shape of the Grains.*—Over fifty years ago it was suggested that a plate-like or laminated structure would permit adjacent grains to slide over each other, and that this would allow a mass of clay to be deformed without rupture. Some confirmation was obtained by the plastic behaviour of normally non-plastic, but scaly substances, such as gypsum, talc, and calcite, when finely ground and mixed with water.

An alternative supposition was that the grains were approximately spherical, and thus could slide over each other without the mass cracking. But some clays, deficient in plasticity, are known to contain a large percentage of ovoid particles.

(*c*) *Molecular Forces.*—Le Chatelier and others have elaborated a theory of molecular and capillary attraction, consideration of which is outside the scope of this article. For our present purpose it is sufficient to bear in mind that any theory of plasticity is eventually susceptible to mathematical analysis and exposition.

(*d*) *Colloids.*—The idea that the colloidal material in the soil is the cause of its plastic properties is the most promising. If we assume a gel skeleton throughout the mass, produced by the imbibition of moisture by the colloidal material when the soil is worked up with water, the soil would be plastic, and, on drying, the gel-structure would contract and harden, so that the soil would then exhibit increased cohesive properties.

In the present stage of development of soil physics there is little more that can be said on the subject of plasticity, because the investigations have so far been of an empirical nature which cannot readily be summarised.

(2) The Soil Moisture

The amount of moisture present in the soil at any time is the balance of gains over losses. Moisture may be obtained from the atmosphere in the form of rain and mist and from the

underground water-table by capillary attraction. The soil loses water by downward percolation into the water-table, by evaporation from the surface, by transpiration through the leaves of vegetation of the moisture taken up by plant roots, and by the running off along the soil surface of part of a heavy rainfall. The actual amount of water held by the soil at any time depends, of course, on the type of soil—a clay soil holding more than a sandy soil. Typical measurements of moisture content for a heavy clay soil and a light sandy soil showed that the former varied from 35 per cent. to 13 per cent., and the latter from 14 per cent. to 1 per cent., according to season. Soils of a loamy nature have intermediate values. The presence of organic matter in the form of farmyard manure also increases the water-holding capacity of the soil, and this additional amount of moisture may be of vital importance in seasons of drought. This is well illustrated by two plots growing mangolds on the Rothamsted Experimental Farm. Both receive ample plant food—one plot in the form of farmyard manure, the other in the form of artificial fertilisers. In droughts the plants on the former plot continue to grow owing to the extra moisture in the soil, whereas the plants on the latter are seriously affected. Actual measurements will show that there is still some water in the soil of the latter plot, and the conclusion is that the amount is insufficient, or unavailable for the plant roots. From the beginning of agricultural science the question has been discussed of how much soil moisture was available for the plant, and it was well recognised that the texture and structure of the soil were concerned in this matter.

Early investigators treated the subject from the point of view of water-films spread over sand-grains, and divided soil moisture into three broad divisions. The best exposition of these views was given eventually by Briggs in 1897. He distinguished between: gravitational water—that which is in excess of the amount normally held by the soil, and can drain away under gravitational forces; capillary water—that which is retained by the soil under natural conditions and is capable of movement through capillary action; hygroscopic moisture—the thin film in intimate contact with the surface of the particles, and not capable of movement under capillary and gravitational forces. Movement of the capillary water would be controlled by the surface tension forces in the curved film stretched over and around the soil-grains. A system of this type is easily treated by the well-established physical laws dealing with the surface tension of thin films, and Briggs showed how the movement of water from places of high moisture content to regions of lower content could take place under these forces, whenever the moisture content was reduced at one

point by the absorbing action of a plant root. This capillary moisture was regarded as available for the plant roots, whereas the hygroscopic moisture was not, because of the great attraction exerted on it by the soil particles. In the early days of soil physics Davy suggested that the amount of hygroscopic moisture taken up by a dry soil when exposed to an atmosphere of saturated water vapour was a measure of the fertility of that soil. Schübler showed, however, that this was not necessarily so, since an infertile clay would absorb more water vapour than some soils of great fertility. Actually it is a fair measure of the relative fineness of texture of different soils, because the deposition of vapour is essentially a surface phenomenon, and, since a clay soil presents a greater total particle surface than an equal weight of sandy soil, we shall normally get more moisture deposited on the former. A standardised method for measuring this quantity was worked out in America by Hilgard, and is still used extensively in the physical specification of soils, under the name of the Hygroscopic Coefficient.

German investigators have paid considerable attention to hygroscopicity measurements, and we may notice here the use made of it by Mitscherlich in studying compound particles. The water vapour is supposed—with good reason—to condense on the *total* particle surface, which includes the interstices within the soil aggregates, whereas an organic liquid of high molecular weight condenses only on the *outer* surfaces of the soil crumbs. It will be seen that comparison of these two condensations for different soils allows an estimate of the relative degree of aggregation to be made.

After these physical divisions of soil moisture were defined, attention was directed towards their relations with the moisture absorption by plant roots; it was found that the original divisions were too broad, and further subdivisions and equilibrium points were defined. This work was done almost exclusively by American investigators—Briggs and his fellow-workers. We have only space to notice two of the equilibrium values—the Wilting Coefficient and the Moisture Equivalent. The former is defined as the amount of water left in the soil when permanent wilting occurs of a plant growing therein. The earlier work of Briggs showed that under his experimental conditions the coefficient was the same for all plants in a given soil, but varied for different soils, which indicated that the soil factors were the dominant ones. Later work—of Caldwell, Alway, and others—shows that the plant factors also come in. The Moisture Equivalent is more of a physical equilibrium point. Saturated soil is subjected to a centrifugal force 1,000 to 3,000 times the force of gravity, which removes the water in the coarser interspaces of the soil. The percentage of water not

removed is determined, and defined as the Moisture Equivalent. Briggs considers that this value gives a quick and reliable basis for quantitative comparisons of soils superior to the slower and more cumbersome methods, such as mechanical analysis, and certainly of more value than measurements such as the water-retaining capacity, which expresses the amount of water retained by a previously saturated soil column of given dimensions, after thorough draining under the force of gravity.

Concurrently with the development of these various coefficients of soil moisture, the feeling arose that the relations of the soil to its moisture content could not be completely expressed by dividing up the soil moisture into various divisions and by the establishment of certain equilibrium coefficients. Endeavours were made to link up in some way the various soil constants, and a number of cross-relations between the variables were experimentally worked out. Briggs gave a series of ratios by which the wilting coefficient of a soil could be calculated from any one of the following constants: moisture equivalent, hygroscopic coefficient, water-holding capacity, mechanical analysis. Alway has given a further series of relations in a number of interesting papers, and has also paid especial attention to the bearing of these constants on the speed at which water moves from place to place in the soil. His experiments were done with care, but the data obtained only permitted of broad generalisations.

It appears, from a critical survey of the literature, that the physical significance of these various equilibrium points and divisions of soil moisture has been stressed too much. Where plants are growing in soil under natural conditions, physiological and physical factors tend towards a kind of balance; but, so far as the physical relations between the soil and its water content are concerned, it is necessary to regard the system soil-soil moisture, as a whole, and to picture the physical forces as acting in a continuous manner over a wide range of moisture content. The experiments of the writer on the evaporation of water from a thin layer of soil under controlled laboratory conditions show that the course of the evaporation can be expressed by an equation over a wide range of moisture content, ranging, in the case of an average loamy soil, from 25 per cent. down to 2 per cent.—a range which includes all the equilibrium points described above. The experimental curves are smooth, and show no sign of any abrupt change of direction in passing through the values of the soil constants discussed.

Furthermore, the organic material in the soil is not mainly responsible for the relationship, because a considerable portion of it can be removed without affecting the characteristic

shape of the experimental curves. But ignition of the soil, which destroys the colloidal properties of the clay, does affect the evaporation relationship; the curve given by remoistened ignited soil is identical with that given by sands, and the inference is that the colloidal portion of the clay fraction in the soil is predominantly concerned in the moisture relationships. It will readily be seen that the conception of a gel-like coating over the particle surface would be necessary under these conditions. Space does not permit of further elaboration of these views: those who wish to follow up the matter will find a discussion by the writer in the *Journal of Agricultural Science*, vol. x (1920), p. 44.

(3) The Soil Temperature

Temperature readings taken in the top layers of soil show that, normally, it experiences a daily rise and fall of temperature, due to the heat received from the sun in the day-time, and radiation from the soil surface into space during the night. This daily temperature wave is propagated downwards into the soil; the temperature fluctuations due to it decrease rapidly with increasing depth, becoming inappreciable at the 3-foot depth. This diminution of temperature amplitude is accompanied by a "time-lag": it takes from 4 to 6 hours before a temperature change at the soil surface becomes apparent at a depth of 6 inches.

The general problems of heat conduction when the heat supply varies in a periodic manner have been worked out by Kelvin, Forbes, and Ångström. The agricultural significance of these fluctuations in the soil has not yet been fully investigated. Ignoring their effect on biological conditions, and confining attention to the physical problems, the important factors are: (*a*) the soil material is not continuous, but consists of irregular particles touching one another at one or two points; (*b*) there is a film containing water surrounding these particles. Patten, in an extensive laboratory investigation on heat conduction in soils, has shown that, although soil material in the continuous, or rock condition, has a higher conductivity than water, the conducting power of actual soil is improved and not decreased by the presence of water, up to a certain point. This is ascribed to the improved thermal contact produced between the particles owing to the presence of the water film. An increase in moisture content beyond the critical point results in a decrease of heat conductivity, which slowly falls towards the value for water.

Temperature changes have another important effect on the soil moisture. Its viscosity and surface tension decrease

with increasing temperature so that, other things being equal, the rate of percolation should increase with temperature. This effect has been demonstrated in the laboratory by Bouyoucos, and in the field by Franklin. The former also showed that, beyond a certain temperature, the rate of percolation reached a maximum and then decreased for soils, while sand showed a continuous increase. He attributes this difference to the presence in the soil of colloidal material, which swells with increase of temperature, thus restricting the diameter of the capillary channels, and leading eventually to a decreased percolation. Possibly the change in structure of the particles which would be brought about mechanically by their expansion in volume when heated would tend to result in closer packing of the grains, and thus decrease the percolation. But this effect was not shown by the sand particles where it might have been expected; hence the decreased percolation in soils was ascribed to the swelling of the colloidal material.

Under natural conditions it is probable that the daily fluctuations in temperature induce local oscillations in moisture content in the manner described above. The effect will be to bring to the root-hairs moisture from areas in their immediate neighbourhood, and thus provide a continuous renewal of the soluble substances taken up by the plant.

(4) The Soil Atmosphere

Analyses of the gas present in the soil show that it has a slightly greater CO_2 content than the atmosphere and correspondingly less oxygen. The average volume of CO_2 in the soil air is ·25 per cent., and in the atmosphere ·03 per cent., and the former shows greater fluctuations than the latter. This comparatively slight difference in composition refers to the "free" soil atmosphere which is withdrawn from the soil by means of a delivery tube inserted to a depth of about 6 inches. In addition to this free gas there is another atmosphere, dissolved in the moisture and colloidal material of the soil, which can be slowly removed by placing the soil in flasks kept evacuated. This dissolved atmosphere contains CO_2 and nitrogen almost exclusively, and practically no oxygen. It is possible, therefore, for anaerobic and aerobic conditions to exist side by side —a fact which is of great importance in connection with biochemical changes in the soil.

The soil atmosphere is capable of inter-diffusion with the ordinary air, and this process is assisted by the alternate expansion and contraction of the soil gases due to the diurnal temperature wave, as well as by the mechanical displacement effect produced by the percolation of rain into the soil. In

soils which are waterlogged, and in bad tilth, the process of gaseous interchange is much reduced, and conditions arise favourable to the retention of the CO_2. In addition, the amount of oxygen which enters the soil from the ordinary atmosphere is necessarily less; hence the composition of soil gases from waterlogged soils shows a progressive diminution in the amount of oxygen and a corresponding increase in the CO_2 content. The characteristic open structure associated with good tilth promotes a free exchange between the soil gases and the ordinary atmosphere, and certain investigators have been led to the conclusion that this is the essential feature in healthy plant growth. Howard and his collaborators, working mainly on the indigo plant in India, have attributed extreme importance to effective soil aeration. Some of their conclusions have been questioned by Davis, who considers that the diseases of the indigo plant are due to lack of phosphatic manures, and not to defective soil aeration. Soil aeration is undoubtedly one of the many constituent factors involved in good tilth, but its degree of importance is not yet settled.

Cultivation Implements

Cultivation implements are used to lighten the labour of preparing a good seed-bed and to maintain the soil in the best condition for the plant at various stages of its development. From the farming point of view the most important is the plough. This instrument is of great antiquity. Rock carvings of a plough and draught oxen have been found in Sweden and the Alpes Maritimes, dating from 1500–900 B.C., and mural drawings of the light plough used in Egypt during the same period may be seen in the excavated tombs. The tapestries and illuminated manuscripts of the Middle Ages show some of the essential features of the modern implement. They all portray a rudimentary and cumbersome form of mould-board, the purpose of which is to invert the furrow-slice. Apparently the earliest attempt, at any rate in England, to express in a definite way the proper shape of the mould-board was made in 1808 by Amos in a Communication to the Board of Agriculture. He treated the mould-board primarily as a system of two wedges: the first horizontal, to pass under the furrow-slice and raise it slightly after it had been cut by the plough-share; the second vertical, to turn the slice over on its side and partially to invert it. Actually these two wedges merged insensibly into one another, and, although Amos's treatment of the problem by the use of solid geometry was somewhat cumbrous, the work deserves to be regarded as a courageous attempt to reduce the traditional design of the mould-board to a definite system. In 1880 the

subject was treated in a more elegant form by W. R. Bousfield, who showed that the fundamental form was a twisted surface, generated by straight lines, and giving the furrow-slice a uniform rotation proportional to the distance traversed along the length of the mould-board. The best practical British types differ but little from this fundamental form. Their purpose is to invert the slice without breaking it, so that it may stand exposed to the action of the weather over the winter months. In other countries a form of plough in general use is provided with a mould-board in the form of a curved scoop, the action of which is to break and crumble the slice. This form of cultivation is generally used where it is necessary to get the land rapidly into a suitable seed-bed—as, for instance, in parts of America, where the land is frozen throughout the winter, and impossible to cultivate until thawed out in the early spring.

Varying soil and climatic conditions, together with advances in metallurgy and engineering, have led to a great multiplication in the types of plough on the market, and in the present state of our knowledge it is impossible to say whether many of these patterns are unnecessary or whether soil cultivation is such a complex problem that each soil type demands a distinctive form of implement.

The economic changes due to the war and the advent of the internal combustion engine are greatly altering modern farming practice. Mechanical traction is rapidly displacing horse traction, and the new conditions are bringing in their wake a whole host of new problems, especially in the domain of soil cultivation. Many fresh patterns of ploughs have been devised suitable for use with tractors, but the basis of the improvement is empirical. At present there is no means of estimating, even approximately, the value of a particular implement except by prolonged practical trials, and even then there is no certainty that the implement will be equally effective on all types, or even limited types, of soils.

The practice of soil cultivation is, in fact, in a period of transition ; it should be possible to guide its progress by studying the factors involved both from the physical and engineering aspects. The directions in which this would profitably be done have been set out in the recent *Report on Agricultural Machinery* by a Departmental Committee of the Ministry of Agriculture (Cmd. 506, 1920, Stationery Office, 1*s*. net). This report emphasises the fact that progress in research as regards tillage implements must largely depend upon the results of investigations into soil physics, and especially the problems of tilth.

At first it will be necessary to proceed by analytical methods, in order to disentangle the many factors involved. For instance,

should a dry September follow the harvest period, ploughing immediately after harvest—a much more feasible operation with the tractor plough than the slower horse-drawn implement—is more easily done and more readily breaks up the moist soil; whereas later ploughing, when the land has dried out further, owing to the removal of the protecting vegetation, brings about less crumbling of the soil and often produces hard clods needing considerable weathering before they crumble into a good tilth. The factors which enter into the two cases must first of all be separated and measured before it is possible to synthesise them, and to say that certain factors, operating in a given manner, will cause certain results. Briefly stated, the study of soil cultivation must proceed first from results to causes, and then from causes to results.

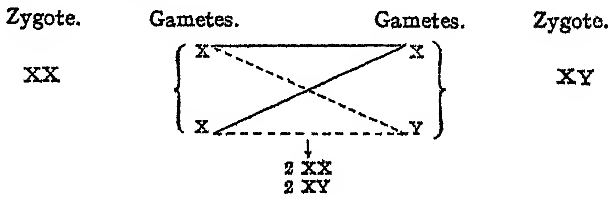
Whatever form of plough is used, it has an effect on the soil besides the mere mechanical inversion, or breaking, of the furrow-slice. In some complicated and, at present, little understood way it brings about conditions conducive to the formation of compound particles, or it may produce the opposite effect, when used incorrectly. The farmer has a traditional knowledge of ploughing problems, and has developed soil cultivation into an art. That is, in fact, its present position—an art, but not a science. There can be no doubt that the development of soil physics now taking place under the impetus of our increasing knowledge of the colloidal state of matter will lead not only to a scientific understanding of the fundamental phenomena in soil cultivation, but will open up many possibilities of improvement.

SEX HEREDITY

With Special Reference to Abnormal Numerical Inequality between the Sexes

By ALAN S. PARKES

Since the days when it was first suspected that sex was determined by gametic differentiation, considerable progress has been made in the study of the peculiarities attached to sex phenomena. Any theory of sex determination had to account for the fact that on the whole the sexes are produced in almost equal numbers ; and, bearing this in mind, once the principle of gametic differentiation was established, three possibilities arose. Either the gametes in both male and female were of two types ; or the gametes of the male were of two kinds and those of the female consistent ; or else the gametes of the female might be of the two types, and those of the male all the same. In other words, both male and female might be heterozygous for the sex factor, or one sex might be heterozygous and the other homozygous. In the first case it would be necessary to assume that gametes of the same constitution could not unite ; otherwise three classes of zygote would be possible. This being so, it would mean that only one class of zygote would be produced, and so take the problem of sex no further. By far the commonest way of producing two types of zygote in equal numbers is to have one parent heterozygous and the other homozygous for the characteristic in question ; and there now seems no doubt that the characteristic of sex is but another example of this mode of differentiation. If the sex constitution of the supposed two kinds of gametes be represented by X and Y (as it conventionally is), the constitution of one sex is XX and of the other XY. The gametes produced by the first will be X and X, and by the second X and Y. On union, the two combinations are automatically, so to speak, formed in equal numbers, and these two combinations correspond respectively to the original constitution of the zygotes.



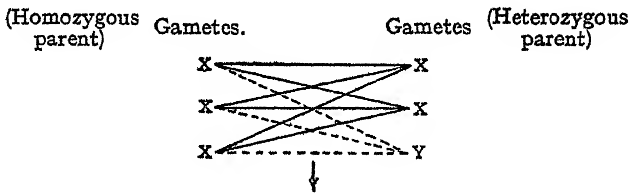
Unions producing males are indicated by dotted lines.

Once this theory had been substantiated, investigations on the subject proceeded along two distinct and definite lines. Firstly, breeding experiments were instituted, and pedigrees of families showing sex-limited characteristics were examined with the object of tracing the inheritance of such points. Secondly, cytological research was brought to bear on the subject, and a physical sex factor was looked for as an elaboration of the chromosome theory. This was, indeed, found as a supernumerary pair of chromosomes; or, at least, a pair in one sex, and one—or one and a vestigial one—in the other sex. The cytological aspect has been very fully worked out in *Drosophila*—the American fruit-fly—and it has been conclusively shown that, with regard to this species, the male is heterozygous, while the female is homozygous. For obvious reasons, cytological research has not yet been pushed to any length in the higher animals, including man, and any evidence we have on these has chiefly come from breeding experiments and reconstructed pedigrees. The higher animals are, however, very awkward subjects for breeding experiments, and little has been done with anything larger than rabbits. The higher the animal the more awkward it is, because of the fewness of the offspring and the long periods of immaturity and gestation. With man, the research worker is reduced to family records and published genealogies, which are not very plentiful, especially when abnormalities are under consideration. In spite, however, of these obstacles, enough has been done to place man and the higher animals with *Drosophila* in possessing heterozygous males and homozygous females.

Breeding experiments with butterflies and moths, particularly with the currant moth, and with domestic birds point to directly opposite conclusions; that is to say, that the male is homozygous and the female heterozygous. For the present, therefore, we have no choice but to assume that at least two types of sex determination obtain—the one exhibited by *Aves* and *Lepidoptera*, and the other by *Drosophila* and *Mammalia*.

The evidence which places man among the latter is, as mentioned above, chiefly obtained from pedigrees showing sex-limited characteristics. Among the more well known of these may be mentioned hæmophilia and colour-blindness. A somewhat diffuse characteristic, which does not seem to have received much attention, is the numerical preponderance of one sex over another in a family. The writer, having occasion to want information on the subject in an agricultural connection, was unable to find any details with regard to this phenomenon, and this paper is the result of an effort to gain a slight amount of information on the subject.

Among agriculturists, to whom the proportion of the sexes is of importance, there has long been a definite feeling that some strains of animals tend to produce males, while others have an equal propensity to produce females. This was the first point which the writer set out to clear up. We may say here that the evidence is in agreement with agricultural opinion, and that there are families in which this occurs. It is a fact, and there is no doubt that it has to be reconciled with the chromosome theory. This is not easy, because, whether the male or the female be considered heterozygous for the sex factor, it would be natural to expect that the sexes would be produced in almost equal numbers in all cases, owing to the X gamete having an equal chance of being fertilised by another X gamete, or by a Y gamete, producing respectively a female or a male. If, however, in some cases the heterozygous parent managed by some chance to bring to maturity more of one type of gamete than of the other, one sex or the other would preponderate in the offspring. Thus, if twice as many X as Y gametes were produced by the heterozygous parent, every gamete of the homozygous parent would have a double chance of being fertilised by an X gamete as compared with the chance of meeting a Y gamete. Thus, on the average, two females would appear in the progeny for every male. This may be demonstrated :



2 females : 1 male.

Unions producing males are shown by dotted lines.

If, on the other hand, the gametic ratio was reversed, males would be in excess in the offspring.

If three X gametes were produced by the heterozygous parent to every Y, the resulting progeny would be in the ratio of three females to every male. Conversely, if three Y gametes were matured to every X gamete, the ratio would be reversed. Of course, in practice no such overwhelming excess of one sex is met with, but, for the sake of clarity, whole numbers are convenient for showing the kind of thing that might account for the fact that in occasional cases a definite preponderance exists.

From the very nature of their origin, the two types of gametes must be produced in equal numbers, and the question

is whether or not this equality can be destroyed before maturity is reached. It is a debatable point, but the fact that strains occur which bear an excess of one sex or the other has to be accounted for, and this seems to be the simple explanation. It is just possible that, for some obscure reason, the gametes of the homozygous parent might in cases exercise a selective power over the other gametes trying to fertilise them, but evidence against this will be put forward later. There is other evidence in favour of the theory of numerical inequality between the mature X and Y gametes. Sometimes in experimental breeding with Mendelian characteristics the most unexpected sequences of anomalous ratios appear, and this can only be attributed to abnormal gametic fluctuation. That the relative numbers of the two types of gametes can be affected before reaching maturity by a variety of factors is also pointed to by other facts. The numbers of the sexes at birth are never quite equal anywhere. There is nearly always some small inequality. In some parts of the globe females preponderate, though in most places males are in excess. This shows that environment has a certain effect on the gametes. Race also appears to be a factor in certain cases, because there is the very singular fact, which, as far as the writer is aware, has never been explained, that among the Jews, no matter in what part of the world they may happen to be resident, there invariably occurs a much greater normal excess of male births over female than is the case with Christians. One other curious thing may be mentioned in this connection, and that is that, during the recent war, the ratio of male to female births throughout Europe rose in a steady and persistent manner, which seemed to eliminate the possibility of coincidence and chance; and one can only assume that the war was in some obscure way beneficial to the welfare of the Y gametes. In conjunction with this is the very remarkable fact that the fluctuation in the number of male births per 1,000 female during the last century follows almost exactly the rise and fall in the economic price of food. Now, it is impossible for sex to be altered by nutrition after conception, so one has to conclude that higher economic prices, and, consequently, more hardships, are capable of affecting the gametic ratios of the heterozygous sex, and of altering it in favour of the gametes with the male potentialities.

It, therefore, appears a just assumption that external conditions can influence the proportions of the sexes; and, therefore, there seems to be no reason why distinct male and female-bearing strains should not occur and be persistent so long as the exciting cause, be it organic and hereditary or causal and due to environment, is present.

Given the undoubted existence of strains showing abnormal

proportions of the sexes, the second question arises: Is the tendency vested in the male or in the female, or is it lodged indiscriminately in both? In agricultural circles there is a strong belief that this tendency is vested in the female. This would mean, however, that the female is the heterozygous parent, because, so far as can be seen, the homozygous parent can have no determining effect on the sex of the offspring, owing to all the gametes being constitutionally the same. But farm animals, along with man and Drosophila, are believed to be heterozygous for sex in the male; so that once again there is a distinct breach between practice and science. It was partly to secure information on the agricultural aspect of the subject, and partly to ascertain whether or not hereditary numerical inequality between the sexes fitted in with the view that in man the female is homozygous, that the following figures relating to human genealogies were collected.

At the outset it was proposed to work with animal pedigrees, but it soon became self-evident that the very close inter-breeding which is the vogue with our pedigree animals interweaves the strains beyond hope of distinguishing the potentialities of one family from those of another. When, for instance, a bull was mated to his own mother, who in turn sprang from a cross between sister and brother, it is more than difficult to say whether any preponderance of one sex in the offspring of the bull came through the male or the female line. In the human genealogies dealt with below very few cases of marrying back into some other branch of the same family occurred, so that this factor of complexity may be considered as being negligible. Also, it is fair to assume that, on the whole, the various members of the families married normal people; people, that is to say, whose tendencies were towards equal numbers of the sexes.

A word may now be said with regard to the selection of the genealogies. To be of any use, these had to present several well-defined features. The chief of these are enumerated below:

(1) The family had to be large enough to eliminate the element of chance as far as possible. Purely arbitrary limits had to be fixed upon as regards this, and, just as a matter of convenience, no family containing less than 150 members was considered. Naturally, the larger the family, the better.

(2) From the genealogies which could be found answering to the above requirement, all those which showed a distinct preponderance of male over female were selected. It appeared easier to work with this characteristic than the reverse one. There must be families with a preponderance of female over male—no doubt every reader is familiar with one or more examples of this happening through generations—but for a variety of reasons such families do not seem to have been

recorded as frequently or as fully as families showing the other type of abnormal proportion. This may be because such families rarely attained the eminence which would lead to records being left, or because they were only left as "pedigrees" which make no pretence of showing more than the chief members of the line.

In point of fact, out of the large number of genealogies examined, only one showed a preponderance of females, while six showed a preponderance of males in excess of the normal.

(3) A family, to be of any use, had to show the descendants of a number of the female members, so that comparisons could be made with the ratios of the whole family. This sounds a trivial point, but actually it is of great importance, because, while genealogies show all the traceable members of a family, pedigrees, as such, only show the direct line of descent of a person, or persons, and only incidentally is it possible to get further records of any large proportion of people figuring in a pedigree. The great majority of family tables available are in this latter form, including, of course, all the "Visitations," which at present constitute almost our only source of family history. It should be understood, however, that only genealogies have been used for the purposes of this investigation, and the figures are, therefore, as full and as accurate as it is possible to get them.

(4) With a view to avoiding inaccuracy, as far as possible, parts of genealogies extending back into the Middle Ages have not been included, and, as a merely arbitrary date, no records of before A.D. 1600 have been used.

A word must be said with regard to the method of dealing with the figures obtained, and of drawing conclusions from the totals. It has not been possible to pursue the ordinary methods in use; that is to say, it has not been possible to compare the actual number of male-bearing sons found descending from male-bearing fathers with what there would have been if there was no inheritance of the characteristic. This is the normal method of the determining whether a characteristic is inheritable or not, but it is not applicable in this case for the following reasons:

(1) It was hardly possible to decide definitely from the offspring of a single marriage whether or not an abnormal tendency was present, because the fewness of the offspring exaggerated the element of chance to an impossible extent. Thus, if a man had four sons and three daughters, it would be very rash to call him male-bearing; yet if the whole family contained 400 males and 300 females, it would be a perfectly just assumption.

(2) It was impossible to calculate what proportion of people

in the country had male-bearing tendencies, and, therefore, what possibility there was of a male-bearing man having male-bearing offspring if there was no inheritance.

For these reasons, the total numbers in the families had to be used as the basis of comparison. With large numbers, it was relatively easy to draw some line to separate normal from male-bearing families; and, also, it was easy to fix the normal proportions of the sexes. Naturally, "at birth" figures had to be used, and these were obtained from the reports of the Registrar-General. In 1838 the proportion was 1,045 males to 1,000 females; in 1914 it was 1,035 to 1,000 females; and the average for the whole period was 1,040 males to 1,000 females. From this average figure the "normal" excess of male over female births was calculated, and to simplify matters it was taken as a working figure that the proportion of males in a normal family would be $\frac{51}{100}$ $\left(\text{actually } \frac{50 \cdot 980}{100}\right)$. As a dividing line had to be fixed somewhere, it was decided that all families found showing a higher proportion of males to females than 1,100 : 1,000 through many generations should be considered male-bearing and abnormal, and, therefore, suitable for the purposes of this investigation.

Many dozens of genealogies were examined, and out of these six were abnormal in having more than 1,100 males to 1,000 females.

The first of these contained 285 persons. Of these, 163 were male and 122 female; this was clearly male-bearing, as the normal numbers would have been 145·35 male to 139·65 female. But the really interesting point appeared when the female lines were separated from the male. The former showed 68 males and 67 females—for all practical purposes a normal ratio (actual normal being 68·85 males and 66·15 females). In the male line, however, the numbers were 95 and 55 respectively, as against the normal ratios of 76·50 and 73·50. This shows definitely that in this family the whole male-bearing potentialities of the strain were vested in the male line. Assuming, as it is fair to assume, that on the whole all the members of the family married normal people, it is indicated that the characteristic of begetting abnormal proportions of the sexes is an attribute of the male.

Full analysis of Family 1:

—	Total.	Totals.		Male Line.		Female Line.	
		M.	F.	M.	F.	M.	F
Found . .	285	163	122	95	55	68	67
Normal . .		145·35	139·65	76·50	73·50	68·85	66·15

This fits in well with the theory that in *Mammalia* the male is heterozygous for the sex-determiner and the female homozygous.

In view of the very instructive results obtained from the first family, the analysis of the second male-bearing strain available was awaited with some interest. The result, however, was disappointing, because only the descendants of one female member could be traced, and thus the element of chance was so considerable that no reliable deduction could be made. The family is only included for the sake of completeness.

Full analysis of Family 2 :

—	Total.	Totals.		Male Line.		Female Line.	
		M.	F.	M.	F.	M.	F.
Found . .	186	112	74	98	67	14	7
Normal . .		94·86	91·14	84·15	80·85	10·71	10·29

The third family also showed too small a female line for justifiable deduction to be made, and again is only included so that all the six male-bearing strains found may be presented.

Full analysis of Family 3 :

—	Total.	Totals.		Male Line.		Female Line.	
		M.	F.	M.	F.	M.	F.
Found . .	161	88	73	67	57	21	16
Normal . .		82·11	78·89	63·24	60·76	18·87	18·13

Family 4, however, fulfilled all the requirements, and had a satisfactory female line. It contained 177 members, and of these 96 were males and 81 females ; thus having well over the normal excess of males. The female line contained 41 males and 47 females, and so approximated to the normal far nearer than did the male line, which contained 55 males and 34 females.

Full analysis of Family 4 :

—	Total.	Totals.		Male Line.		Female Line.	
		M.	F.	M.	F.	M.	F.
Found . .	177	96	81	55	34	41	47
Normal . .		90·27	86·73	45·39	43·61	44·88	43·12

This family, therefore, well supports the figures obtained from Family 1.

Families 5 and 6, the last two suitable families found, show

exactly the same sort of thing, and they can, therefore, be considered together.

Full analysis of Families 5 and 6 :

—	Total.	Totals.		Male Line.		Female Line.	
		M.	F.	M.	F.	M.	F.
Found, 5 .	783	415	368	272	223	143	145
,, 6 .	200	127	73	98	46	29	27
Normal, 5 .		399·33	383·67	252·45	242·55	146·88	141·12
,, 6 .		102	98	73·44	70·56	28·56	27·44

With the exception, therefore, of Families 2 and 3, which an uncertain female line rendered untrustworthy, all the families point to the same conclusion, as do the grand totals :

No. of Family.	No. in Family.	Males.	Females.	Male Line.		Female Line.	
				Males.	Females.	Males.	Females.
1	285	163	122	95	55	68	67
2	186	112	74	98	67	14	7
3	161	88	73	67	57	21	16
4	177	96	81	55	34	41	47
5	783	415	368	272	223	143	145
6	200	127	73	98	46	29	27
TOTALS:							
Found .	1,792	1,001	791	685	482	316	309
Normal .		913·92	878·08	595·17	571·83	318·75	306·25

The percentage deviations from the normal are very interesting :

SUMMARY OF TOTALS, WITH DEVIATION FROM NORMAL

—	Totals.		Male Line.		Female Line.	
	Male.	Female.	Male.	Female.	Male.	Female.
Found	1,001	791	685	482	316	309
Normal	913·92	878·08	595·17	571·83	318·75	306·25
Per cent. Deviation from Normal . . .	+ 9·53	− 9·92	+ 15·10	− 15·71	− 0·86	+ 0·90

From this table the following comparative figures can be calculated. The percentage excess of the per cent. deviation of males found from males normal in the male line over the per cent. deviation of males found from males normal in the female line = 1,651·0; while this figure for the females is 1,650·0.

The percentage *excess* of per cent. deviation of males found from males normal in male line over per cent. deviation ot males found to males normal in whole family = 58·46. On the other hand, the percentage *deficiency* in per cent. deviation of males found from males normal in female line under per cent. deviation of males found from males normal in whole line = 90·99. These figures for the females, on the other hand, would be respectively an excess of 58·38 per cent., and a deficiency of 90·95 per cent.

These figures supply direct evidence in support of the supposition that in man the male is heterozygous for sex, while the female is homozygous; and they prove the corollary that the female has no determining influence on the sex of the offspring. Also, they show that females, come they of ever so strong a male-bearing strain, will, assuming the probability that on the whole they marry normal people, have a normal equality of the sexes among their offspring. In addition, the families analysed above favour the proposition that preponderance of males is due to abnormal excess of Y over X sperms produced by the male, and not to any selective power on the part of the ova.

This supposition is also supported by other evidence, indirect, but still interesting to note. As mentioned above, during the war the one-time normal excess of male over female births rose throughout England in a manner which could only be attributed to the war. Now the war influenced the female population but little compared with the fundamental alienation experienced by the vast majority of the men; and it is hard not to connect the very appreciable alteration in the proportions of the sexes at birth with the uprooting of the normal life of the men rather than with the relatively small difference with which the women had to contend.

Finally, a last word may be said with regard to the accuracy of the genealogies used. While every possible care was taken to ensure that those presented above were as complete as could be reasonably expected, it is a fair argument to say that females tend to get omitted from such compilations more than the males. True; but, if this had happened to any large extent in the tables under review, it would have made the families, as they stand male-bearing, more or less normal. Now, there is no reason to suppose that, if females had been left out, they would have been left out more in one part of the family than in another. If, therefore, a number of females had been omitted, the present equality in the female lines would be disturbed, greatly in favour of the females, though the family on the whole was normal, and the females on the average married normal people. Clearly, it is hardly legitimate to suppose that this could be uniformly so.

We think, therefore, that the genealogies may be regarded as reliable, and, granted this, there is no choice but to believe that strains showing abnormal proportions of the sexes do occur, and that the characteristic of begetting a preponderance of one sex over another is an attribute of the male.

APPENDIX

The following are the references of the families mentioned in the paper :

Family No. 1. *A Memoir of John Kay.* Privately printed.
,, ,, 2. The Family of Rogers. From the "Visitations."
,, ,, 3. The Family of Fielden. From the "Visitations."
,, ,, 4. *The Hawtrey Family.* From the book.
,, ,, 5. *The Genealogy of Fox.* Privately printed.

COOKING AND VITAMINES

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1. *Introductory.*—Cooking is an art of great antiquity. In the not distant future it may well become also a science. The recognised objects of cooking are to improve the digestibility and palatability without at the same time impairing the food value of the substance. To this may also be added that cooking often destroys bacteria and other organisms, and thus lessens the danger of harmful effects in this direction.

Until recent years it has been the custom to estimate the food value of an article chiefly by its chemical nature (whether protein, carbohydrate, or fatty), and also by the digestibility. Thus, bacon fat, or lard, is thought to be of great value because as much as 90 per cent. of the intake can be absorbed. It is now known, however, that other considerations are involved, one of the most important being the presence or absence of what have been called accessory food factors, or vitamines. At present, the biological value of a food can be determined only by the assistance of experiments on subjects kept on controlled diets. Such experiments appear to be essential to the understanding of food and dietary problems.

Vitamines are substances which are present in nearly every natural foodstuff, and they are necessary, in smaller or greater amount, to the growth of the young animal as well as to the maintenance of health in the adult. The absolute amounts required by man are not determined, but probably do not differ much from those of the monkey, which, in turn, have been compared with those of the guinea-pig,[1] so that experimental results with either of these animals can be used with more or less accuracy in the case of man.

Three accessory food factors, or vitamines, are at present well known. These are widely distributed in natural foods, and a sufficiently varied diet of food, which is not overcooked, is unlikely to be deficient in any of them. Two are especially concerned with growth, and have been called respectively fat-soluble A and water-soluble B. In the entire absence of either of these normal growth ceases. Certain diseases are definitely associated with a deficiency in the diet of these vitamines, especially rickets in the case of the fat-soluble, and

beriberi in the case of the water-soluble vitamine. A third vitamine, sometimes called water-soluble C, is also important for maintenance of health. A prolonged deficiency (14–16 months for man) in this vitamine leads to the development of scurvy : water-soluble C is therefore better known as the anti-scurvy or anti-scorbutic vitamine. It must be remembered, however, that a deficiency in any of these vitamines in the diet may lead to a depression of the vitality and a lowered resistance to infection for some time before any definite symptoms of scurvy appear. In young animals it has been shown that a diet deficient in either fat-sol. A or in the anti-scurvy vitamine, or both, results in the development of defective teeth.[8]

2. *Properties of Vitamines and their Distribution among Foodstuffs.*—These vitamines have not yet been isolated, but their chemical and physical properties have been investigated, and the latter throw some light on what may be expected when food containing them is stored or subjected to heat. The following table shows a comparison of the three accessory food factors with regard to stability under different conditions.

TABLE I

SHOWING THE PRINCIPAL PROPERTIES OF THE THREE ACCESSORY FOOD FACTORS

—	Anti-beriberi, or Water-soluble Factor.	Anti-rachitic, or Fat-soluble Factor.	Anti-scorbutic Factor.
Solubility in water	Very soluble.	Soluble.	Soluble.
Stability (*a*) to heat . .	Stable at temperatures up to and including 100° C. Destroyed rapidly at 120° C. in neutral or acid solution.	Destroyed slowly at 100° C. Destroyed more rapidly at 120° C.	Green vegetables lose their anti-scorbutic properties at temperatures of 60° C. and upwards. Juice of raw swedes loses about half its anti-scurvy value after heating for an hour at 100° C. Orange and other acid fruit juices are nearly stable at 100° C., but not at 130° C.
(*b*) In presence of acids.			Acid fruit juices are relatively heat stable.
Hot dilute .	Rapidly destroyed.	Not ascertained.	Addition of acid to water in cooking cabbage increases the loss of vitamine.
Cold dilute .	Stable.	,, ,,	
(*c*) In presence of alkalis.			
Hot dilute .	Rapidly destroyed.	,, ,,	Rapidly destroyed, both in vegetable and fruit juices.
Cold dilute .	Stable.	,, ,,	
(*d*) Storage.			
Cold storage .	Stable.	,, ,,	Probably stable.
At ordinary temperatures	,,	,, ,, Probably deteriorates during storage.	Unstable.

It will be seen at a glance that the anti-beriberi factor is so stable as not to be affected by storage or by ordinary cooking operations. The principal sources of this vitamine are to be found in the seeds of plants and in the eggs of animals. In the case of cereals, the germ and husk of the grain are richest in the vitamine, the white endosperm from which white flour is made containing very much less. From this point of view, brown bread affords a good source of this vitamine.

The anti-rachitic factor, on the other hand, may be completely destroyed by prolonged boiling, as in the case of milk, or stews, or by the commercial methods of canning meats and vegetables. The principal sources of the fat-soluble factor are animal fats, fish-oils, and green vegetables, as may be seen from Table II. Of the animal fats, butter-fat and beef-fat are the most important, pork fat being singularly deficient in this respect. Fish-oils appear to be very rich, but vegetable oils generally poor or lacking in this growth-promoting vitamine. Margarines, made chiefly from vegetable oils, are therefore more suitable for adults than for children, who need the fat-soluble vitamine most during their growing years. When margarine is, however, inevitable, green food and carrots should be given more freely, and, if necessary, cod liver oil.

Green vegetables, such as cabbages, are rich in the fat-soluble vitamine, but the white parts, or "heart," of green cabbages are almost completely lacking in it. According to Steenbock,[3] yellow sweet potatoes and other yellow foods, such as carrots, and yellow varieties of Indian corn have also the fat-soluble vitamine, but not white varieties of similar nature. Comparatively little is known as to the stability of this vitamine when stored.

The anti-scorbutic vitamine is, on the other hand, easily destroyed by heat and deteriorates under most forms of storage. It is necessary to consider this in relation to each of the principal sources of this vitamine—fresh vegetables, fresh fruits, and sprouted seeds.

3. *Influence of Heat on the Anti-scorbutic Properties of Vegetables and Fruits.*—Carefully controlled experiments make it clear that there is a progressive and serious destruction of the anti-scorbutic principle in fresh cabbage during cooking.[4] Young guinea-pigs have been kept successfully free from scurvy on a daily ration of 1 gm. raw fresh green cabbage, the basal additional diet being nutritious but purely scorbutic. If the cabbage ration is still further reduced symptoms of scurvy make an early appearance.

When, however, the cabbage leaves are cooked before being offered to the animals a much larger ration is necessary to give the same result. After simmering for an hour in hot water

TABLE II

Table showing the Distribution of the Anti-rachitic and Anti-scurvy Vitamines

Class of Foodstuff.	Foods.	Vitamine Content (approximate).
Anti-rachitic.		
Fats and oils . .	Butter, cod-liver oil.	Very rich.
	Cream, beef-fat or suet, whale, and other fish-oils.	Rich.
	Pea-nut (*Arachis*) oil.	Poor.
	Lard, bacon-fat, olive-oil, and hardened animal fats.	Deficient
Meat, fish, etc. .	Liver, kidney, heart, fat fish (herring, salmon, etc.)	Rich.
	Lean meat, brain, sweetbread, and roe.	Poor.
Milk. . . .	Whole cow's milk, unheated.	Rich.
	Dried whole cow's milk.	Less rich, especially after keeping.
	Boiled milk.	Undetermined. Presumably less rich according to time of heating.
Eggs. . . .	Fresh or dried.	Rich.
Cereals . . .	Wheat germ, linseed.	Rich.
	Whole grain pulses.	Poor.
Vegetables and fruit	Cabbage, fresh germ, green leaves	Rich.
	,, whitish "hearts" leaves.	Deficient.
	,, dried.	Poor.
	,, canned.	Deficient.
	Lettuce, spinach (fresh or dried) carrots.	Rich.
	Potatoes, bananas, and nuts.	Poor.
	Dried carrots, beetroot, juice of swedes.	Deficient.
Anti-scorbutic.		
Meat, fish, etc. .	Lean meat, liver.	Poor.
	Tinned meats.	Deficient.
Milk	Cow's, raw, whole or skim.	Poor.
	Dried or condensed.	Deficient.
Cereals, pulses .	Whole grain, pulses, and flour from these.	Deficient.
	Germinated peas, beans, lentils, or grain.	Rich.
Vegetables and fruit.	Swede (raw expressed juice), cabbage (fresh), orange-juice (fresh), lemon-juice (fresh), and tomato-juice (fresh raw).	Very rich.
	Beans (fresh, scarlet runners), lemon-juice (preserved), raspberries, canned tomatoes.	Rich.
	Lime-juice (fresh), grapes, apples, dried tamarind, dried mango, carrots (raw), potatoes, canned tomatoes (*purée*).	Poor.
	Cabbage (dried or canned, beetroot, and preserved lime-juice.	Deficient.

(60° C.), or after steaming for 20 minutes at 90° C.,* 5 gm. is the necessary minimum, and, after steaming or boiling 20 minutes at 100° C., 5 gm. is not sufficient to ensure health, mild scurvy appearing in the former and more severe scurvy in the latter case. In all these cases the value of 5 gm. of fresh cabbage has been reduced by cooking to that of only 1 gm., *i.e.* there has been a loss of about four-fifths of the original value (about 80 per cent.).

Similar results were found in the case of runner beans [6] and germinating lentils,[6] as shown in Table III, and also in vegetable, but not to the same extent in the case of fruit juices (Table IV). The contrast between the behaviour of raw cabbage-juice and orange-juice when heated is particularly striking, since both these juices have a similarly powerful anti-scorbutic effect when fresh and raw. From Table III it may be seen that a loss of anti-scurvy value also follows when runner beans are heated, as in canning, and it is to be expected that a similar loss of anti-scorbutic value would occur when other vegetables or fruits are heated.

TABLE III

SHOWING EFFECT OF HEAT ON THE ANTI-SCORBUTIC VALUE OF CERTAIN VEGETABLES

Substance.	Treatment.	Time. Min.	Temp. °C.	Limiting protective ration.	Per cent. destruction.	Author.
				Grammes.		
Green cabbage	Raw	—	—	1	—	Delf, 1918.
	Simmered	60	60	5	80	do.
	Steamed	20	100	>5	80	do.
	,,	20	90	5	>80	do.
	,,	120	130	<15	—	do.
	Canned (2 weeks old)	60	90–100	>7.5	>70	Campbell and Chick, 1919.
Runner beans .	Raw	—	—	5	—	do. do.
	Canned (3 months old)	140	100	>20	>75	do. do.
Germinating lentils . .	Raw	—	—	5	—	Chick and Delf, 1919.
	Boiled	15	100	12	—	do. do.

4. *Methods of cooking Vegetables.*—When the effects of heating cabbage at different temperatures are compared, it will be seen that the *time* has more significance than the temperature used. For instance, simmering cabbage for 1 hour at 60° C. has about the same destructive effect on its anti-scurvy value as steaming at 100° C. for 20 minutes, but

* A temperature of 90° C. is obtained at or near sea level when a steam chamber fits loosely over a vessel containing fast-boiling water; or when the lid of a tight-fitting steamer is tilted, the water below being kept boiling.

in the former case the leaves remain tough and leathery, whereas in the latter they are quite softened and fit for eating. Slow methods of cooking are thus unsuitable for obtaining the best results with vegetables.

There are three methods in common use for cooking green vegetables: (1) by throwing into excess of fast boiling water; (2) by steaming over boiling water*; and (3) by boiling in a very small amount of water, so that no straining is necessary before serving, the vegetables being mainly cooked in their "own juices." The last is the so-called "conservative method." In order to preserve the green colour of cabbage and similar vegetables it is a common practice to add a little bicarbonate of soda to the cooking water: this also considerably hastens the softening of the tissues.

The relative merits of these methods must now be considered in relation to our knowledge of the properties of vitamines.

In experiments made by Harden and Zilva[7] with fresh orange-juice, it was found that, after being made just alkaline, the juice quickly lost its anti-scorbutic efficacy. In the case of vegetables, therefore, there is every reason to believe that this destruction would be still more rapid when sodium bicarbonate is added to the cooking water, which is kept at or near boiling-point.

According to Masters and Garbutt[8] soda or sodium bicarbonate is commonly added to cooking water for two reasons), viz. to "soften" the water and to preserve the bright green colour of the vegetables. These authors point out that nothing is gained by "softening" the water, since green vegetables discolour as readily when cooked in soft as in hard water. The discoloration appears to be due to the formation of some acid substance during cooking; this may be prevented by the addition of a volatile alkaline salt such as ammonium carbonate, in small quantity. According to these authors, addition of about ½ gm. ammonium carbonate to a litre of water would reduce the time needed to steam cabbage from an hour (steamed over plain water) to 18 minutes (steamed over water to which the carbonate has been added). In the case of the conservative method, addition of $\frac{1}{10}$ gm. carbonate reduced the time of cooking from 1 hour to 12 minutes, and left "no appreciable smell of ammonia." In the light of the experiments of Harden and Zilva, already quoted, however, it seems

* In reckoning time for steaming, allowance must be made for loss of heat involved in opening the steamer and introducing a cold substance. In the experiments quoted on p. 605 small amounts were introduced and the thermometer registered the desired temperature within two or three minutes of replacing the lid. In domestic operations this is often not possible, and the time for cooking must be reckoned from the time when the steam chamber is again full of steam at or near 100° C.

very doubtful whether the vegetables would retain any anti-scorbutic value after this treatment.

According to experiments of Delf [4] (1918), addition of small amounts of citric acid to the cabbage-leaves before cooking, or to the cooking water in the case of sprouted whole lentils, only increased the destruction of anti-scurvy vitamine involved in the cooking. This is an unexpected result, since acid fruit juices such as lemon and orange are better able to withstand the effect of heat than are the juices of vegetables (*cp.* Table IV).

TABLE IV

EFFECT OF HEAT ON EXPRESSED FRUIT AND VEGETABLE JUICES [9]

Juice.	Treatment.	Time. Min.	Temp. °C.	Minimum Protective Ration Daily. c.c.	Per cent. destruction.
Cabbage, filtered	Raw	—	—	<1.5	
	Heated	60	100	>5	<70
Swede . .	Raw	—	—	2.5	
	Heated	60	100	<5	<50
		60	130	>5 <10	>50 <75
Tomato . .	Raw	—	—	1.5	
	Bottled (commercial brand)	—	—	>5 <10	>70
	Purée, canned	100	80–100	5 (= 10 before concentration)	85
Orange . .	Raw			1.5	
	Heated	60	100	1.5	Not indicated.
	Heated after nearly neutralising	60	100	1.5	,, ,,
	Heated	60	130	>2 <3	>15 <50

In the present state of our knowledge, therefore, it is safe to say that green vegetables should be cooked in an open vessel until just tender, in fast boiling water, or steamed over water without addition of either acid or alkali to the water. By the former method a better colour is obtained than by the latter, but there is more loss owing to exudations from the cabbage into the water. Salt may be added if desired, for this does not appear to cause increased destruction of the vitamines.

The experiments of Givens and Cohen [11] suggest that the anti-scorbutic value of root vegetables, such as carrots, may vary with age, the young carrots being more powerfully anti-scorbutic than the older, and the freshly gathered older carrots superior to those which have been stored. Young

carrots require also much less time for cooking than old ones, and in this way would suffer less deterioration than the old. It will be seen from Table II that vegetables such as carrots and potatoes are not highly anti-scorbutic in value; nevertheless, since they are easily obtained, and in almost constant daily use, they may form a valuable part of the diet as a source of anti-scorbutic, and, in the case of the carrot, also of the fat-soluble vitamine.

5. *Methods of cooking Pulses.*—In cooking pulses (peas, beans, and lentils), it is often difficult to ensure the softening of the seeds without a long time of boiling. The addition of bicarbonate of soda to the water in which they are soaked previous to boiling is a common practice with peas and beans in this country, and, no doubt, makes them more digestible. An alternative method, however, is to germinate the seeds.

Pulses contain no appreciable amount of anti-scorbutic vitamine when dry, but are valuable for their starch and protein content as well as for their considerable anti-neuritic value. It has been proved by experiment that in germination other vitamines develop, especially the anti-scorbutic vitamine.[6] At the same time the hard substance of the seed softens. Germinated lentils are well cooked if boiled for 10–15 minutes, germinated peas in about 20 minutes, and germinated beans in not less than 45 minutes, in the absence of any added soda. In order to produce this effect it is only necessary to allow the seeds to soak for about 12 hours in water (*e.g.* over night), rinse them, and keep in a damp place for another 2 days, with free access to air. This can be accomplished by placing them in a perforated tin box or in a clean flower-pot, covering them with a damp cloth, which should be occasionally moistened with water in dry weather. In two days a short radicle will appear (in the case of peas and lentils), and the seeds are then ready for use.

6. *Methods of cooking Fruit.*—In cooking fruit there is little danger of destroying the anti-scorbutic value, especially in the case of those containing acid juices. When it is desired to reduce the acidity before cooking, however, as in the case of rhubarb, or young gooseberries, it is important to take care that a perceptibly acid reaction remains, in order that the anti-scorbutic value of the juice may be retained. (*Cp.* Table IV.)

7. *Methods of preserving Fruits and Vegetables.*—Our knowledge of the properties of vitamines has also an important bearing on the question of preserving foods. The three principal methods are drying, canning, bottling, and the making of preserves or jams.

With regard to the first method, except in dry climates, where the sun can be used for rapid open-air drying, the labour

and cost of fuel make this process a relatively expensive one. Dried vegetables have been shown over and over again to be deficient in anti-scurvy value; they are without any appreciable protective power against scurvy, and, although they may doubtless be used as flavouring, they *should never be relied upon as the main source of anti-scorbutic in a diet.*[12] On drying, by the most favourable methods, a loss of about 90 per cent. of the original anti-scorbutic value occurs: after about six months no anti-scorbutic value whatever can be detected. When dried vegetables are stored at ordinary temperatures a gradual further deterioration of their slight anti-scorbutic value therefore takes place. The dried products are best stored in a cool, dry place in a stoppered jar or closed tin, so that no atmospheric moisture may be absorbed.

With regard to canning, direct evidence from the results of experiments by Campbell and Chick[6] show that a large amount of destruction of anti-scorbutic value is inevitable in the methods commonly employed, and recommended to the public for canning vegetables in Pamphlet No. 34 of the Food Production Department. Controlled experiments showed that by this method cabbage would be exposed in canning for at least an hour and a half to temperatures varying from 90° to 100° C.: the corresponding loss of anti-scurvy value was over 70 per cent* (*cp.* Table III). The growth-promoting vitamine (fat-soluble A) had, moreover, been lost, part of it, at least, being found in the liquor surrounding the leaves.

In the case of runner beans, the canned product must be twice heated in order to ensure sterilisation, thus involving a total period of heating to 100° C. for 2 hours 20 minutes. The loss of anti-scurvy value involved amounted to about 90 per cent. of that of the fresh green bean-pods. These authors conclude that *the value of canned vegetables, as regards scurvy and growth-promoting properties, must be regarded as negligible.*

With regard to fruits, it is probable that canning and bottling the more acid fruits does not involve so great a loss as in the case of vegetables; for, in the first place, the fruit is not kept at 100° C. for more than 20 minutes (even in the case of "hard" fruits), and, in the second place, the juice of oranges, lemons, and raspberries is known to be heat stable. The juice of tomatoes is somewhat less stable (see Table IV). A brand of bottled tomatoes tested at the Lister Institute† in 1919 had lost about 70 per cent of its original value (*i.e.* as compared

* The loss of anti-scurvy value is less than when cabbage is cooked for a corresponding time in open vessels, but most of this heating is done after sealing up the tins.

† Unpublished experiments by Rhodes and Delf, 1919.

with fresh raw tomato-juice).* Tomato *purée* prepared by simmering tomatoes in their own juice at 80–90° C., straining the thickened juice and sealing in cans, had lost still more. In this case, the simmering took about 1½ hours, and the canning involved further heating at 100° C. for 5–10 minutes; about half the original amount of water had been driven off: 5 c.c. of this juice (equivalent to 10 c.c. fresh) had only the same anti-scorbutic effect as 1–5 c.c. of the raw juice. The original juice had thus been reduced in anti-scorbutic value by 85 per cent.

It has been found impossible as yet to test the vitamine contents of jam, for the experimental animals cannot tolerate the amount of sugar that would be necessary in giving jam rations. It is probable, however, that the anti-scorbutic value—if any—to be found in preserves depends upon the time taken in boiling the fruit, and also on the nature and ripeness of the fruit. There is little evidence on the latter point, but it seems probable that over-ripe fruit has less anti-scorbutic value than unripe or barely ripe fruit: and it is a well-known maxim that only sound fruit should be used for both bottling and preserving.

8. *General Considerations.*—Methods such as the above are especially necessary at times when fresh food is for any reason difficult to obtain, but even at other times much harm may be done by the common method of providing stews which are kept hot for indefinite periods of time. Numerous occasions arise when this is the most convenient form of providing the principal meal of the day, as in catering for camps, hospitals, and prisons, and other institutions. It must be remembered that adverse results may follow a diet poor in vitamines, such as general debility, weakness, and apathy, with low resistance to infective diseases, when no sign of any specific disease can be found. Such a condition has, in fact, been reported in certain students' hostels in China.[13]

There are, however, also cases of outbreak of scurvy on record which have been traced to continual over-cooking of an otherwise adequate vegetable and meat ration. Two are quoted in the *Report of the Medical Research Committee*,[14] pp. 65, 66. Scurvy broke out in a camp in Scotland in the spring of 1917, and eighty-two men were affected. At the time potatoes were scarce, but the ration contained a fair proportion of fresh meat and 2 oz. of fresh swedes were available daily. These are amongst the most potent anti-scorbutic vegetables we possess, and, if cooked satisfactorily, should have afforded considerable protection daily. The cause of the outbreak was investigated

* The loss was due to the combined effects of bottling and storing, but we had no means of estimating the age of the bottle. The *purée* was prepared, canned, and tested at once.

by Prof. L. Hill, who discovered that the meat was always served as a stew, the vegetables were added, and the whole cooked for about 5 hours. This circumstance was considered by Prof. Hill as the explanation of the outbreak.

A second example is afforded by an outbreak of scurvy which broke out in a labour battalion in France between May and July 1918, and in which 142 cases of pronounced scurvy were diagnosed. In this case there was a ration of fresh vegetables equal to 8 oz. daily; these were cooked with the meat and boiled for a period of at least 3 hours. In the opinion of the medical officer by whom the circumstances of the outbreak were thoroughly investigated this fact was an important contributory cause.

More recently, an outbreak of severe scurvy was reported in the University Kinderklinik, Vienna. At the time of the outbreak there was no corresponding occurrence of scurvy in the general population. Dr. Harriette Chick, writing of this outbreak, says: "In the present case the allowance of vegetables up to two months before the first case appeared was more than 200 gm. daily, which is not less than is usual in many private households during the winter, and may be expected to prove adequate for prevention (*i.e.* of scurvy). A similar ration, 250 gm. daily fresh vegetables, is a common ration for soldiers on active service, and instances must be numerous in which this amount has failed temporarily without appearance of scurvy in so short a period as eight weeks. In the present case, therefore, we are forced to conclude that, while the supply of vegetables was adequate, the supply of anti-scorbutic was defective." On further inquiry it was found that, as is usual in Vienna, the vegetables were all twice cooked before serving, first being boiled until soft, and then fried with flour and fat before serving. In the soups, the vegetables were also cooked for at least three or four hours. In normal times, the additional milk, fresh meat, and fruit consumed must counteract the deficiency from this destructive method of cooking; but, when these sources fail, as in the case quoted above, for a considerable time, outbreaks of disease may be expected.

It is, therefore, a safe general rule that, so far as its vitamine content is concerned, the less food is cooked the better. The heating involved in cooking should be not more than sufficient to sterilise and soften the food; as a rule, the natural flavours will be the more apparent by this means. Over-cooking and "keeping hot" should be avoided, especially in the case of fruit and vegetables.

In times of scarcity of butter, milk or animal fats, salads, and lightly cooked green vegetables, and also carrots, should be increased in the diet, together with fat fish (herring, mackerel,

or salmon), or, in the case of children, cod-liver oil. The stewing, which is frequently necessarily prolonged in the case of tough meats, may be prolonged to the tough outer green leaves of cabbages and cauliflowers, since these contain the valuable fat-soluble vitamine which is less readily destroyed by heat than is the anti-scorbutic vitamine.

In times of shortage of fruit and vegetables, pulses and even cereals should be germinated or sprouted before use. All vegetables used in stews should either be cooked first and added just before serving, or should be finely chopped, thrown in the stew at the last moment, and served after 5–10 minutes' boiling. When vegetables are scarce, but fruit of well-known anti-scorbutic value is available, fruit will replace the anti-scurvy, but not the growth-promoting, value of the vegetables. A ration of about half a pound of vegetables daily has often been regarded as an ample ration for an average man doing full work.* Experience justifies this assumption, on the whole, excepting in cases of persistent over-cooking such as those quoted above. On analogy with the protective ration of fruit-juice necessary to prevent scurvy in a monkey, and allowing for the difference in the body weight, the fresh juice of a medium-sized orange or lemon, or a fairly large fresh tomato daily should prove an ample anti-scorbutic ration for the average adult.

* There seems little doubt that a diet richer in vitamines is required during periods of active work than during periods of rest. In South Africa natives who live on a diet poor in vitamines seldom develop scurvy in their own homes; when brought to the mining centres and given a better diet than that to which they are accustomed, scurvy often supervenes after only two or three weeks of the unaccustomed and hard labour of the mines.

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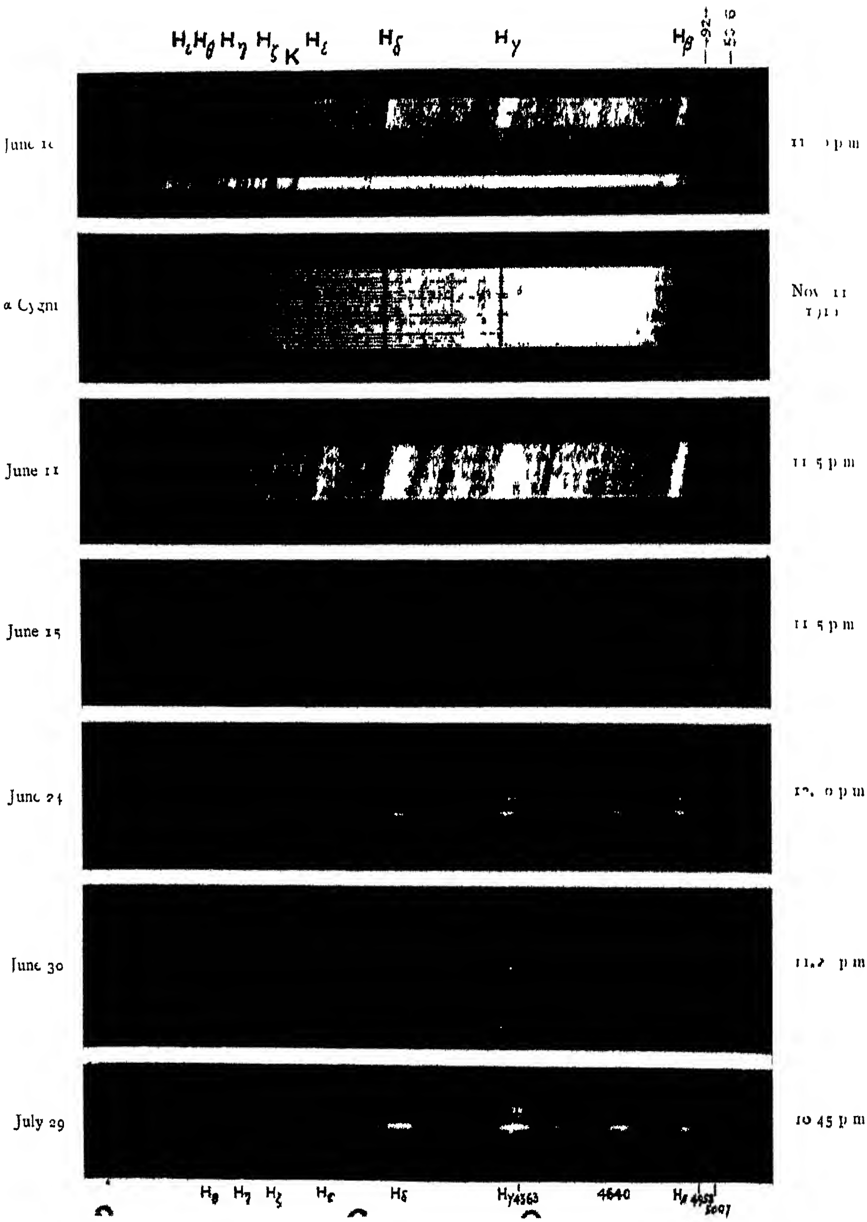
PROGRESSIVE SPECTRA OF NOVA AQUILÆ, 1918.



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POPULAR SCIENCE

NEW STARS

By the Rev. A. L. CORTIE, S.J., F.R.A.S., F.Inst.P.

One of the most wonderful of the phenomena which can be observed in the starry firmament is the sudden blaze into luminosity of a new or temporary member of the starry hosts, which have, so far as visual observations are concerned, shed their light upon mankind, unaltered in relative position or in number, for untold centuries. It may happen that the new star suddenly appears in a position in the sky which was unoccupied by any star before. Or if a star existed, it had not been previously detected, owing to its excessive faintness, by any observation, visual or photographic, anterior to its sudden increase of lustre. Nova Aurigæ, for instance, discovered by the Rev. Dr. Thomas W. Anderson, of Edinburgh, in the last days of January 1892, was absent from a plate of the region of the sky in which it shone forth, taken by Dr. Max Wolf, of Heidelberg, on December 8 of the preceding year, on which stars were photographed as faint as the eleventh magnitude.[1] Two days later it was of magnitude 5 on plates taken at Harvard College Observatory by Prof. E. C. Pickering. This means that the star had increased its luminosity two hundred and fifty fold in two days. In the case of Nova Persei, also discovered by Dr. Anderson, a plate of the region of the sky in which it shone, taken by Mr. Stanley Williams on February 20, 1901, failed to record the new star, though stars as faint as the twelfth magnitude were shown. Two days later it was a magnitude brighter than a first magnitude star, which indicates an enormous increase in light, fully sixty thousand fold.

Nova Cygni (1920)[2] also was presumably, if it existed at all as a luminous body, below the sixteenth magnitude prior to

[1] The lowest star magnitude visible to the naked eye is the sixth. The relation of brightness between one magnitude and the next in order is $\sqrt[5]{100}$, nearly 2·5.

[2] A plate of the field taken at Heidelberg, June 3, 1905, which shows stars of magnitude 16, has no trace of the nova. The *Observatory*, No. 557, p. 367. October 1920.

August 16, when its magnitude, on a photograph of the field in which it appeared, taken in Sweden, was reckoned as the seventh. When discovered by Mr. Denning on August 20 it had risen to magnitude 3·7, and it attained its maximum, 1·8 magnitude, on August 24. The difference between the magnitudes 16 and 2 would indicate an increase of luminosity about four hundred thousand fold.

On the other hand, a new star may be one which was known as existing previously, but which suddenly received an extraordinary access of luminous energy. Such was Nova Aquilæ (1918). It had been recorded as a star of the eleventh magnitude on the Harvard College photographs in 1888, and again at Algiers in 1909. Its light was known to be slightly variable to the extent of about half a magnitude. On June 3, 1918, it was a star of the eleventh magnitude. On June 7 it had risen to the sixth magnitude, a hundredfold increase in light. Discovered on June 8, it rivalled Sirius in brilliancy in the course of the next twenty-four hours, so that in less than six days its luminosity had increased more than twenty-five thousand fold.

The rate of discovery of new stars during the first eighteen centuries of the Christian era was on the average one for each century. Of these the most famous was that which appeared in the constellation Cassiopeia, in November 1572, and which was assiduously observed by the famous Danish astronomer, Tycho Brahe. He was then twenty-six years of age, and he has left to posterity a full account of his observations in his work entitled *De Nova Stella.* When the opening year of the twentieth century was marked by the appearance of Nova Persei, which year, too, coincided with the tercentenary of the death of the distinguished astronomer, the Royal Society of Copenhagen issued in photogravure a reproduction of Tycho's book, of which at that time only three copies were extant.

Of this celebrated nova we learn that it rivalled the planet Venus in brilliancy, and indeed was visible in broad daylight at noontide. It was not seen after the month of March 1574, but in the meantime its colour had changed from white to red, and then again to white, indicative, no doubt, of its changing spectrum, on which we shall dwell more at length in what follows. Besides discussing the colour changes in the star, and its varying magnitude, Tycho also, from his measures, came to the conclusion that the appearance was not one that belonged to our atmosphere, but appertained to the sphere of the fixed stars, and consequently was very distant. The book is replete, too, with astrological lore and prophecies. The appearance of the star is likened, as a wonderful portent, to the standing still of the sun at the command of Joshua, and

to the brilliant luminary that heralded the birth of Christ our Lord. The star is prominently figured in the constellation of Cassiopeia in Beyer's *Uranometria*, published in A.D. 1661.

Very wonderful, too, must have been the appearance of the Venus-like nova of A.D. 1604, witnessed and described by an equally famous astronomer, the illustrious Kepler. This star was seen in the constellation of Ophiuchus. Struck by its intense brilliancy, Kepler thereupon broached a theory that the Star of Bethlehem was to be explained by the appearance of a new star, combined with a conjunction of the brilliant planets Mars, Jupiter, and Saturn in the same region of the sky.

Yet another remarkable new star, discovered in the same century, was Anthelm's nova of A.D. 1670, which, appearing near β Cygni, was to be seen for two years, the brilliancy fluctuating in a noticeable manner. No other new star was seen for a period of one hundred and seventy-eight years, the next recorded discovery being that of a second new star in the constellation of Ophiuchus, by Hind, in the year 1848. Since that date no less than twelve novæ have shone out as visible to the naked eye, while telescopic discoveries have been greatly multiplied.

Up to July 1917 thirty-two of these objects were known, and all of them, with three exceptions, were situated in the Milky Way. One exception was the variable star T Coronæ, which in 1866 rose from the ninth to the fourth magnitude in a few hours. Though similar to a nova, it was not a true member of the type. It was the first new star submitted to spectroscopic analysis, by Sir William Huggins, and showed a composite spectrum of dark absorptions and of bright radiations. The other two exceptions to this concentration of new stars in the plane of the Milky Way were two novæ, discovered in spiral nebulæ. The more remarkable of these two was that discovered in the heart of the great nebula in Andromeda in the year A.D. 1885. It shone with a distinctly green tinge of light in a continuous spectrum. It was the first new star examined spectroscopically by the writer. Vogel's observations of the nova of 1876, in Cygnus, had demonstrated a transition of the spectrum from a double spectrum of absorptions and corresponding radiations, mainly due to hydrogen, until a phase was reached when the chief green ray of the nebular spectrum alone survived. Burnham, observing the star at the Lick Observatory in 1891, stated that it appeared to be like a small nebula. In the period from July 1917 to the end of 1919 no less than seventeen new stars had been discovered. Of these last, fifteen are situated in spiral nebulæ, and of these again eleven have been found in the central condensation of the Andromeda nebula.

The fact that new stars, which in our stellar universe are confined to its luminous backbone, the Milky Way, are also found in spiral nebulæ, among other reasons, suggests that the latter may be distant replicas of our stellar system. If so, considering that according to modern methods of celestial photography it would be possible to get a record of about three-quarters of a million of such spirals, a magnificent picture is unfolded before the imagination of the immense depths of space, emblematical of the attributes of Power and of Wisdom, of Immensity and of Omnipresence, of the Adorable Creator.

Incidentally a comparison of the luminosities of the novæ of our Milky Way, and of those of the spiral nebulæ, enables us to get some idea of the distance of the spirals. We may reasonably suppose that the two sets of novæ, the Milky Way family, and that of the spiral nebulæ attain in general the same absolute magnitude, or luminous power, when they are at their greatest brilliancy. If their apparent magnitudes, or intensities of illumination, be considered, the new stars of the Milky Way, discovered in the last quarter of a century, attained a maximum brilliancy about eight times as great as those of the spiral nebulæ. This means that they appeared to the eye to be sixteen hundred times as bright. But, by a well-known law of photometry, the apparent brightness, or intensity of illumination of two objects, is inversely as the square of their distances. From which it follows that the new stars immersed in the spiral nebulæ are forty times as far away as those peopling the clouds of our own Milky Way. The spiral nebulæ containing them are therefore at least 120,000 light-years away, and may be as distant as 800,000 light-years. A light-year is the distance travelled in a year by a light beam, the velocity of which is 186,000 miles a second.

To return to the enumeration of the new stars recently discovered. A systematic search on the plates of the Harvard College Observatory has revealed six others, two of which, Nova Lyræ and Nova Ophiuchi, the third recorded in this constellation, appeared in the year 1919. For 1920 we have to record the naked-eye new star, Nova Cygni, also number three in the constellation, discovered by that veteran and indefatigable observer of meteors, Mr. Denning, on August 20, when it had a magnitude of 3·7. It rose to 1·8 on August 24, since when it has steadily declined in apparent luminosity, until at the beginning of November it had fallen to about the tenth magnitude.

A light-curve is a graph which connects dates, yearly, monthly, or daily, or indeed in general intervals of time, with the apparent magnitude, or intensity of illumination, of a star that varies in brightness. If we compare the light-curves of

new stars, we find a general common likeness, consisting in a sudden great increase in light and then a gradual waning, the latter phase accompanied in some cases, notably in that of Nova Persei (1901), and to a less extent in that of Nova Aquilæ (1918), by a series of periodic fluctuations. These fluctuations of luminosity in the case of Nova Persei (1901) were accompanied by an alternating spectrum. As Father Sidgreaves showed, from the Stonyhurst spectrograms of the star, when the apparent magnitude was above 4·5, the spectrum was the bright radiation spectrum characteristic of the second stage in the progressive spectra of a new star. But when the luminosity fell below 4·5, then the nebular spectrum emerged as strengthened. Father Sidgreaves adopted an ingenious explanation to account for these fluctuations in brilliancy and concomitant changes in spectrum, first suggested by Sir William Huggins in 1866, and which is possibly correct. "The sudden expansion of a vast mass of mixed gases would naturally become oscillatory for a length of time, the temperature oscillating with it; and when the mean temperature of the oscillations was near the combining temperature of some of the mixed gases, the spectrum would also alternate with the temperature."[1] This supposes, presumably, that the expansion results in the formation of a planetary nebula. Some observers are inclined to believe that the nebula exists independently of the star, and is not formed from, but illuminated by, its outburst. But the difficulty remains that the appearance of the nebular radiation, near the sixth radiation of hydrogen (Hζ), was concomitant with each waning of the star's light below a critical magnitude. This would indicate a true expansion of the materials forming the light-giving envelope of the star.

In its initial stages a new star is visually of a brilliant white or bluish-white colour, which changes into an orange or reddish tint, and finally into a greenish hue. These colours are connected with the progressive changes which occur in the spectra, as the nova waxes and wanes in luminosity.

Several observers of repute have stated that the spectrum of a new star, when nearing its maximum brilliancy, is continuous, without any bright radiations or dark absorptions. Such, for instance, was the earliest spectrum reported in the case of Nova Persei (1901) and Nova Aquilæ (1918). The next stage in the spectral changes is the appearance of a series of dark absorptions on a strong continuous background, representative of hydrogen, iron, calcium, titanium, chromium, silicon, and other metallic vapours, and in the state, and presumably at the corresponding temperature, at which they are

[1] *Monthly Notices*, R.A.S., lxii. 148.

rendered luminous in the laboratory by an electric spark discharge. This type of spectrum was observed in the case of Nova Persei (1901), Nova Geminorum (1912), Nova Aquilæ (1918), and Nova Cygni (1920). It is most likely a characteristic phase in the spectral changes of all new stars. In the first three stars named their light was waning when the spectrum was observed. In the case of Nova Cygni (1920) this same type of spectrum was photographed by Dr. Lockyer, also when the star was approaching its maximum of luminosity.

This spectrum substantially matches that of the star α Cygni. Now the spectrum of α Cygni is very similar to that, which is not infrequently observed, in outbursts of metallic vapours and gases on the sun's surface, in the neighbourhood of sun-spots. Such outbursts are indicative of an eruptive release of immense stores of internal energy. The sudden appearance of new stars, their rapid rise to luminous maximum, and the metallic vapours involved in the early spectrum are all analogous to a cataclysmic eruption, though on a greatly enhanced scale, on the solar surface.

At this stage, too, in the progression of spectral changes, the spectrum extends far into the ultra-violet, which indicates a very high degree of temperature. In the case of Nova Aquilæ (1918), as observed at Stonyhurst, the ultra-violet spectrum embraced the whole series of hydrogen absorptions, and extended well beyond them. Here again we have a connecting link with the spectrum of the lower layers of the sun's atmosphere, which are revealed in particular at times of total solar eclipse. Probably when the new star is near the period of greatest brightness, or at any rate a very few days after the appearance of the absorption solar chromospheric spectrum, a series of bright radiation bands begin to appear. This bright band spectrum is for the most part the exact replica and counterpart of the absorption spectrum. The brightest bands are those due to hydrogen, and to iron vapour excited in the electric spark discharge, and these bright broad bands always occupy positions just less refrangible, or to the red side of the accompanying absorptions. By far the brightest of these bands is the red radiation of hydrogen. This is the cause of the visual red or orange colour of the star, and of the change of colour from white to red, observed by the early observers, Tycho Brahe and Kepler.

These bright bands, which increase in intensity as the star begins to fall in luminosity, are of great breadth, extending over many wave-lengths in the spectrum, on each side of the normal positions of the radiations. In the case of Nova Aquilæ (1918), for instance, they covered about fifty units of wave-length. The effect of pressure on an incandescent

metallic vapour is to broaden the radiations in the spectrum, as shown by the laboratory researches of Dr. Duffield, and of other investigators. The pressure required for the broadening of the bands, such as are observed in new stars, would be very great. Yet the possibility of pressure, as an agent in the widening of the bands, when we consider the scale of the catastrophic outburst which results in a new star, is not to be neglected. Enormous pressure is competent to furnish the continuous spectrum, recorded by several observers as the first stage in the spectral phases of a new star. It may therefore be a contributory cause for the widening of the bright bands.

But the chief cause would seem to be the motion of shells or clouds of gas, moving towards and away from the observer. For the breadth of the bands is a function of the wave-length which, according to the well-known principle of Doppler, means motion in the line of sight. These broad bright bands show a considerable amount of structure, especially so in those due to hydrogen. For the most part they are double bands, and in the case of Nova Aquilæ (1918) the hydrogen bands were triples. The central bright bands of the triples were practically stationary, the other two members indicated line of sight velocities of the order of 1,300 kilometres per second approach and 1,200 recession. In all new stars these bands of hydrogen remain permanently widened without change for a considerable period, in some cases for several months. This would mean a persistence of these enormous velocities of approach and recession for such periods, which presents a very great difficulty for any adequate explanation of the origin of new stars. Considering the scale of the initial catastrophe, it may not be found to be altogether unimaginable or insuperable.

On the analogy of a solar outburst we should expect that there would be absorptions by the relatively cooler clouds of hydrogen and other gases advancing towards us, and that these absorptions would show indications of velocity of approach. This expectation is realised, and in all new stars the absorption spectrum which accompanies and reproduces the radiation spectrum is, as we have noted, always on the violet side, and greatly displaced, indicating motion of approach. In Nova Aquilæ (1918) the hydrogen absorptions were doubled, and even tripled, indicative of successive outbursts of masses of this gas. The velocities of approach, corresponding to the wave-length displacements, were of the order of 1,700 kilometres per second for the first set of absorptions, and 2,400 kilometres per second for the second set, about twice and thrice respectively the velocity of the quickest moving solar prominences. The displacements are directly proportional to wave-length, and therefore indicate line of sight velocities.

Dr. Adams has found an interesting relation between the average displacements of the dark absorptions in the four stars Nova Aurigæ (1892), Nova Persei (1901), Nova Geminorum (1912), and Nova Aquilæ (1918), the four brightest discovered in recent years. " We find," he writes,[1] " the surprising result that the displacements of the lines in all those stars are directly proportional to wave-length, and divide themselves into two pairs of equal amount. Of these the first pair of stars has exactly twice the displacement of the second, and it is perhaps a significant fact that Nova Aquilæ and Nova Persei were much brighter stars apparently, and probably intrinsically as well, than Nova Geminorum and Nova Aurigæ." He also finds, in the case of Nova Aquilæ, a progressive displacement of the absorptions at successive dates, the average daily increase in displacement between June 10 and June 15 being slightly over a half unit of wave-length. Mr. Evershed had also recorded the same phenomenon for the hydrogen absorptions. This progressive increase in velocity is also observed in solar prominences, and furnishes another bond of union between the solar outbursts and the sudden appearance of new stars.[2]

The next phase in the progression of spectra in a new star is the gradual dying out of the bright bands, characteristic of an α Cygni or solar chromospheric type, and their replacement by bright bands which are represented in helium stars, such as γ Orionis. The helium is accompanied by oxygen and nitrogen. Meanwhile a bright band near wave-length 4640, a composite band, which is seen in the very early stages of the star's spectrum, grows brighter and brighter, until it becomes one of the brightest bands in the spectrum. This band has been unravelled by Dr. Lunt,[3] who shows that it contains high-temperature nitrogen. His conclusions have been corroborated by Prof. Fowler[4] from laboratory researches. Besides nitrogen, the band contains carbon, which can be obtained in a laboratory only by very heavy electrical discharges, and helium. This composite band is of great importance in new stars, for it links up all the successive phases of its radiative activity. The B type of spectrum, or spectrum like γ Orionis, gradually, in its turn, gives place to the spectrum of a planetary nebula. In this spectrum the radiations at wave-lengths 5007

[1] *Proceedings National Academy of Science,* iv. 355.

[2] In the case of Nova Cygni (1920) Dr. Lockyer has made the interesting observation that the variable velocity curve of the dark hydrogen is the counterpart of the light curve. The velocity varied from 330 kilometres per second on August 22 to a maximum of 900 on August 26, and then were gradually reduced to 460 by September 19. (*Monthly Notices,* R.A.S., lxxxi. 46.)

[3] *Monthly Notices,* R.A.S., lxxx. 534.

[4] *Ibid.,* lxxx. 692.

and 4958, the characteristic radiations of a gaseous nebula, have replaced those due to high-temperature iron at 5016 and 4924, which are very prominent in the first stages of the bright band spectrum. Another nebular radiation at wave-length 4363 also becomes quite pronounced.

Not only spectroscopically but visually the star has changed into a planetary nebula, for it shows a distinct greenish disc when observed with a telescope. But this is not the final stage, for eventually the characteristic nebular bands yield pride of place to those which are found in a class of bright band stars, called Wolf-Rayet stars from their discoverers. Nova Aurigæ, Nova Persei, Nova Geminorum, though now very faint stars, are not beyond the reach of the giant telescopes of Mount Wilson.[1] They all show a continuous spectrum marked by the bright bands of hydrogen, but lacking in the chief nebular bands, the places of which have been taken by bands found in Wolf-Rayet or Class O stars. Nova Aquilæ, in June last, was still in the planetary nebular stage, but the diameter of the green disc had increased from 0·65″, as observed by Barnard in October 1918, to 3·69″, according to the measures of Aitken. Some remarkable spectrograms of the star were secured by Moore and Shane, and by Dr. Wright at the Lick Observatory. These showed that the hydrogen was not expanding to the same dimensions as the nebular radiations, or in other words, that different portions of the disc into which the star has developed give bright radiations displaced by different amounts. Here again we have indications of an actual expansion of the gases of the nova, and not of the illumination of nebular gases rendered visible by the advance of a spherical wave of light which originated in the nucleus.

We have seen that new stars in our system are confined to the Milky Way. So, too, are the Wolf-Rayet stars, and the planetary nebulæ, with but few exceptions. There are several other characteristics which are shared by these three classes of heavenly bodies. New stars are comparatively few amongst the hosts of heaven. So, too, are the planetary nebulæ and the Wolf-Rayet stars. With regard to the number of the planetary nebulæ, Dr. H. W. Curtis states, in the truly wonderful and fascinating volume of researches on the nebulæ recently issued from the Lick Observatory,[2] " On any reasonable and probable basis of correlation between the planetary nebulæ and the stars of supposedly corresponding magnitudes, it would seem certain that the relative proportion of the planetaries to the stars must be of the order of one-thousandth of 1 per cent. or less.

[1] Adams and Pease, *Proceedings National Academy of Science*, i. 391.

[2] Publications of the Lick Observatory. Vol. xiii, " Studies of the Nebulæ," p. 72.

This minute percentage would seem to stamp the planetary at once as an exceptional case, a sporadic manifestation of a path which has been but rarely followed in stellar evolution."

Dr. Wright, too, in the section devoted to the spectra of the gaseous nebulæ, finds that about half the planetary nebulæ have bright nuclei of the Wolf-Rayet type of spectrum, and has "no hesitancy in advancing the opinion that the nebular nuclei belong to the same general division as the Class O stars, irrespective of whether they exhibit bright bands or not."[1] These nuclei of the planetary nebulæ have been found to give a continuous spectrum strong in ultra-violet light. This is generally supposed to be an indication of very high temperature. But there can be no possible doubt as to the close connection between the planetary nebulæ and the Class O stars, and, as we have seen, new stars pass in their spectral changes into planetary nebulæ and finally into Class O stars.

Another link between new stars and nebulæ is furnished by Hubble's discovery of a variable nebula, the light radiations of which are exactly matched by the bright bands in the early spectrum of Nova Aurigæ. Nor must we forget in this connection the apparently expanding nebula about Nova Persei, photographed by Ritchey at Yerkes with the two-foot reflector on September 20, 1901, and again on November 18. Four well-marked condensations in these nebulous clouds had moved one minute of arc in a month. If the distance of the nebula was that of the nearer fixed stars, this angular displacement would have indicated a velocity of about 2,800 miles per second. The more probable view is that the nebula was already there before the outburst in Nova Persei, and that the light waves were reflected from the successive nebulous clouds, as sunlight is reflected from successive ranges of mountains. There is no doubt at all as to the connection of new stars with nebulæ. The question at issue is whether the nebula represents a real expansion of the materials of the star, or whether, being already present as a dark invisible mass, it is thrown into sympathetic luminous vibrations by the tremendous outburst that takes place in its midst. Nor, again, can there be any reasonable doubt that the preliminary explosion, however caused, and whatever may be the agency that carries the tenuous gases into space, perhaps light-pressure, was a solar eruption on a greatly magnified scale.

Reverting again to the successive phases in the progressive spectra of new stars, Lieut.-Col. F. J. M. Stratton, of the Solar Physics Observatory, Cambridge, in a study of the spectra of Nova Geminorum, has shown that the appearance of these spectra was accompanied by a succession of jets or impulses,

[1] *Loc. cit.*, p. 252.

The velocities in the line of sight indicated by the displacements of the absorptions would become gradually less, when a new pulse would seemingly eject a fresh jet of hydrogen, accompanied by other gases and vapours of metals. Certainly in these valuable observations there is a suggestion of an explosive outburst of hydrogen from moving bodies, or a moving body, which carried the other elements.[1]

We have already adverted to the fact that the bright band at wave-length 4640, which is partly due to helium, partly to nitrogen, and partly to carbon, excited by an extra strong electric field, becomes more pronounced as the star wanes in brilliancy. Again the order of the various phases of the spectrum, through Classes F, A, B, P, O, occur as the star loses lustre, and presumably becomes cooler. But this order is that of giant diffuse stars ascending in temperature. This anomaly may be explained, and the behaviour of the bright band 4640 furnishes the clue, if we attribute the later spectra not to temperature changes but to a successive strengthening of the electric field. According to the experiments of Dr. G. A. Hemsalech,[2] the principal conditions for the appearance of the spark-produced radiations in metallic vapours are, first a low degree of ionisation of the medium, and secondly the existence of a strong electric field giving rise to an electric current through the medium at a high potential gradient. To make an application of these valuable experiments to the varying spectra of nova: it would appear that in the early stages of high temperature in the constituent gases, indicated by the great brilliancy of the star and its extended spectrum far into the ultra-violet, the ionisation would be very active. Hence a strong electrical field could not be established. But with a falling temperature, indicated by the star's waning in brilliancy, the electric field would become correspondingly greater. Hence the increased lustre of the band 4640, produced in the laboratory by heavy electrical currents, and the successive changes of the spectrum through the above-named classes.

Several speculations have been broached as to the origin of new stars. Prof. Bickerton has strenuously upheld a grazing impact between two suns, resulting in the formation of a third body. There is much to be said for this hypothesis. Or it may be that the approach of a luminous to a non-luminous massive body of low density would have caused the ejection of a stream of gas towards the passing star. Others, again, incline to the view, first proposed by Seeliger, that the phenomena of new stars are due to the entrance of a star into a nebula, on the analogy of the luminous appearances that accompany

[1] *Journal British Astronomical Association*, xxx. 181.

[2] *Report to the Department of Scientific and Industrial Research*, Oct. 8, 1920.

a shooting star in the earth's atmosphere. But there are plenty of nebulæ not in the Milky Way, though not so many gaseous nebulæ, and there are plenty of moving stars. Why, if the supposition is correct, are the new stars confined to the Milky Way?

We have endeavoured to state the conditions, as derived from observation, of the problem that awaits solution. Somehow, at first, an explosive outburst of a solar type is involved, and finally a planetary nebula. How? In his extensive study of Nova Geminorum II (1912), recently published,[1] Lieutenant-Colonel Stratton gives as his opinion that the best solution available at present is "the spreading out of streamers of glowing gases from a central body after a collision with a quiescent dark cloud, and the final formation of a planetary nebula with a Wolf-Rayet star as nucleus." Of prime importance is his discovery that the absorption spectrum can be analysed in terms of the stronger lines of two typical spectra, α Cygni and γ Orionis, and that the displacement of these two spectra, which varies from day to day, is widely different for the two groups of elements involved in the two typical spectra.

[1] *Annals of the Solar Physics Observatory*, Cambridge, vol. iv, part i.

NOTES

Sir William Mather

THE death of the Right Hon. Sir William Mather on September 18, 1920, inflicted a loss upon science—not exactly because Sir William Mather was himself a man of science, but because he was very sympathetic towards all scientific questions and did his utmost to promote science in this country. He was born in 1838, the son of one of two brothers whose father had founded the iron-works, now those of Mather and Platt, at Salford ; and at the age of thirty he took over the entire control of the business for a time. He was a very successful administrator ; entered Parliament in 1889 ; and in 1893 established the forty-eight hour week at his works—the result of which fully bore out his anticipations, the business of the firm being greatly improved both as regards quality and quantity of output. In addition to his business he had wide interests. He paid many visits to Russia and was a member of the Official Parliamentary Party which went there in 1921. He equipped the mechanical training workshop at the Gordon College at Khartoum and was a trustee of the college. He was also President of the Textile Institute for several years. He had a great liking for the United States, and was made honorary LL.D. by Princeton University. For a number of years he was President of the British Science Guild, and also a Governor and Member of Council of Owens College, Manchester (now the University). He was knighted in 1902 in recognition of the services he rendered to the Committee which had been entrusted with the task of reorganising the War Office after the Boer War, and in 1910 he was appointed a member of His Majesty's Privy Council. In later years he lived at Bramwell Hill in the New Forest, where many have enjoyed his hospitality and that of Lady Mather. A man of medium size, of a clear and ruddy complexion and bright blue eyes, full of interest in everything, he was looked upon with affection by large numbers of people. We heard someone ask one day, "Why is it that Sir William Mather always looks so supremely happy?" The answer was that his whole life had been an almost ideally happy one, while he had plenty of that quality which does more to ensure happiness than anything else—goodness. He was everyone's friend.

Pure Science Again

We regret to see that people are still clacking about Pure Science as if it were something quite distinct from such a mean, sordid, and inferior article as the rest of science—which we presume they will call Impure Science. The fact is that nearly all the scientific work that has ever been done in the world, and certainly almost all the really successful part of it, was carried out for the express purpose of obtaining useful information. Those who talk loftily about pure science would have us believe that it is something which is quite separate from all practical objects; and everyone has heard of a meeting of "men of science" who drank the toast of Pure Science with the acclamation, "May it never be of any use to anyone." Probably the gentlemen who drank this toast were so enthusiastic because they themselves had never done any work which was of any use to anyone; but it does not follow that those who toil for the service of their fellows would be equally pleased. When we examine the history of science we find that most of it was undertaken for purely practical purposes. Astronomy was created largely in the interests of navigation; Geometry, largely in the interests of architecture and agriculture; Chemistry, for the purpose of alchemy and then of innumerable manufactures; Physics, in the interests of machinery and invention; Geology, in the interests of prospecting for valuable metals, coal and other kinds of deposits; Botany, for the discovery of drugs; Zoology, for the light which it throws on the anatomy, physiology, pathology, and growth of the human body; and Medicine, entirely for the purpose of preventing and curing disease and maintaining the body in a perfect state of health. Undoubtedly, as all these sciences have grown, there have come moments to each when the true discoverer has wished to detach his feet from the solid earth of utility and to soar (or to sink) into more ethereal space. But it is seldom that a real man of science actually starts by drawing a distinction between pure and useful science. Indeed, he is not generally guided by any such considerations, either on the one side or on the other; but simply pushes in wherever he can see an opportunity for successful investigation, whether success is likely to be immediately useful to the world or not. No man who has ever done successful research imagines that he can do it by attacking any single one he pleases of all the immense number of problems which lie before him—only unpractical persons have such ideas. The intelligence of the successful investigator is proved by his choice of the subject to be investigated. Nature is like an infinitely huge mountain showing immense precipices directly opposed to us—precipices which none but fools would attempt to climb. But every

here and there there is some crumbling of the rocks, some landslip, some little ravine, some little rivage, some more gentle slope where the feet may hold; and there it is that the man of sense endeavours to ascend. He tries, and he may be foiled; but, if he succeeds, he makes an advance. Nature is infinite. We can rise only step by step. The people who talk about pure science think that they can jump vast distances—with the result that they generally remain where they are. We can say that the true investigator takes the most promising opportunity offered to him, irrespective of the question whether his success will lead to immediately useful results or not; but he always knows this—that, whatever new result he may obtain, it is almost certain to be a key which will open new treasures of nature for the benefit of men in general. For example, when Faraday investigated electricity, do we think that he had no vision within him as to the large practical results which might follow his work? He did not talk of these practical results at the moment because before his work was done he could not specify them; but he knew that knowledge brings power, and that power enhances prosperity. Another example is that of Darwin. He saw his opportunity in our ignorance of the reason why different species of living things exist; and he studied the matter and gave us the Theory of Evolution. True, this was a piece of pure science; but it was not a piece of useless science. It added to the dignity and the honour of human intelligence. It was therefore useful. There are many people, in this country especially, who may be called the fatuitists—fatuitists in science, art, and politics—who talk, but never achieve anything, except perhaps mischief-making. Those who have themselves made no advances in science too often belong to this school. They write most of the textbooks, sit on most of our committees, preside over many of our societies, and reap many of the rewards; but let them allow the man with the genuine passion for investigation to choose his subject for himself.

De Moivre's Theorem (Sir R. Ross)

I will be very much obliged to any of our mathematical readers who will be so kind as to inform me where I can find any record of the following proposition—which shows that De Moivre's famous theorem connected with complex numbers is only a particular case of the iteration—that is, the *operative* involution—of a real algebraic function of which one of the parameters is reduced to zero. I have known the proposition for many years, and indeed indicated it in my paper on "Operative Involution" in SCIENCE PROGRESS, No. 50, p. 288, October 1918, in some examples at the end, and in No. 51, p. 486, January 1919, last example; but I have searched in vain for it through my books—even in the works of Hamilton, Tait, and Joly on quaternions, on which subject it has an important bearing.

The expression

$$r^2 - (\cos\theta . x + \sin\theta \cdot \sqrt{r^2 - x^2})^2$$

is easily seen to be a complete square if we put $\cos^2\theta . \{r^2 - (r^2 - x^2)\}$ for $\cos^2\theta . x^2$; that is, it equals either

$$(\cos\theta . \sqrt{r^2 - x^2} - \sin\theta . x)^2 \text{ or } (\sin\theta . x - \cos\theta . \sqrt{r^2 - x^2})^2.$$

Now if $y = \cos\theta . x + \sin\theta . \sqrt{r^2 - x^2}$ and $x = \cos\phi . z + \sin\phi . \sqrt{r^2 - z^2}$, it follows that (taking the first of the two squares)

$$\begin{aligned} y &= \cos\theta . (\cos\phi . z + \sin\phi . \sqrt{r^2 - z^2}) + \sin\theta . (\cos\phi . \sqrt{r^2 - z^2} - \sin\phi . z) \\ &= \cos(\theta + \phi) . z + \sin(\theta + \phi) . \sqrt{r^2 - z^2}. \end{aligned}$$

This process can evidently be continued indefinitely, and, if all the angles $\theta, \phi, \ldots$ be the same, is a case of iteration, or successive substitutions, of the same operation. Thus, if the original function $\cos\theta . x + \sin\theta . \sqrt{r^2 - x^2}$ be iterated $(n - 1)$ times on itself the result will be

$$\cos n\theta . v + \sin n\theta . \sqrt{r^2 - v^2};$$

and this can easily be proved to hold for any real values of n. The proposition may be exhibited in a single equation by means of my "explicit operative notation," which I employed and explained in the previous paper mentioned—

$$[\cos\theta . \mathbf{o} + \sin\theta . \sqrt{r^2 - \mathbf{o}^2}]^n = \cos n\theta . \mathbf{o} + \sin n\theta .$$

(Here $\mathbf{o}$ is the "symbol of substitution," and the index n affixed outside *square* brackets denotes operative as distinct from algebraic involution.)

Now, so far as the *mechanical process* of algebra can inform us, this proposition holds true whatever value r may have—that is, the mechanical result is independent of the numerical magnitude of r, while $\mathbf{o}$ has no numerical significance at all. Hence the same result will be obtained when r is zero. In this case $\sqrt{r^2 - \mathbf{o}^2}$ becomes $\sqrt{-\mathbf{o}^2}$, which $= \mathbf{o} . \sqrt{-1}$; and our equation becomes

$$[(\cos\theta + \sqrt{-1} . \sin\theta)\mathbf{o}]^n = (\cos n\theta + \sqrt{-1} . \sin n\theta)\mathbf{o}.$$

But the iteration of any function $a . \mathbf{o}$, where a is a constant, is given by $[a\mathbf{o}]^n = a^n . \mathbf{o}$ (that is, ax iterated $(n - 1)$ times becomes $a^n x$). Hence finally,

$$[(\cos\theta + \sqrt{-1} . \sin\theta)\mathbf{o}]^n = (\cos\theta . + \sqrt{-1} . \sin\theta)^n\mathbf{o},$$

and $$(\cos\theta + \sqrt{-1} . \sin\theta)^n = \cos n\theta + \sqrt{-1} . \sin n\theta;$$

which is De Moivre's Theorem.

This proposition has so many implications, especially as regards vectors, complex numbers, and trigonometrical functions, that I shall be very grateful for any information regarding it. In fact, these functions are connected with the nth *operative roots*, or with the *operative logarithms*, of the primary algebraic operations; and the imaginary unit seems to be only the *symbol of error* which occurs when the operative mechanism is wrongly applied to numerical subjects.

Sociological Society

A meeting of the Sociological Society was held at Leplay House, 65 Belgrave Road, on Tuesday, January 25, at which Mr. H. J. Laski spoke on "The Prospects of Parliamentary Government"; Prof. Graham Wallas in the chair.

Mr. Laski said that Parliamentary Government in this country had been

subject to criticism for a long period. It might seem that serious criticism was a post-war development; on the contrary, however, there were vigorous discussions in the seventies of last century on the defects of parliamentary procedure. The difference at the present day lay in the rapid development of a widespread distrust of the whole parliamentary system, whereas in the seventies the criticism reflected a general belief in the system and desire to improve it. There was a definite decline in the influence of the House of Commons since its great period, between 1832 and 1870. The attention of the people was no longer concentrated on its proceedings, as it was forty or fifty years ago. At that time the fate of ministers was decided there: now it was decided within the purlieus of Downing Street, whose barriers few people are permitted to pass.

This change Mr. Laski attributed to various causes. Among these was the growth of the power of the Executive, many questions nowadays being argued, and even decided, privately, between ministers or civil servants, and representative traders or workers. A deeper cause was the change in the character of the questions to which Parliament had to devote attention after 1870. Economic problems had come to the front, and were now more important than political questions in the older sense. The House had not sufficient time to deal effectively with work of this sort, nor were members well equipped, as a rule, for the task. Members of Parliament were drawn from a comparatively small section of society, and the qualifications which gave them their seats were not usually the results of training and service in the local area which they represented in Parliament. Such training in local government would be of great value to members in enabling them to understand and represent effectively the interests for which they stood.

The disadvantages of the present system of Committees in the House were great. Members were chosen to sit on Committees without reference to their qualifications for the work. There was a whole range of questions, now dealt with by Committees of the House, which could be much more effectively treated outside it, as, for instance, industrial questions. These should come under the consideration of such a body as the National Industrial Council set up by the Government not long ago. It was obvious that in a Parliament elected on a nominally geographical basis, but in which occupational interests really preponderated, industrial problems would meet with a somewhat one-sided treatment, owing to the small proportion of labour and independent representatives.

To reassure the working classes of the honesty and value of parliamentary government, some *beau geste* was necessary, *e.g.* the Nationalisation of the Mines.

Mr. Laski said, in conclusion, that he could see no better method of government for Great Britain than by the party system, which was so closely allied to the spirit of our race.

In the discussion which followed Mr. Graham Wallas, Mr. G. P. Gooch, and Mr. Finer took part.

Science Masters' Association (G. H. J. Adlam, M.A.)

Edu at.onal conferences are generally very heavy, solemn affairs from which the rank and file often go away depressed rather than stimulated, and with their mental faculties in rags and tatters, so to speak. Their only consolation is that, by putting in an appearance, they have done the correct thing, and they hope that benefits may follow. The S.M.A. does not aspire to the dignity and dullness of a conference, but prefers to call its annual gathering a "meeting," and encourages the social side, being convinced that members get at least as much help from informal talks with fellow members as from the set pieces.

This year the Association met at Oxford (Jan. 4–7), and by the kind permission of the authorities the members were housed in Balliol and Trinity

Colleges. The arrangements made by Brig.-Gen. H. Hartley were admirable. The attendance was only one or two short of 200—a most gratifying record for an association numbering about 575 members domiciled all over Great Britain, Ireland, and the Colonies.

A notable feature of the educational side of the meeting was that the members were expected to, and indeed did, drop back again to something approaching the status of undergraduates, and received instruction, sometimes from former tutors, sometimes from those who were once fellow undergraduates, and sometimes even—not without feelings of pride and satisfaction—from their own old pupils.

Since the days when some of the members were "up," a lot of water has passed under Folly Bridge. Let those who scorn the implication perpend the book of Ecclesiastes or the writings of Dean Inge. In the meantime, the progress of science has been very rapid, and many have been left behind, for, amid the multifarious duties of a science master, very little time is left for study and contemplation. It was, therefore, most stimulating and encouraging to members to be brought once again into an atmosphere of recent advance, and was, at the same time, most salutary to be kindly but firmly reminded of the perishable nature of their stock-in-trade.

The proceedings started with an address by Mr. Archer Vassall of Harrow, a member of the Association, one of its founders, and the first member to be called to the office of president. Mr. Vassall's address dealt with the following professional topics, among others, as they appeared to him looking back over his twenty-five years' experience.

Preparatory School Science.—The relation between the curricula of the preparatory school and the public school has always presented difficulties. With one or two exceptions, science is unknown in the preparatory school; consequently, so far as science is concerned, the boy of fourteen comes to the public school with his mind, as John Earle puts it, "yet a white paper unscribbled with observations, wherewith at length it becomes a blurr'd note-book." Nevertheless, he has the valuable quality of freshness and unbounded curiosity. This ignorance of elementary science may be no great drawback. By the introduction of a paper in the common entrance and scholarship examinations, preparatory schools might be compelled to teach "science," though the remedy might be worse than the disease, for boys might then come knowing no real science, but only "examination science," and, as a result of that, their ardour might be somewhat abated.

English in Preparatory Schools.—The halting way in which boys of fourteen, whether from the preparatory school or the primary school, use their mother tongue, their scanty vocabulary, their ignorance of the significance of even common words, are far greater drawbacks than ignorance of formal science. To quote Mr. Vassall's own words: "Except for a few gifted boys amongst the scholars, our present material, though having the invaluable quality of freshness, has a handicap which in many cases more than neutralises the virtue. This handicap, as you know, is that boys arrive at our schools having spent four or five years playing at three or four languages and having a mastery of no single one, least of all—in the case of British boys—of their mother tongue." If English were taught, and properly taught, "we might hope for much more average intelligence at fourteen, and for material which we could use in our own branch of education during the next year or two up to a standard undreamt of at present."

Science for All.—As to the aims of science teaching in schools, emphatically it should not be merely the training of the best boys only, to the total neglect of the weaker brethren. The classics did this, and did it very thoroughly, both in developing the faculties of the "best" boys and in crushing the less able like a Juggernaut car. It was precisely in consequence of this that the classicists lost their hold in the schools. There is a danger that scientists

may repeat the blunder, forgetting that in every school there are not only future scientists, whether academic or industrial, but also others—future citizens, in fact. The many can no longer be sacrificed in the interests of the few, and clearly it is the schools that must look after future citizens, future cabinet ministers, members of Parliament, and labour leaders. The general aim of science teaching for the average citizen must be the interpretation of Nature; its particular aim must be the inculcation of scientific method and a critical faculty, the stimulation of the imagination and the æsthetic sense—not, let it be added, without the appreciation of the value of science through its applications.

A Retrograde Movement.—There is another aspect of Science for All. Under the stress of war good resolutions were made, and science masters seemed well on the way towards gaining an important point, namely, that every boy capable of profiting by it should at least have the opportunity of doing a reasonable amount of science during his school career. But, as was clearly foreseen by some and forcibly expressed by Sir Harry Johnston in his presidential address to the S.M.A. in 1918, now that peace is restored there is a tendency to slip back again into the old ways, and "time allotted to science is already being filched back for Greek." The fight for adequate science teaching in schools is not yet won; in fact, in Mr. Vassall's opinion it is only just beginning.

Danger from the Specialists.—The specialist, whether geographer, historian, mathematician, or scientist, is a source of grave danger in schools, particularly if he lacks the humanities (not necessarily Latin and Greek) and a saving sense of humour. A man may have a full and adequate knowledge of a subject without being a specialist in a narrow sense; he may be an enthusiast without being a latter-day saint. For the majority of boys school science is the only introduction to the subject that they are going to get, and it is folly, or worse than folly, to confine these, at any rate, to the narrow path of the future specialist, even if it is not wrong to narrow down the preliminary education of any boy to the point that it is indistinguishable in a moral sense from vocational training.

Supply of Science Teachers.—The future supply of adequately trained scientists depends on the demand, and also on the prospects that a scientific career has to offer. If industry requires first-class scientists, it will make for them an attractive career. In the near future the new Civil Service regulations may greatly affect the distribution of trained scientists. Academic science will probably claim its own as heretofore. With all these more attractive prospects, it is difficult to see how schools will be adequately staffed if science masterships are not also made more attractive.

For the remainder of the meeting Oxford did the talking, and most delightful talk it was. The Master of Balliol, the Warden of Wadham, and Prof. Sir Charles Oman, M.P., conducted parties round some of the colleges. Prof. T. R. Merton, F.R.S., lectured on Spectroscopy to a large and critical audience, which was obviously delighted. Mr. J. S. Huxley made a fascinating but difficult subject—the control of growth—clear, even to the non-biologist. Brig.-Gen. Hartley snatched an hour from his most arduous labours as organiser to give a most suggestive lecture on Indicators and the Law of Mass Action, while Mr. D. G. Hogarth brought the proceedings to a close with a singularly clear and attractive lecture on the Hedjaz.

The university laboratories were thrown open during the meeting, and demonstrations were arranged in all. These demonstrations not only indicated many possible improvements in the laboratory technique of schools, but showed also the lines of future progress. Whatever may be happening elsewhere, it is abundantly clear that the Natural Science School at Oxford is vigorous and well equipped with inspiring teachers.

Our Holiday System

Is it not time that some effort should be made to reform our system of holidays? The observance of bank holidays, and of Christmas, Easter, and summer holidays, for the whole public on the same dates really causes an immense amount of trouble, expense, and dissatisfaction, for the obvious reason that all the people are compelled to pour away out of the cities to seaside and other resorts almost at the same time, thus overcrowding the hotels, boarding-houses, and lodging-houses, besides flooding the railways, tramways, and roads. One result is that keepers of hotels and lodging-houses are able to raise their prices owing to the competition and that every-one is made uncomfortable, while at the same time most important affairs are apt to be paralysed for days and weeks. Would it not be much better to make some effort towards persuading the people to take their holidays, not on fixed dates for all, but whenever they can conveniently be spared? Of course this is already done to a certain extent, but not to the extent which one could wish. Last year the railways were offering inducements for travel in June and July instead of August, and we may hope that this effort will be continued.

The real reason for the simultaneous crush is that the schools give their holidays all at the same time. Could not this possibly be avoided? Surely some schools might have their holidays in June, others in July, and others in August, and parents could send their children to those schools which have their holidays at times which suit them. The division of terms at the Universities might also be revised. It is essential to our C 3 population that it should obtain as much fresh air as possible; why, then, is so much holiday-time given at Christmas and Easter? Would it not be better to concentrate most of our holidays in the summer months, especially June, July, and August? In Sweden, for instance, nearly the whole of this quarter is devoted to the health-giving open-air holiday existence, and the people work all the harder for it during the less agreeable months. The bank-holiday system is especially bad—not because holidays are given, but because they are given on the same dates. Surely it would be possible to rule that every person must have so many holidays during the year without specifying that the whole nation shall take them simultaneously. This would be a great relief to the railways, the hotels, and to lodging-houses at the seaside, and, most of all, to the nation at large. Prices for lodgings would fall; the seaside hotels would be occupied all through the summer; and people would be able to enjoy the country-side without being always surrounded by crowds eating buns and chocolates and making the peculiar kinds of noises which they call singing.

Dikinz Drops iz Eitshiz (R. R.)

Last July we ventured to write in support of the criticism which the Poet Laureate had published against a school of "fonertishns," who, he thought, made out spoken English to be even more slipshod than it really is. This school is, however, really engaged not so much on phonetics as in instructing foreign learners of our language—such as, let us say, German commercial travellers—how to speak English without a trace of a foreign accent; and they achieve this result by using the phonetic notation of the International Phonetic Association. Since then we have received Charles Dickens's story, *Our Mutual Friend,* transcribed into the same script by Mr. C. M. Rice, M.A., A.R.C.M. (W. Heffer & Sons, Cambridge, 1920). Mr. Rice is to be highly commended for what must have been a very serious labour, and the book will be of interest not only to foreigners but to the many Britons who would like to see how the "fonertishns" are pleased to think we are in the habit of speaking. The book is a curiosity, and deserves

to be studied apart from its special purpose; but I should like to make two remarks upon it.

First, regarding the supposed pronunciation of English. As I said before, I do not think that it is a sound rendering. Pronunciation is a very variable quantity, and I myself am conscious of using different pronunciations when I speak in various ways. When I talk casually to a friend I suppose that I approach very nearly to fernetik English; when I talk through the telephone my phones are much more distinctly uttered; and when I am called upon to address an audience I am forced to be equally distinct, and therefore to soar far above the fernetik standard. This book adopts the first class of pronunciation almost throughout, not only when Dickens's characters are speaking, but when he speaks himself. With the former perhaps we can have no quarrel; but is it quite fair to treat Dickens himself in a similar manner? Would he have said *klouz* instead of *clothes*? Above all, would he have dropped his h's in unstressed words, as in *him* and *his*? I do not quite blame Mr. Rice for this, because I know that it has long been supposed among fonertishns that we actually do drop our h's in unstressed positions—even when we speak "educated" English; though, of course, we should not dream of dropping an h at the beginning of a stressed word. The pose is a similar one to that which ascribes to our aristocracy the habit of dropping g's in *shootin'* and *fishin'*. But I do not think either accusation is true, and have recently made some very careful observations on the point among my friends. Indeed, they have been struck by the fact that I have attended much more closely to their conversation than usual, and, on their asking the reason why, they have been informed that I wished to find out whether they dropped their unstressed h's. As a matter of fact, not one of them did so, even though many of them are such humble or half-educated creatures as mere men of science, or doctors, or the like. But I have had the supreme honour of listening to one peer, and he dropped neither his h's nor his g's; while lecturers on platforms and parsons in pulpits have seldom done so in my hearing. On the other hand, I know a peer who drops his stressed h's, and always does so, I am convinced, on purpose, just to show his independence—and quite right too! As the result of my inquiry my friends have been rather hurt; but I did find one common defect even among the most distinguished of them, namely, the horrible one of inserting an r where it ought not to be. For example, a man told me that he was just off to the *Indier Office*. The reader will easily obtain an *idear of* what I mean. This is, of course, a serious fault.

Now, as to the script employed—I am very familiar with the notation of I.P.A., but must confess that this book has given me a rude shock. I find great difficulty in reading the book, in spite of my familiarity with that notation. It has fundamental mistakes which ought never to have been made—though, at first sight, they seem to be very small matters. We must remember, however, that ordinary script has been evolved after many centuries of trial and is designed, if not for expressing words phonetically, yet certainly for expressing them legibly. For example, capitals are not used in this book for proper names, thus causing immediate stumbling of the reader almost in every sentence. Again, the "longus" employed for the vowels is a very bad one, because it breaks up numbers of words as if with a colon, and it takes a moment or two to learn whether we are not dealing with a mark of punctuation. Again, the word *weev* perplexed me for some time until I found it meant *we've*—another defect in the notation, because we must make such distinctions in the written word if not in the spoken. Again, the accents (which are placed before the accentuated syllables) are a terrible plague, and are often unnecessary. Really the consonantal diphthong in *judge* is not very truthfully represented by *dzhudzh*—unless the speaker has been dining somewhat freely. In this case, however, the speaker seldom

uses either this word or the similar word *tshurtsh*, so that I have not been able to test the question by direct observation. The fact regarding this script is that, like the spelling adopted by the Spelling Reform Society, it has not been deeply enough considered with reference to the actual needs of general readers as distinct from phoneticians. It would be easy to employ a notation which would render languages just as phonetically as the notation of the I.P.A. does, but without the horrible devices used in the latter.

But all this does not detract from the credit due to the author for his pains.

The Quarterly Report of the Research Defence Society

The Quarterly Report of the Research Defence Society for January last publishes a photograph of Gloucester Cemetery, where, it is stated, "279 poor unvaccinated children under ten years of age lie buried, who fell victims to the epidemic of smallpox in Gloucester in 1895–6, together with only one child out of more than eight thousand who were vaccinated before or during the epidemic." It is hoped that the new Ministry of Health will have at least one great function, and that will be to exercise some control over our more ignorant municipalities in their frequent neglect of life-saving scientific discoveries and inventions.

Notes and News

The New Year Honours List included the names of Prof. P. R. Scott Lang, Regius Professor of Mathematics in the University of Aberdeen, and Prof. James Walker, Professor of Chemistry in the University of Edinburgh, both of whom received knighthoods.

The 1920 Nobel Prize for Chemistry was awarded to Prof. A. O. Aschan, of Helsingfors, in recognition of his researches in connection with the production of synthetic rubber. Nobel prizes were also awarded to Dr. Jules Bordet, Professor of Bacteriology in the University of Brussels, and to Dr. A. Krogh, Professor of Animal Physiology at Copenhagen.

The following candidates for election to the Fellowship of the Royal Society have been selected by the Council: W. E. Agar (Professor of Zoology in the University of Melbourne); Dr. F. W. Aston (Physicist and Research Fellow of Trinity College, Cambridge); W. L. Bragg (Langworthy Professor of Physics in the University of Manchester); Dr. W. T. Calman (of the Natural History Museum); Dr. A. H. Church (Lecturer in Botany, Oxford University); G. Dreyer (Professor of Pathology, University of Oxford); W. H. Eccles (Professor of Applied Physics at the Finsbury Technical College); Dr. J. C. G. Ledingham (Chief Bacteriologist, Lister Institute); C. G. Middlemiss (lately Superintendent, Geological Survey of India); K. J. P. Orton (Professor of Chemistry in the University College of North Wales, Bangor); Dr. J. H. Parsons (Ophthalmic Surgeon to University College Hospital); J. C. Philip (Professor of Physical Chemistry in the Imperial College of Science and Technology and Secretary of the Chemical Society); Dr. A. A. Robb (Mathematician and Physicist); Sir E. Tennyson d'Eyncourt (Director of Naval Construction); G. Udny Yule (Lecturer in Statistics, University of Cambridge).

The Poncelet Prize for Mathematics has, this year, been awarded by the Paris Academy of Sciences to Elie Cartan; the Janssen Medal for Astronomy goes to Dr. W. W. Coblentz, of the Bureau of Standards, Washington, in recognition of his work on infra-red radiation; the L. La Caze prize for physics to G. Sagnac; and 4,000 francs from the Bonaparte Fund to Émile Mathias for his researches (in collaboration with Kamerlingh Omnes) on the curve of densities of gases whose critical point is near the absolute zero.

Sir Thomas Kirke-Rose has received the gold medal of the British Institution of Mining and Metallurgy for his services in the advancement of metallurgical work, more especially with reference to the metallurgy of gold.

Prof. J. Perrin (Paris) and Prof. C. Fabry (Marseilles) have been elected honorary members of the Royal Institution, and Prof. Arthur Keith has been re-elected Fullerian Professor of Physiology for a further period of three years.

The Wollaston Medal of the Geological Society has been awarded (in duplicate) to Dr. J. Horne and Dr. B. N. Peach.

The Weldon Medal for biometric research has been given by the University of Oxford to Dr. J. A. Harris, of the Carnegie Institution of Washington.

It is stated that Dr. A. Einstein has accepted the " Chair of Science " in the University of Leiden, and that he will hold it in addition to his post in the University of Berlin.

We have noted, with great regret, the announcement of the deaths of the following well-known scientific men during the past quarter: Sir William Abney; Dr. H. A. Bumstead, Professor of Physics at the Sloane Physical Laboratory, Yale University; Dr. G. von Bunge, Professor of Physiology at the University of Basel; Mr. J. C. Cain, the chemist; Mr. C. E. Fagan, Secretary of the Natural History Department of the Natural Science Museum; Sir Lazarus Fletcher, until lately Director of the Natural History Museum; Prof. F. Houssay, Professor of Zoology at the Sorbonne; Prince Kropotkin; Dr. S. J. Meltzer, head of the department of Physiology and Pharmacology in the Rockefeller Institute for Medical Research; Dr. H. N. Morse, Professor of Chemistry at the Johns Hopkins University, and famous for his experimental measurements of osmotic pressure; Alexander Muirhead, F.R.S., the electrical engineer and cable expert; Spencer Pickering, F.R.S., physical chemist and horticulturalist; Mr. F. Pullinger, Chief Inspector of the Technological Branch of the Board of Education; Dr. Charles A. Sadler, well known as joint author (with Prof. Barkla) of papers on the absorption of characteristic X-rays.

Sir John Snell, member of the Council of the Institution of Civil Engineers, and past President of the Institution of Electrical Engineers, has been appointed to be a member of the Advisory Council for Scientific and Industrial Research. Sir R. T. Glazebrook has been appointed Chief Gas Examiner under the Gas Regulation Act (1920), and Mr. C. V. Boys, Dr. J. S. Haldane, and Mr. W. J. A. Butterfield to be Gas Referees.

In the last issue of this journal we were unable to mention a single gift of any magnitude to the scientific institutions in this country; this quarter there are two. Mr. George Wills and Mr. Henry Wills, of the Imperial Tobacco Co., have given £200,000 for the completion of extensions to the University of Bristol (making a total of £540,000 from the family since 1908), and an anonymous donor has given £20,000 for the endowment of a University Chair of Physiology at Middlesex Hospital Medical School. There have probably been contributions towards the appeals for £500,000 each from the Universities of Birmingham and Leeds, and the Armstrong College, Newcastle-on-Tyne; but in none of these cases has the satisfactory closure of the appeal yet been announced. The appeal for a centennial endowment fund from McGill University was met in a different spirit; $5,000,000 was asked for, $6,321,511 was subscribed in fourteen days—as much as the Universities in Great Britain have received from private benefactions during the whole three years 1916–19!

Among the donations to universities abroad we must mention first the sum of 1,500,000 marks given to Frankfort University by James Speyer, the New York banker. The General Education Board of New York has given $700,000 to the Medical School of the University of Colorado, and an equal sum to the Medical College of the University of Cincinnati, on the condition that $1,300,000 is raised elsewhere to complete the two million dollar endowment fund of the college. Williams College, Clinton County, U.S.A., receives the residuary estate of its late senior trustee, Mr. F. L. Stetson (this, it is estimated, will amount to from one to one and a half million dollars),

and in addition receives $100,000 to establish eight scholarships for students from the county. Yale University has received $100,000 from an anonymous donor for its department of Public Health, and Stanford University is in so flourishing a financial position that it proposes to spend $750,000 on a stadium, seating 60,000 spectators, for the 1923 intercollegiate games.

In connection with Mr. Rockefeller's generosity to educational causes and to medical education in particular, it is interesting to note that he has given away altogether £123,000,000. This still leaves him a reasonably wealthy man, inasmuch as he has accumulated altogether in his life-time about £375,000,000 !

We are very pleased to note that, on November 17 last, Mr. Justice Eve refused an injunction to prevent Messrs. Brunner, Mond & Co. from distributing the sum of £100,000 among the universities and scientific institutions of the United Kingdom for the furtherance of scientific education and research.

The Hudson Bay Company has given the University of Manitoba a fellowship of the annual value of $1,500, in commemoration of the 250th anniversary of the foundation of the Company and of its long association with Winnipeg. The fellowship is open to graduates of any Canadian University, and may be used for any branch of research.

A Ramsay Memorial Fellowship of £300 a year for three years has been founded by subscriptions received from the Swiss Government and Swiss donors as a result of the energetic action of Prof. Ph. A. Guye, of Geneva. The first fellow is now working in Prof. Perkins' laboratory at Oxford.

The Institute of Physics has now been incorporated and has begun to carry out its work. It will be remembered that the object of the Institute is, on the one hand, to secure the recognition of the professional status of the physicist, and, on the other, to co-ordinate the work of all the societies interested in physical science or its applications. This co-ordination has already been secured by the participation of five of these societies, namely: the Physical Society of London, the Optical Society, the Faraday Society, the Royal Microscopical Society, and the Röntgen Society. The Institute thus promises to become a powerful unit in connection with any wider scheme of federation of scientific societies.

The first list of members includes the names of over 200 Fellows. Sir J. J. Thomson, O.M., the retiring President of the Royal Society, has accepted the invitation of the Board to become the first, and at present the only, Honorary Fellow. It is a tribute to the status already acquired by the newly formed Institute that its diploma is now necessary for applicants for Government and other important positions requiring a knowledge of physics, and, thanks in great part to the Institute, the physicist is now becoming recognised as a member of a specific profession. The first President of the Institute is Sir Richard Glazebrook, K.C.B., F.R.S., who will preside at the first Statutory Meeting of the Institute which will be held. Particulars with regard to the qualifications required for the different grades of membership can be obtained on application to the Secretary, 10, Essex Street, London, W.C.2.

As a result of the balloting in the reorganisation of the International Commission on Zoological Nomenclature, England will be represented by Dr. J. A. Bather (London), Dr. W. E. Hoyle (Cardiff), Dr. K. Jordan (Tring), and Dr. E. J. O. Hartert (Tring). The first three of these retire in 1922 and the last in 1928.

It is stated that the paid-up membership of the American Association for the Advancement of Science is 10,000, 600 members having joined since November 1, 1919. At the seventy-third meeting of the Association, held at the end of December last in Chicago, 2,412 persons registered their attendance, and probably many others attended without registration. The most notable paper presented to the Association was that read by Prof. Michelson

on the application of interference methods to astronomical measurements. In this paper he announced that the diameter of *a* Orionis had been measured and found to be about 260,000,000 miles—a figure which confirms the estimate previously made from theoretical considerations by Prof. Eddington. It appears, however, that the calculations on which the result is based depend on our rather uncertain knowledge of the parallax of the star.

Dr. F. W. Aston gave an account of his work on the isotopes of the lighter elements at the Royal Institution on February 11. A full description of these experiments by Dr. Aston himself appeared in SCIENCE PROGRESS last October. The list of elements given in that paper has now been extended by the addition of iodine, which proves to have only one form. The existence of two isotopes of argon has now been confirmed, and two more possible isotopes of xenon have been detected, making seven in all for this element. Dr. Aston received the first Mackenzie Davidson medal from the council of the Röntgen Society for the paper on this work which he read before the Society. This medal is to be given annually for the best paper presented to the Society during the session.

In a letter to SCIENCE (December 10, 1920) from the Ryerson Physical Laboratory, Chicago, A. G. Dempster announced that he has succeeded in analysing magnesium (atomic weight 24·36) into three isotopes of atomic weights 24, 25, and 26 present in the metal in relative amounts 6, 1, 1, and so giving as close an agreement with the accepted atomic weight of the element as can be expected at the present stage of experimental work.

In the Kelvin Lecture to the Institution of Electrical Engineers Sir William Bragg indicated that, by means of improvements he has recently made in his X-ray spectrometer, he has been able to show that atoms have different properties in different directions. This follows indeed from the fact that the atoms in a crystal are not always close packed as they must be if they are not held together in definite positions. Thus some at least of the electrons surrounding the central nucleus of the Rutherford atom must occupy fixed positions relative to the nucleus, a view which accords with the Lewis-Langmuir theory of atomic structure. These "fixed" electrons may describe small closed orbits about fixed points on the "surface" of the atom and so produce magnetic fields which serve as bonds of attachment between the atoms. This is the theory advocated by Dr. A. E. Oxley, who gave an account of some of his work in SCIENCE PROGRESS, April 1920. A further account of this subject will appear in the July number of SCIENCE PROGRESS from the pen of Prof. W. L. Bragg.

In a lecture on Helium last July at Toronto, Prof. McLennan expressed the hope that a cryogenic laboratory for low temperature research would be established in the British Empire. It is now stated that a laboratory of this kind has already been constructed, but in the building of the U.S. Bureau of Mines, Washington. It has two 4-stage compressors with a capacity of 75 c. ft. of atmospheric air per minute for liquid air manufacture, a compressor with a capacity of 12 c. ft. of air for liquid hydrogen, and another of 8 c. ft. capacity for use in connection with a liquid helium cycle, together with the necessary gas-holders and mechanical equipment.

The *Technical Review* (December 14, 1920) contains an abstract from an article in the *Rivista Marittima* (July–August 1920) giving some important details concerning the production of helium from the natural gas reservoirs in Texas. It appears that the U.S. Government compelled the companies owning the wells to send all the gas obtained to the establishment at Fort Worth for the helium to be extracted before the gas was distributed for private consumption. This arrangement has caused many complications, and naturally the companies have made heavy claims against the Government, tending to greatly increase the cost of the helium. But a more serious question confronts the authorities operating the extracting plant—namely,

the fact that the duration of the gas blow-holes is predicted by geologists to be very short. The consensus of opinion is that it will be exhausted in 10–15 years, and the costly plant installed will be useless. So far, no army or naval airship has been filled with helium, which would seem to prove that production is still very limited.

Considerable doubt has been expressed by many people as to the wisdom of the Government in crowding the universities and technical schools with ex-service men; or perhaps it would be better to say, as to the wisdom of the ex-service men who have accepted government grants for this purpose, since it is notorious that authorities who provide scholarships never worry themselves in the least as to the ultimate fate of their victims. That these doubts are likely to be justified is proved by the following extract from the fourth number of the *Archives of the Cambridge University Forestry Association* (October 1920): "With the exception of Indian and Colonial conservators, we have heard of no appointments either by the Forestry Commission or by private owners of estates. It is certain that far too many disabled officers were encouraged to take up Forestry, and that there is now a large number of qualified men at liberty who cannot hope to be absorbed for several years. The responsibility for the flooding of the market with trained foresters is divided amongst several Ministries, and it is difficult to fix the blame on one more than on another; still, the necessity of turning to some other occupations after having wasted two years naturally causes those whose wounds prevent them considering posts in the tropics, to feel that they have been betrayed."

It seems very certain that the "flooding of the market with trained foresters" will be insignificant compared with the flooding with trained chemists (and engineers) after the degree examinations of 1921 and 1922, and bitter feelings of betrayal are likely to become widespread among the many men who are working really hard in the hope of achieving a successful scientific career.

Refractometers are used very widely by industrial chemists in their routine work, but the methods of spectrum analysis have hitherto been employed very little for this purpose. A little monograph published by Messrs. Adam Helger, Ltd. (*Optical Methods in Control and Research Laboratories*, pp. 30, price 1*s*. 6*d*.), shows that with suitable apparatus the spectrometer and spectrophotometer provide the chemist with an additional and very powerful help towards the solution of his everyday problems. The constant deviation spectrometers constructed by Messrs. Hilger enable the positions of the lines or absorption bands in the spectrum of a substance to be determined very rapidly, both in the visible region and in the ultra-violet. By combining the spectrometer with a sector photometer or a Nutting photometer quantitative measurements of the absorption can be made, the whole of the data required for the construction of an absorption curve being obtained on a single photographic plate. The quantities of certain substances which can be detected by the presence of their characteristic bands in the ultra-violet spectrum is extraordinarily minute. Thus, Hartley and Dobbie (*J.C.S.*, 1900, **77**, 318) found that as little as 0·00001 gm. of pyridine in 100 c.c. of water produced a definite effect in the ultra-violet spectrum, while 0·5 mg. of strychnine can be detected and verified by its ultra-violet bands. The monograph deals also with refractometers and polarimeters, and supplies a very useful bibliography to the recent literature relating to each instrument.

The Cambridge University Press announces that, instead of publishing a new edition of N. R. Campbell's *Modern Electric Theory*, it proposes to issue monographs, each corresponding roughly to a chapter of the book. These monographs will be edited by Dr. Campbell, but will be written by different authors, who will not be experts in the branches of physics concerned. This rather novel procedure has been adopted on the ground that

a critical survey of a subject, such as is appropriate to a text-book, is more easily adopted by those who have not made important contributions to it. It is hoped to publish the first of these books in October, and the first three will deal with Spectra, the Quantum Theory of Energy, and the Constitution of Atoms and Molecules.

"Just a quarter of a century ago—that is, in 1895—the announcement was made that diphtheria, one of the most severe and fatal diseases of mankind, had been conquered by means of an antitoxin. This great event is a landmark, not alone in the history of medicine, but also in the history of the world, and it provides a high peak of achievement from which the growth of bacteriology may be viewed." (From the Address of the President of the American Association for the Advancement of Science.)

Prof. Robert Hegner, lately appointed to the Chair of Protozoology at Johns Hopkins University School of Hygiene, has written an account of his recent European tour, which appears in *Science*, December 24, 1920, and during his stay in England he attended the British Association at Cardiff. He remarked upon the absence of young men in the audience of zoologists at that meeting of the Association. It is quite true that the supply of young zoologists is becoming very low, and, unless steps are taken to induce more students to take up the subject, we can see a possible future decline in British zoology.

Messrs. Beck & Co. have brought out a new cheap microphotographic camera suitable for all grades of work. It is solid and handy.

We have to announce the new Imperial Bureau of Mycology, which is the outcome of a proposal unanimously adopted by the Imperial War Conference in 1918 that a central organisation should be established for the encouragement and co-ordination of work throughout the Empire on the diseases of plants caused by fungi, in relation to agriculture. The Committee of Management consists of some of the foremost biologists in the country.

Dr. E. J. Butler, late Imperial Mycologist, Director of the Research Institute, Pusa, and Agricultural Adviser to the Government of India, has been appointed Director and has started work at the headquarters of the Bureau, No. 17–19, Kew Green, Kew (Telephone, Richmond 603); this site has the advantage of proximity to the fine library and collections of the Royal Gardens, with the director and staff of which the Bureau will work in co-operation.

The funds of the Bureau are entirely provided by contributions from the various self-governing Dominions, India, Egypt, and the Soudan, and the non-self-governing Colonies and Protectorates. It will work broadly on the lines of the existing Imperial Bureau of Entomology at South Kensington, and will aim at doing for the other great class of destructive agencies in agriculture—namely, the diseases and blights of plants caused by fungi—what the older Bureau has so successfully done in regard to injurious insects. It will be a central agency for the accumulation and distribution of information and for the identification of specimens sent in from all parts of the Empire. It is proposed to issue, as soon as funds permit, a periodical journal through which those interested in mycological work in regard to agriculture will be kept informed of progress elsewhere. There are at present over fifty officials engaged in this work in the overseas parts of the Empire, while the number of agriculturalists, planters, and the like practically interested is legion.

The effect of fungus diseases in reducing crop production is great beyond calculation. A Canadian scientist has estimated the loss caused by rust in wheat in the prairie region of Canada in 1917 at 100,000,000 bushels, representing a value of between £25,000,000 and £50,000,000. For the same year the loss in the five chief cereals in the United States exceeded 400,000,000 bushels. The effect of this on the provisioning of the world may be easily imagined.

CORRESPONDENCE

To the Editor of "Science Progress"

THE INHERITANCE OF ACQUIRED CHARACTERS

I. From Julian S. Huxley, M.A.

Sir,—The article by Prof. MacBride on "The Inheritance of Acquired Characters," which appeared in Science Progress for January, contains certain statements which I think should not pass unchallenged. As regards the positive results of experiments on the transmissibility of modifications, the matter can be allowed to rest until further independent work has either proved or disproved their accuracy. The issues hanging on them are so large that they cannot be accepted as gospel on the word of a single investigator, however capable.

It is with other aspects of Prof. MacBride's position that I am concerned. On p. 309 he states that "*the pathological mutants* which Mendelians employ for their experiments are utterly unlike the variations which the study of comparative anatomy induces us to postulate as the material of evolution" (italics mine). Such a statement is not only inaccurate; it is unfair. Among the characters which are known to segregate according to Mendelian laws are the finest gradations of coat-colour and marking in rodents (*e.g.* mice, guinea-pigs); size (fowls, Punnett; peas, Mendel); the forms of pistil and stamens in heterostylic plants; every shade of eye-colour in Drosophila; starchiness and sugariness of seeds (peas, maize); leaf-form (many plants); phototropism and its absence (Drosophila); sex (many animals and plants); hen-feathering (fowls); minute modifications of proportion and distribution (Morgan's modifying genes); increases or decreases of vigour (maize, East and Jones); probably the patterns of dimorphic butterflies; probably many apparently quantitative characters, such as flower-size (tobacco-plant, East); fertility (fowls, Pearl; Drosophila, Hyde); fluctuation of pattern (rat, Castle)—but the list could be extended indefinitely. I have jotted down the first instances that came into my head of characters that happened to be both Mendelian in their inheritance and normal in their nature.

It is natural that a great many abnormalities are also known to be inherited according to the same laws; they are what strike the eye of the investigator first. But it is not only in Genetics that pathology has helped to an understanding of physiology.

It is, in any case, quite obvious that the mechanism of segregation is a fundamental part of the organisation of animals and plants. It may or may not turn out to be identical with the chromosome-mechanism; but, in any event, I do not believe that there exists a single geneticist to-day who would deny that it represented something universal and fundamental. If so, it is obvious that it will be capable of segregating the abnormal equally with the normal. Prof. MacBride, in order to clear the ground for theories which do not fit in well with Mendelism, prefers to look at the abnormal. Most zoologists will, I think, prefer to look at the normal. The mechanism

itself is a mere distributor. The variations thus distributed are, as Darwin long ago urged, neutral—of every description, bad, good, and indifferent; and it is natural selection which eliminates the bad, and so the good are left.

Prof. MacBride also leaves out of account all reference to modifying factors, of small and yet Mendelian differences, and other points which are of importance for the presentation of the Mendelian case.

In one specific instance he also, I venture to say, makes a difficulty where none exists. On p. 397 he refers to the well-known experiments whereby a lens is formed from a portion of epidermis from which lenses do not usually develop; and he raises an argument about the existence of special lens-determiners which " recalls the delicious blends of thirty years ago," when Weismann was to the fore. Surely, if we simply postulate (*a*) optic cup, (*b*) epidermis, (*c*) a substance formed by the optic cup which causes epidermis to develop into lens, we have a formal explanation which is no harder to swallow than many definite facts of physiology—*e.g.* the specific action of adrenalin. It is, at any rate, infinitely harder to explain the facts from any Lamarckian standpoint! Similarly with the regeneration of an amphibian leg (p. 396), Prof. MacBride makes his adversaries think in terms of determiners, whereas they would much prefer to do so in terms of organic equilibrium.

I might also refer to his statement on p. 404: " It is necessary [to carry out convincing experiments on the inheritance of acquired characters] to find an animal that will respond to a change of environment by a change of habit or structure, and this is by no means easy." To the uninitiated this would appear to detract from the importance of Lamarckism as an evolutionary factor.

Finally, he makes an assumption as to Guyer's work (p. 405) which appears to me to be wholly unjustified. Guyer states that he has caused hereditary changes in the lens of the eye of rabbits by injecting anti-lens serum. Prof. MacBride's comment is as follows: " If this conclusion be accepted, the cardinal principle of the theory of the inheritability of acquired characters, *viz. the influence of the soma on the germ*, is conceded " (italics mine). Personally, I had always supposed that the " cardinal principle of the inheritability of acquired characters " was that *adaptive* somatic changes, especially those concerned with *use and disuse*, were transmitted to the offspring in some degree. I am not acquainted with any leading biologist since Weismann's time who would deny that changes in the body *might* affect the germ-plasm, although the process does not appear to be common. Such a phenomenon, known as somatic induction, is familiar in all the textbooks—but it has very little in common with Lamarckism. If Guyer's results are confirmed, we are brought face to face with new possibilities in biology—the identification of factors in the germ-plasm through the methods of immunology. The reaction would appear to depend on protein specificity, and to be very unlike (to use Prof. MacBride's own argument) anything that might occur in nature.

In conclusion, I should like to point out that I have not concerned myself to attack Prof. MacBride for upholding this or that doctrine. I have only wished to protest against his belittling the positive achievements of branches of science which do not happen to square with his beliefs, and against his using, as support for the theories which he upholds, results which do not support those theories as they are ordinarily stated.

With apologies for trespassing upon your space at such a length,

Yours faithfully,

JULIAN S. HUXLEY.

NEW COLLEGE, OXFORD,
January 28, 1921.

II. From Prof. E. W. MacBride, F.R.S., D.Sc.

Dear Sir,—I am deeply indebted to you for your courtesy in allowing me to see, in proof, the letter of Dr. Julian Huxley criticising my article on the inheritance of acquired characters which appeared in your issue of January last.

Dr. Huxley's criticisms may be summarised as follows:

(*a*) He accuses me of belittling the achievements of Mendelian investigators, and objects in particular to the statement that "the pathological mutants which Mendelians employ for their experiments are utterly unlike the variations which the study of comparative anatomy induces us to postulate as the material of evolution."

(*b*) He criticises the evidence which I have brought forward in favour of the truth of the Lamarckian doctrine of the inheritance of acquired characters.

Let us deal with the first point to begin with. Dr. Huxley gives a list of characters distinguishing the mutant from the type which he asserts to be normal—*i.e.* if I understand him aright, to be comparable to the differences which separate allied species in nature. Not one of these will stand examination, and some are positively ludicrous. Amongst these characters occur "sex." Does Dr. Huxley seriously wish us to believe that the difference between male and female is one analogous to that separating two allied species? I have not space to deal with all the instances mentioned, but shall try to expose one or two. For instance, Dr. Huxley mentions "every shade of eye-colour in Drosophila." Now the normal eye-pigment necessary for vision in the eyes of this fly is of a dark red colour. In Morgan's cultures, however, "mutants" appeared in which the red pigment had suffered successive degrees of degradation, giving rise to insects with eyes of colour termed cherry, eosin, cream, écru, and finally pure white. Dr. Allen has encountered very similar pathological mutants in his study of the eye of the Crustacean Gammarus. If, however, we compare these degenerate eyes with the vestigial eyes characteristic of natural species, we find no resemblance whatever. In the Amphibian Proteus the eye in the adult is a mere vestige, but it shows no regression whatever towards albinism. As Dr. Huxley is perhaps aware, Kammerer has shown that, by exposing the eye of the larva of Proteus to suitable illumination, it can be made to grow into a well-developed eye.

Size is another of the characters which, according to Dr. Huxley, "segregate according to Mendelian laws." But Dr. Bateson, in his Croonian lecture on segregation, states, "Merely quantitative differences seldom, if ever, have a perfectly simple inheritance"; and again, "It may almost be said to be characteristic of purely quantitative distinctions that one or other of the original parental types fails to reappear in its extreme form after a cross." Dr. Bateson goes on to state that, where segregation of quantitative differences appears to be complete, the quantitative difference is found to be a secondary result of some substantive factor.

Again, Dr. Huxley mentions "the finest gradation of coat colour and marking in rodents." Does Dr. Huxley seriously believe that any natural races of mice are characterised by the colours shown by these mutants? Are pure black mice (back and belly showing the same colour), chocolate, or yellow mice ever found in nature as natural species? The question answers itself—the colours of natural races show the general features of the coat-colour found in the wild mouse, and of no other.

Let me answer two further objections before passing on to the next point. Dr. Huxley states that I "leave out of account all reference to modifying factors." Now, it is possible to "explain" any given difference between two types as due to Mendelian factors, provided we assume a

sufficient number of hypothetical factors, and explain every failure to segregate as due to the interference of factors.

Thus extended, the Mendelian hypothesis becomes totally indifferent to the facts; whichever way these turn out, it may be made to fit by making some additional assumptions, and it is impossible to devise a crucial experiment. This is the pass to which some of the factor-mad pupils of Prof. Morgan have reduced the theory, and at this point it ceases to interest the serious biologist, whether he label himself geneticist or not.

Again, although I do not believe that the mutations experimented with by Mendelians have played any part in evolution, I should be the last to "belittle" the importance of their discoveries. They are, I think, of prime importance for the human race where our outlook is limited to two or three generations, and where the efforts of our sickly sentimentalists to keep all our Mendelian defectives alive, and encourage them to breed, are threatening the future of our race.

Turning now to the second point, viz. Dr. Huxley's criticisms of the evidence which I brought forward in favour of Lamarckism, the impression made on my mind is that he must be in a state of profound ignorance of the whole subject. Thus, he states that my argument about lens determiners recalls the "delicious blends of thirty years ago—when Weismann was to the fore." But this case was raised by me to show the untenability of the Weismannian theory, which is at bottom identical with the Mendelian theory. If I turn to Goddard, *Feeble-mindedness, its Causes and Consequences*, a book which contains *the* Mendelian discovery of most practical importance yet reached, we find that he speaks of "determiners," and of the large number of these "determiners" which must be present in the spermatozoon. Yet Dr. Huxley states that Mendelians prefer to talk in terms of "organic equilibrium"! He further states that the abnormal formation of the lens "is infinitely harder to explain on Lamarckian principles."

Now, "organic equilibrium" is a physiological conception, and, may I add, a Lamarckian one, and the aberrant formation of the lens is just what we should expect if Lamarckism were true. For, according to Lamarckian theory, organs arise as a response by the organism to the demands of the environment. But the environment is internal as well as external, and one organ within the body is, so to speak, an environment for another, and should exercise a modifying influence on it. Then he says that my statement that "it is by no means easy to find on it an animal which will respond to a change of the environment by a change of structure" detracts from the importance of Lamarckism as an evolutionary factor. Now, Lamarck expressly states that the changes which he postulates will only become apparent after a long time, *i.e.* after many generations. It is a matter of great luck that in Amphibians we seem to have a stock sufficiently plastic to show an observable difference in two or three generations.

All our available evidence goes to prove that the evolutionary process was extraordinarily slow. My colleague, Lord Rayleigh, tells me that he has determined the *minimum* age of some Oligocene beds from their helium contents, and it turns out to be 30,000,000 years! and yet all this time was required to convert the three-toed Mesohippus into the one-toed horse.

Lastly, Dr. Huxley attacks the relevance of the experiments recorded in Guyer's work to the point at issue. Their relevance is strongly asserted by Guyer himself, who points out that they afford an explanation of the diminution of the eyes of cave-animals. The fact that the original vestigial lens was produced by a lens-destroying serum, and that such an occurrence is not met with in Nature is totally irrelevant. The important point is that, once the defective lens was produced, it was propagated through several generations—without any further use of serum. The influence of the soma on the germ, which Dr. Huxley states that no "learned biologist" would

deny, is as a matter of fact denied by all the prominent Mendelians of my acquaintance, and it is the crux of the whole Lamarckian doctrine. No doubt in nature changes in the sizes of organs are due to adaptation of function, but to observe these we should need in most cases to be endowed with the years of Methuselah, and the brilliant result obtained by Guyer opens up a short cut which enables us to see the change in a few generations.

I should like to say, in conclusion, that I, for one, am strongly disposed to believe in the chromosome mechanism for segregation, and further to believe in the hypothesis put forward by Bateson in 1914, that the cause of Mendelian "mutations" is probably to be found in irregularities in the separation of sister chromosomes, so that one daughter cell receives too little chromatin, and becomes the parent of the mutant. Further, it is obvious that before any functional adaptation can become firmly engrained in the hereditary constitution it must affect the constitution of the chromosomes. But I repeat that the mutations due to abnormalities of division appear to me to have played no part in the evolutionary process, which, there is every warrant for believing, must have been slow, functional, and continuous.

Yours faithfully,
E. W. MacBride.

February 7, 1921.

THE GEOLOGICAL AGE OF THE PALÆOLITHIC FLINT IMPLEMENTS

I. From J. Reid Moir, F.G.S., F.R.A.I.

Dear Sir,—In Science Progress for January 1921 (No. 59, pp. 387–9) there are published some brief notes upon some of the papers appearing in the *Proceedings of the Prehistoric Society of East Anglia* (vol. iii, pt. 2). The writer of these notes refers especially to the Presidential Address delivered by Prof. J. E. Marr, F.R.S., and to a paper from the pen of Mr. M. C. Burkitt. Both these papers deal with the question of the geological age of the palæolithic flint implements, and their relationship to the East Anglian glacial deposits. And in both papers an attempt is made to correlate these deposits with those existing in the Alps and described by Penck (*Die Alpen im Eiszeitalter*). It is now many years since similar attempts were made, and those of us who are dealing with this question make no claim to be the first to have published speculations upon the possible contemporaneity of the English glacial beds with those upon the Continent of Europe. But I think I may accept the responsibility for having been the first person to publish a detailed account of the glacial and interglacial deposits (and their contained flint implements) of East Anglia, and their possible relationship to others described by Penck. My correlation, based upon numerous discoveries made by me in East Anglia was first made public on January 17, 1920, and appears in the number of the *Proceedings of the Prehistoric Society of East Anglia* (vol. iii, pt. 2, pp. 238–43) dealt with by the writer of the anthropological notes in Science Progress.

Prof. Marr's Presidential Address (*Proc. P.S.E.A.*, vol. iii, pt 2, pp. 177–90) was first made public on March 17, 1920, and Mr. Burkitt's paper (*Proc. P.S.E.A.*, vol. iii, pt. 2, pp. 311–14) was read on April 12, 1920. In both these papers the authors fully acknowledge my work, which formed in part the basis of their respective correlations. I notice, however, that in the notes in Science Progress my researches—no doubt through inadvertence—are not mentioned, and I feel that, altogether apart from any personal motive, this should be rectified.

I would wish also to comment very briefly upon the note (Science Progress, No. 59, January 1921, p. 389) dealing with Prof. Sollas's paper

on "A Flaked Flint from the Red Crag," which specimen was found by me and presented to Prof. Sollas. As your reviewer states, the latter investigator regards the flint as having been flaked, not by man, "but by some sub-human tool-making animal." This, however, in view of the remarkable and skilful manner in which the sub-Crag implements were fashioned, appears to me to be improbable. But I appreciate, nevertheless, Prof. Sollas's true scientific attitude in accepting as artificially made certain flaked flints which he formerly claimed as of natural origin—and especially his generous references to my work which appear in the footnote to his paper.

J. Reid Moir.

February 3, 1921.

II. From A. G. Thacker, A.R.C.Sc.

Sir,—In regard to Mr. Reid Moir's comments on my notes in Science Progress for January 1921, dealing with the correlation of British glacial and interglacial deposits with Penck's well-known scheme, I beg to state that my omission to refer to the correlation published by Mr. Moir in January 1920 was due to an oversight. Mr. Moir published this correlation at the end of an article dealing with another (and less general) subject, and I had not noticed that he had dealt with this question of correlation. If I had noticed it I should certainly have referred to his scheme. My attention had been especially directed to Professor Marr's and Mr. Burkitt's articles. Both these writers refer with just appreciation to Mr. Moir's work in East Anglia, but Professor Marr refrains from making a detailed correlation between the British and Continental Strata, and, although Mr. Burkitt publishes such a detailed scheme, he happens not to refer to the earlier and totally different scheme published by Mr. Moir. Attempts have, of course, been previously made to construct Pencko-British correlations, notably by Professor James Geikie. And the suggestion made by Mr. Moir that the Chalky Boulder Clay is of "Riss" age is also not new—the dating of the Chalky Boulder Clay being the chief key to the whole problem. But Mr. Moir's scheme is based upon the most recent work—very largely his own work—and is very detailed; and he is entitled to claim originality for it.

I am, sir,

Yours faithfully,

February 8, 1921. A. G. Thacker.

THE BRITISH COMMITTEE FOR AIDING MEN OF LETTERS AND SCIENCE IN RUSSIA

Sir,—We have recently been able to get some direct communication from men of science and men of letters in North Russia. Their condition is one of great privation and limitation. They share in the consequences of the almost complete economic exhaustion of Russia; like most people in that country, they are ill-clad, underfed, and short of such physical necessities as make life tolerable.

Nevertheless, a certain amount of scientific research and some literary work still goes on. The Bolsheviks were at first regardless, and even in some cases hostile, to these intellectual workers, but the Bolshevik Government has apparently come to realise something of the importance of scientific and literary work to the community, and the remnant—for deaths among them have been very numerous—the remnant of these people, the flower of the mental life of Russia, has now been gathered together into two special rationing organisations, which ensure, at least, the bare necessities of life for them.

These organisations have their headquarters in two buildings, known as the House of Science and the House of Literature and Art. Under the former we note such great names as those of Pavlov the physiologist and Nobel Prizeman, Karpinsky the geologist, Borodin the botanist, Belopolsky the astronomer, Tagantzev the criminologist, Oldenburg the orientalist and permanent secretary of the Petrograd Academy of Science, Koni, Bechterev, Latishev, Morozov, and many others familiar to the whole scientific world.

Several of these scientific men have been interviewed and affairs discussed with them, particularly as to whether anything could be done to help them. There were many matters in which it would be possible to assist them, but upon one in particular they laid stress. Their thought and work is greatly impeded by the fact that they have seen practically no European books or publications since the Revolution. This is an inconvenience amounting to real intellectual distress. In the hope that this condition may be relieved by an appeal to British scientific workers, Prof. Oldenburg formed a small committee, and made a comprehensive list of books and publications needed by the intellectual community in Russia if it is to keep alive and abreast of the rest of the world.

It is, of course, necessary to be assured that any aid of this kind provided for literary and scientific men in Russia would reach its destination. The Bolshevik Government in Moscow, the Russian trade delegations in Reval and London, and our own authorities have therefore been consulted, and it would appear that there will be no obstacles to the transmission of this needed material to the House of Science and the House of Literature and Art. It can be got through by special facilities, even under present conditions.

Many of the publications named in the Oldenburg list will have to be bought, the costs of transmission will be considerable, and accordingly the undersigned have formed themselves into a small committee for the collection and administration of a fund for the supply of scientific and literary publications, and possibly, if the amount subscribed permits of it, of other necessities, to these Russian savants and men of letters.

We hope to work in close association with the Royal Society and other leading societies in this matter. The British Science Guild has kindly granted the Committee permission to use its address.

We appeal for subscriptions, and ask that cheques should be made out to the Treasurer, C. Hagberg Wright, LL.D., and sent to

The British Committee for Aiding Men of Letters and Science in Russia,
British Science Guild Offices,
6, John Street, Adelphi, London, W.C.2.

Faithfully yours,

Montagu of Beaulieu
Ernest Barker
E. P. Cathcart
A. S. Eddington
I. Gollancz
R. A. Gregory
P. Chalmers Mitchell
Bernard Pares
Arthur Schuster
C. S. Sherrington
A. E. Shipley
H. G. Wells
A. Smith Woodward
C. Hagberg Wright

ESSAY-REVIEWS

HIGHWAYS AND BYWAYS IN THE THEORY OF NUMBERS, by L. J. Mordell (Manchester College of Technology): on **The History of the Theory of Numbers,** vol. ii, **Diophantine Analysis,** by Leonard Eugene Dickson, Professor of Mathematics in the University of Chicago. [Pp. xxv + 803.] (Washington: Carnegie Institution of Washington, 1920.)

All mathematicians interested in the Theory of Numbers, and this means sooner or later most pure mathematicians, will welcome vol. ii of Prof. Dickson's "Chronological History." It notes practically everything written on the subject, sums up the results of a paper in a few lines, and might serve as a model of orderly arrangement.

This history adds considerably to the increasing debt of mathematicians to America, and is a real necessity in their libraries. Writers will hereafter frequently refer to it rather than give a large list of references—a tendency already noticed with respect to vol. i. He will be a rash investigator, indeed, who does not consult it. The energy and untiring patience devoted to his task by Prof. Dickson will save his readers endless trouble. He has placed them under the deepest obligations, and indeed has set an example that may well be followed in other branches of mathematics. The results in the Theory of Numbers are now being made so accessible that the next thirty years should see even more progress than the preceding period, during which numerous theorems of outstanding importance have been proved.

Vol. ii, dealing with such a large number of famous questions, in many of which mathematicians of the present day are displaying considerable interest, and some of which are known even to the layman, will soon convince the reader that the Theory of Numbers is still of supreme importance. It is concerned with the solution of diophantine equations—that is, to find the rational values of $x, y, z \dots$ satisfying the equation

$$f(x, y, z \dots) = m \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where f denotes a polynomial in $x, y, z \dots$ and m is given. The term "diophantine equation" (so called after Diophantus, who flourished about A.D. 250) has been frequently used when integer values only are required for the unknowns, the term indeterminate, when rational solutions are desired; but writers have not always adhered to this distinction. It is unimportant in discussing the general equation (1), but, for a non-homogeneous equation, the distinction between rational and integer solutions usually is of considerable importance.

We shall now indicate the relation between the study of the integer solutions of equation (1) and some branches of the Theory of Numbers, as given in this book. When f is a binary quadratic, the solution of

$$ax^2 + 2bxy + cy^2 = m$$

contains practically the elements of the Theory of Numbers as developed by Gauss. When f is a quadratic form in n variables, it gives rise to the arithmetical theory of the general quadratic form as developed by Gauss, Eisenstein,

Hermite, Smith, and Minkowski. The author, however, finds it convenient in this volume to consider only special, though very important, cases of these quadratic forms, such as, for example, the solution of

$$x_1^2 + x_2^2 + \ldots x^2_n = m$$

for different values of n, or, again, of

$$x^2 + y^2 + z^2 = f^2, \text{ etc.}$$

When f is a linear function we have the theory of partitions, a subject noteworthy for the part played both in the past and the present by English mathematicians, among whom may be mentioned Cayley, Sylvester, Glaisher, and MacMahon. There are also developments by Gauss, Eisenstein, Dirichlet, Smith, Frobenius, and Minkowski, which have been of the greatest importance in the development of the arithmetical theory of the general quadratic form, and of the arithmetical properties of algebraic numbers.

Another very important section arises from Fermat's Last Theorem, concerning the impossibility of

$$x^n + y^n = z^n$$

in rational numbers when n is a positive integer greater than 2.

Many an unsophisticated writer, taking an equation of this type, say

$$x^3 + y^3 = z^3,$$

that is

$$(x + y)\,(x + \rho y)\,(x + \rho^2 y) = z^3,$$

where ρ is a complex cube root of unity, argues at once, by analogy from elementary arithmetic, that $x + \rho y$, for example, is the cube of a similar expression. The simple example

$$(2 + \sqrt{-5})\,(2 - \sqrt{-5}) = 3^2$$

where $2 \pm \sqrt{-5}$ are certainly not the squares of, nor have, for a common factor, expressions of the form $a + b\sqrt{-5}$, where a and b are integers, may show that the properties of ordinary integers cannot be applied to algebraic numbers without investigation. Indeed, it has been the great merit of Fermat's Last Theorem that it has directed the attention of mathematicians to the study of the arithmetical properties of algebraic numbers. In this way, Kummer found that the equation was impossible if n was an odd prime not dividing any of the numerators of the first $\frac{1}{2}(n - 3)$ Bernoullian numbers. From some of his results, it has been shown in recent years that if n is a prime and the equation has solutions for which all the unknowns are prime to n, then $q^{n-1} - 1$ is divisible by n^2 for

$q = 2, 3, 5, 11, 17$, and also for
$q = 7, 13, 19$, if n is of the form $6N + 5$.

Since the prize of 100,000 marks was established in 1908 for a proof of Fermat's Last Theorem, it has become very widely known, about 3,000 efforts, mostly futile, being published within three years of the announcement of the prize. Much labour and paper would be saved if all future aspirants consulted Prof. Dickson's volume; but this is too much to hope for.

There is also Waring's Problem, dating from 1770 and proved by Hilbert in 1909, that for a given positive integer n and for any positive integer m, an integer r can be found independent of m, so that

$$x_1^n + x_2^n \ldots x_r^n = m$$

can be solved in positive integers; and, to mention only one other, the question of finding asymptotic formulæ not only for the number of solutions of the equation (1), but also of finding the total number corresponding to $m = 1, 2 \ldots N$.

It must not be supposed, however, that the subject deals only with theorems that require the attention of great mathematicians. The reader soon notices some familiar questions, never out of date, whose antiquity will surprise him. To mention only a few, 1669 is the date of the problem, "Anna took to market 10 eggs, Barbara 30, Christina 50. Each sold a part of her eggs at the same price per egg, and later sold the remainder at another price. Each received the same total amount of money. How many did each sell at first, and what were the two prices?"

An older one, due about 1220 to Leonardo Pisano, is to find a square which, when either increased or decreased by 5, gives a square; while a still older type dates from a Chinese work of about the first century A.D., giving a rule for determining a number having the remainders 2, 3, 2, when divided by 3, 5, 7 respectively.

Topics such as those previously mentioned have led to investigations and results of the greatest importance, even in apparently unconnected subjects. Considerable progress has been made by methods which are really arithmetical in spirit, although apparently transcendental in character. At the present time, however, equation (1) naturally suggests the study of functions defined by the series

$$\phi(q) = \Sigma q^{f(x, y, z \ldots)},$$

or, again,

$$\chi(s) = \Sigma [f(x, y, z \ldots)]^s,$$

where the summation refers to integer values for $x, y, z \ldots$.

Indeed, for many equations (1), the arithmetical development of the theory has been carried furthest when the function $\phi(q)$ has been most studied. Only in a few cases, such as, for example, Fermat's Last Theorem or when f is an indefinite quadratic form, is it greatly in advance of the analytical theory. Thus there is now no theoretical difficulty in finding the number of solutions of

$$x_1^2 + x_2^2 + \ldots x_r^2 = n,$$

(or even of

$$f(x, y, z \ldots) = m,$$

where f is a definite quadratic form in n variables), as the function $\phi(q)$ is a well-known modular function for which there exist other expressions as a power series in q, and from which the required number of solutions at once follows.

When the properties of the function are not so well known, another method has proved very successful lately in the hands of Hardy, Littlewood, and Ramanujan. The unit circle is usually a line of essential singularities for the function $\phi(q)$. Taking the singularities given by $e^{2\pi i a/b}$, where a and b are integers prime to each other, it is a simple matter to construct a function $\psi(q)$ with these singularities, and so that the function $\phi(q) - \psi(q)$ will not have such heavy singularities as the original function $\phi(q)$. The question then arises, Is it likely that the coefficients of the expansion of $\psi(q)$ in ascending powers of q are an approximation, at any rate for large values of m, to the corresponding coefficients in the expansion for $\phi(q)$? These writers have established this possibility in a number of cases by means of contour integration—a method which has already established the truth of the famous prime number theorem. The first of two cases which we shall mention, is to find an approximate formula for the number of partitions of m, *i.e.* for the number of solutions in positive integers of

$$x + 2y + 3z + \ldots = m,$$

or the coefficient of q^m in the expansion of

$$1/(1 - q)(1 - q^2)(1 - q^3)(1 - q^4) \ldots .$$

The approximate formulæ discovered are so accurate that for $m = 200$, the coefficient—a number of thirteen figures—is found with an error of 0·004. The second is Waring's Problem, where the error introduced by taking the approximation suggested is of smaller order for large values of m than the approximation, thus proving the theorem for large values of m, and also finding a rough estimate of the number of nth powers required.

It is because of the fact that new functions and new questions are so easily suggested in the Theory of Numbers that it is so intimately associated with many branches of pure mathematics. But, while it is easy enough to suggest a problem, the solution is usually a matter of great difficulty; the arithmetical and analytical difficulties attending each new type of question being such as to require a new chapter in mathematics. A simple illustration will make this clear.

Consider first the equation

$$x^2 + y^2 = z^2$$

of which rational solutions were known to the Egyptians nearly 2,000 years B.C., and of which rational solutions involving a parameter were known to Pythagoras and Plato. The general solution of this equation in co-prime integers can be written as

$$x = p^2 - q^2, y = 2pq, z = p^2 + q^2,$$

and was practically known by Diophantus, and explicitly by the Indian mathematicians about 600 A.D.

Take next the equation

$$x^3 + y^3 = z^3,$$

(or, for that matter, say

$$x^3 + 7y^3 = z^3$$

where an obvious solution is $x = 1$, $y = 1$, $z = 2$), which it seems likely must have been discussed by mathematicians before the time of the Arab Alkhodjandi, who gave a defective proof of its impossibility at some date before A.D. 972. For a long time, many persons must have wondered if such equations could be solved in a similar manner by means of parameters p, q. Their efforts, of course, would have been in vain, as the introduction of elliptic functions, certainly one of the intellectual achievements of the nineteenth century, makes almost obvious. Their difficulties may illustrate one still facing mathematicians after several hundred years.

Consider the equation

$$y^2 = ax^2 + bx + c,$$

which includes as a special case the famous equation miscalled the Pell Equation, which has led Kronecker to surprising relations between the number of classes of binary quadratics and elliptic functions. There is no difficulty in finding not only all the integer solutions, but also tests for deciding if the equation is possible. Take now the equation

$$y^2 = 4x^3 - g_2x - g_3,$$

and practically the same argument applies to the equation

$$y^2 = ax^4 + bx^3 + cx^2 + dx + e$$

$$\text{or to } \phi(x, y, z) = 0$$

if ϕ is a ternary cubic in x, y, z. If we know one set of rational values for x, y, to which we shall refer as the rational point $P(x, y)$ of the curve

$$y^2 = 4x^3 - g_2x - g_3,$$

it is clear that in general we can find another rational point Q from the other

intersection with the curve of the tangent at the point P. Similarly, we can proceed with the point Q; or, again, we might take the third point of intersection of the line PQ and the curve. Hence, in general, an infinite number of rational solutions can be found from one known one.

The analytical interpretation is also very simple. The co-ordinates of any point on the curve can be written as

$$x = \wp(u), y = \wp'(u)$$

in the usual notation of elliptic functions. But the point $(\wp(nu), \wp'(nu))$ is also on the curve, and if n is an integer, $\wp(nu)$ and $\wp'(nu)$ can be expressed rationally in terms of $\wp(u)$ and $\wp'(u)$, giving in general an infinite number of solutions corresponding to $n = 1, 2, 3 \ldots$.

The question, of course, arises, "Can we find thus all the rational solutions from an initial one?" For some equations this can be done, as was proved by Sylvester, but it does not appear to be true in general. In any case, how can we find an initial solution, let alone a general one? The prospect of any immediate solution of such problems appears almost as remote now as that of discovering any knowledge concerning the chemical constitution of the stars must have appeared, say, in 1800. Perhaps our difficulty is comparable with that of the persons who, in the Middle Ages, tried to solve

$$x^3 + y^3 = z^3$$

by means of parameters.

Some account of indeterminate equations of the types just mentioned, was usually given in Algebras published some 125 years ago. The classical example is the Algebra of Euler, to whom Diophantine Analysis is so much indebted. The subject seems to have gone out of fashion; perhaps because many of its devotees have overlooked its connection with the Theory of Numbers, and have concerned themselves with particular solutions of isolated problems from which little advance in knowledge could be expected. This is to be regretted, as few things are more productive of astonishment, and more stimulating to the student, than to be brought quickly and intelligently to an impenetrable mathematical frontier.

The reader will now see, not only interesting results by Sylvester, Pepin, and Lucas, but also that the general rational solution of the ternary cubic $f(x, y, z) = 0$ requires a knowledge of only one solution, and the rational solution of

$$y^2 = 4x^3 - g_2x - g_3,$$

where g_2 and g_3 are numerical multiples of the fundamental invariants of the cubic. Further, despite an assertion by Fermat that seems almost to the contrary, the equation

$$y^2 = x^3 + k$$

has at most a finite number of integer solutions if k is given. (The same theorem applies to the equation

$$ey^2 = ax^3 + bx^2 + cx + d$$

where a, b, c, d, e are given, and the right-hand side has no squared factors in x.)

For equations of higher degrees, the difficulties are considerably greater. Thus, for

$$y^2 = ax^5 + bx^4 + \ldots$$

then, even if one rational solution is given, no method is known whereby from it we can find another. Nevertheless, there are some very noteworthy results for equations of higher degrees, such as the one by Thue, that if $f(x, y)$ is an irreducible binary quantic of degree greater than the second, then the equation

$$f(x, y) = k$$

has at most a finite number of integer solutions. There is also another by Hilbert and Hurwitz that all the rational solutions of $f(x, y, z) = 0$, a homogeneous equation representing a curve of genus zero, can be found by solving equations of the first and second degrees.

We shall conclude by mentioning one other question of the many to be found in Dickson, namely, that the sum of three biquadrates is never a biquadrate, that is, there are no rational solutions of

$$x^4 + y^4 + z^4 = t^4$$

apart from the obvious ones for which two of the unknowns are zero. Euler stated 150 years ago that this theorem was hardly to be doubted, but he could not prove it. The difficulty of the question may be gauged from the fact that Prof. Dickson gives only three references to other writers.

The above account may give the reader some idea of the contents of Prof. Dickson's volume, and convince him that he should lose no time in acquiring so valuable a guide to the highways and byways of the Theory of Numbers.

THE COMPLETION OF THE PUBLICATION OF THE COLLECTED PAPERS OF THE LATE LORD RAYLEIGH, by Prof. Alfred W. Porter, D.Sc.: on **Scientific Papers**, by John William Strutt, Baron Rayleigh, O.M., D.Sc., F.R.S. [Vol. VI, pp. xvii + 718.] (Cambridge: at the University Press, 1920. Price 50s. net.)

In this sixth volume appear all the papers of the late Lord Rayleigh published in the period 1911–19, together with two papers which were left ready for the press, but were not sent to any channel of publication until after the author's death. Those which were not printed off until after Lord Rayleigh's death have been carefully revised by Mr. W. F. Sedgwick, late Scholar of Trinity College, Cambridge, who has added footnotes to elucidate doubtful or obscure points in the text.

To look back upon Lord Rayleigh's work is to survey the developments of mathematical and experimental physics during the last fifty years. While there are subjects which exerted a special attraction to him, the chief characteristic of his writings is the great range that they cover. Of the grand total of 446 communications, 97 are contained in the present volume. A classified index to all six volumes at the end groups the papers according to subjects, and in this brief survey of the last instalment it will be convenient to keep to the same classification. When a paper falls into more than one group any reference to it will be made only in connection with the subject to which it in the main belongs.

This procedure practically excludes special reference to mathematics. With Rayleigh, mathematics was always looking forward to its applications. His general attitude is well illustrated by No. 404, "on Legendre's function $P_n(\theta)$ when n is great and θ has any value." After pointing out that Hobson has developed the complete series proceeding by descending powers of n, not only for $P(n)$, but also for its associated functions, he adds: "The generality aimed at by Hobson requires the use of advanced mathematical methods. I have thought that a simpler derivation, sufficient for practical purposes and more within the reach of physicists with a smaller mathematical equipment, may be useful." He proceeds to supply this derivation by simple methods of successive approximation. By acting in this and other ways as the intermediary between the rigour of mathematics and its employment by the physicist, he has often earned the gratitude of the latter, though, at the same time, he may have frequently incurred the displeasure of the mathematician.

Under the heading General Mechanics are papers on the Sand-blast and on the cutting and chipping of glass. Both of these illustrate the interest he felt in simple processes. In the former (No. 386), after quoting far-reaching statements of Thomas Young[1] and Osborne Reynolds,[2] he proceeds to show, by an application of the method of dimensions, that when two spheres collide the maximum strain is independent of the linear scale, and if rupture depends only upon the maximum strain it is as likely to occur with small spheres as with large ones. The most interesting case is when one sphere is very large relatively to the other, as when a grain of sand impinges upon a glass surface. If the velocity of impact is given the glass is as likely to be broken by a small grain as by a much larger one. But he explicitly recognises that this conclusion would be upset if rupture depends upon the *duration* of a strain as well as upon its *magnitude*. In the paper on glass (No. 417) he asks for information on the theory of the cutting of glass by a diamond in addition to that given in a very suggestive statement by Wollaston[3]; though at the same time he throws out the warning "that it may be as well to remember W. Taylor's saying that everything calculated by theorists is concerned with what happens within the elastic limit of the material, and everything done in the workshop lies beyond that limit." It appears that "the force transmitted across internal surfaces parallel to the external surfaces is a *pressure* all along, but the force transmitted in a perpendicular direction, although at first a pressure, at a very small distance below changes to a *tension*, which soon reaches a maximum and afterwards gradually diminishes. I suppose it is this tension which determines the crack. . . ." With regard to the chipping of glass by a film of gelatine allowed to dry on it, surprise is expressed that a film of gelatine, scarcely thicker than thick paper, should be "able to tear out fragments of solid glass, but there is no doubt of the fact."

On the *theory* of Capillarity there is practically nothing in this volume. This is the more to be regretted because, in his earlier contributions, while he wrote interestingly and in an illuminative way on the theory as founded by Laplace, he never advanced beyond the consideration of continuous (*i.e.* non-molecular) media. The Dutch School has made a tremendous advance by recognising that the centres of molecules never approach to less than a diameter apart, measured from centre to centre. Some of the integrations which are made in calculating the capillary constant from the attractions between molecules require, in consequence, a lower limit equal to this diameter instead of zero. The consequence is that Rayleigh's objection to a power law for this attraction, that it involves infinite values for all powers, breaks down completely. Objection can only be raised to inverse powers equal to or less than five. It would seem (from a letter from him to the present writer) that he was rather oppressed by a sense of the difficulties in treating the molecular dynamics of liquids in an adequate fashion. His only contribution in this volume is contained in a paper on the lubricating and other properties of thin, oily films (No. 429). Recalling the probability that the point at which surface tension begins to fall corresponds to a thickness of a single layer of molecules (given first *Phil. Mag.*, vol. xlviii, p. 321 (1899)), he states: "It seems pretty clear that, from pure oil, water will only take a layer one molecule thick. But when oleic acid is available, a further drop of tension ensues. The question arises, how does this oleic acid distribute itself? Is it in substitution for the molecules of oil or an addition to them constituting a second layer? The latter seems the more probable. Again, how does the impurity act when it leads the general mass into the unstable flattened-out form? In considering such questions Laplace's theory is of

[1] *Natural Philosophy* (1807).
[2] *Phil. Mag.*, vol. xlvi, p. 337 (1873).
[3] *Phil. Trans.*, 1816, 265.

little service, its fundamental postulate of forces operating over distances large in comparison with molecular dimensions being plainly violated."

A communication to *Nature* on Breath-figures (No. 353) is delightfully characteristic of the author's love of examining into familiar phenomena. "One of the first things is to disabuse our minds of the idea that anything wiped with an ordinary cloth can possibly be clean." "Quincke employed hot sulphuric acid." "An even better treatment is with hydrofluoric acid, which actually renews the surface of the glass. . . . The parts so treated condense the breath in large laminæ, contrasting strongly with the ordinary deposits (*i.e.* in lens-shaped drops)."

A difficulty arises in the comparative permanence of breath-figures which often survive wiping with a cloth. "The thought has sometimes occurred to me that the film of grease is not entirely superficial, but penetrates in some degree into the substance of the glass. . . . We know but little of the properties of matter in thin films, which may differ entirely from those of the same substance in mass."

Attention may also be called to the theory of the capillary tube (No. 399).

Thirty-four articles on Hydrodynamics form a group surpassing the limitations of a short review. Thirty-two articles (many of them the same) appear under Sound. Amongst those of chiefly theoretical interest is one on Æolian Tones (No. 394). Many are of very great practical importance in connection with the developments of aerial flight. The characteristic feature is the recourse to the theory of dimensions in all these problems. This is indicative of the backward state of the dynamical theory of viscous fluids. A special article in *Nature* on the principle of similitude (No. 392) summarises what can be proved by means of this principle. This article gave rise to some discussion in subsequent numbers of *Nature*, especially in connection with heat problems when the temperature is taken as a thing *sui generis*. A different set of "dimensions" is obtained if temperature is regarded as measuring the mean kinetic energy of agitation of the molecule. The probability is that the last possible word was not said in this discussion. It would appear that two systems are not completely dynamically similar unless at least the temperature is made to vary in the appropriate manner. In this case no duplicity in sets of dimensions would be obtainable.

The principle of similitude is applied in No. 420 to elucidate the laws governing the flow of heat from moving liquids to solids. Considering a liquid enclosed between two parallel plates, one of which is fixed while the other moves parallel to its own plane with a given velocity, v, he shows that the heat, H, transmitted per unit area per unit time is given by—

$$H = \frac{k\theta}{a} F\left(\frac{av}{\nu}, \frac{c\nu}{\kappa}\right)$$

where a is the distance between the plates, θ the difference of temperature between them, ν the kinematic viscosity of the liquid, c the thermal capacity per unit volume, κ the thermal conductivity, and F stands for any function of the two fractions inside the brackets. For a given fluid $c\nu/k$ is a constant and may be omitted. Dynamic similarity is attained when av is constant; so that a complete determination of F does not require the variation of both a and v. There is an advantage in keeping a constant, for otherwise any roughness of the surfaces would have to be changed in proportion. The chief object of the paper was to show that the principle assumed by Osborne Reynolds and others (that the passage of heat from solids to liquids moving past them is analogous to that governing the passage of momentum) is not always true. A term in the equation for the fluid motion involving drop of pressure in the fluid is absent from the thermal equation. It is pointed out, however, that the analogy may hold for *average* values of

the tangential traction and of the passage of heat. The question whether this is so or not is left open, but it is declared not to be probable.

The papers on Sound teem with suggestive experiments and observations, whether they are dealing with the character of the S sound, æolian tones, fog-signals, the perception of sound, the resonant reflexion of sound from a perforated wall, or the propagation of sound in water. Take the paper on the last subject for example (No. 414), which summarises in a succinct way the chief effects of the free surface, distinguishing between this case and that of reflexion from a wall in air.

In connection with Thermodynamics there occur only four papers, but they are of great interest. In a letter to Nernst (No. 356) he returns to the question of the failure of the law of equipartition of energy. "Perhaps this failure might be invoked in support of the views of Planck and his school that the laws of dynamics (as hitherto understood) cannot be applied to the smallest parts of bodies. But I must confess that I do not like this solution of the puzzle. Of course I have nothing to say against following out the consequences of the [quantum] theory of energy. . . . But I have a difficulty in accepting it as a picture of what actually takes place." This was written in 1911.

In a paper (No. 379) on the pressure of radiation and Carnot's principle he shows in detail, by means of a Carnot's cycle in which radiation is the substance operated with, that the second law must be violated if no radiation-pressure exists. Bartoli's proof "employs irreversible operations," and "does not lend itself to further developments."

Article 418 contains a timely exposition of the La Chatelier-Braun principle so much employed by physical chemists. "As usually formulated, the principle is entirely ambiguous." "Nothing definite can be stated without a discrimination among the parameters by which the condition of a system may be defined." "Returning to the compressed gas, we now recognise that it is the pressure, δp, which is the force and $-\delta v$ the effect." "But we may still feel a doubt as to which is the strained condition, the isothermal or the adiabatic, and without a decision on this point no statement can be made." It is, however, evident that if the general theorem is applicable at all, the adiabatic condition must be regarded as the constrained one, since the response is to be *diminished* by a constraint.

The volume contains a large number of papers in the region of Optics. Two are on the propagation of waves in stratified media. In the second (No. 422) the case is taken of a succession of equally spaced parallel plates of equal thickness—a succession of Haidinger plates, in fact. This differs from the problem of a pile of plates discussed by Stokes, inasmuch as attention is paid to phase as well as amplitude. The reflexion coefficient for a single plate being r (where r is a complex quantity such that multiplied by its conjugate the reflected *intensity* is obtained), and the transmission coefficient is t, then it is shown that for any number, n, of plates the corresponding quantities are—

$$\text{for reflexion } \phi_n = \frac{(p+1)(q+1)\{p^n - q^n\}}{r\{(q+1)p^n - (p+1)q^n\}}$$

$$\text{where } p + q = r^2 - t^2 - 1 \text{ and } pq = t^2,$$

and for transmission

$$\psi_n^2 = 1 - \phi_n \frac{r^2 + 1 - t^2}{r} + \phi_n^2.$$

The problem is worked out on account of its application to the light reflected from certain chlorate-of-potash crystals which are multiple-twinned. For favoured wave-lengths it is shown that the reflected light is 73 per

cent. of the incident light if there are no more than thirty-two alternations of crystal directions. But the problem is of greater importance at the present time, because it gives a good illustration of the general effect of multiple reflexions in connection with X-rays and crystal structure. It must be admitted that there are considerable differences between the two phenomena. Whereas in the case of chlorate-of-potash crystals and ordinary light a ray reflected from a lower layer is bound to be caught by an upper layer, yet it is otherwise with X-rays. Owing to the shortness of their wave-length, there is much more chance of a wave passing through the interspaces between the atoms without being caught. Nevertheless, a careful perusal of Rayleigh's problem is exceedingly instructive in connection with applications to X-rays.

At various times photographic questions attracted Lord Rayleigh. In No. 359 he takes up the general problem of photographic reproduction, with suggestions for enhancing gradation originally invisible. The original object is itself taken to be a transparency, the fraction of light transmitted at a given point being t (*i.e.* the transparency). Similarly, the transparency of the negative is t', and since this transparency depends upon the light that had acted upon the negative we may write $t' = f(t)$. When the operation is repeated, using the negative as "source," and a positive is taken, the transparency of the last t'' is given by

$$t'' = f(t').$$

Complete photographic reproduction may be considered to demand that at every point $t'' = t$. This requires that if the relation between t and t' be written $F(t,t') = 0$, then F must be a symmetrical function of t and t'. So far all is beyond question; at any rate, if the negative and positive are on the same kind of plate and are similarly developed. In the particular examples Rayleigh does not hit on cases that approach sufficiently near to practical conditions to be of use. The question has recently been thrashed out in greater detail, with a close approach to practical applications.

In Article No. 381 Rayleigh takes up the case of the diffraction of light by (dielectric) spheres of relative refractive index differing little from unity. The object of the fresh attack was to extend the calculations to particles larger, compared with the wave-length, than had previously been done. Unfortunately, they are limited, for simplicity, to cases for which the dielectric constant of the particles differs little from that of the surrounding medium. The present writer had previously shown experimentally that if bright light is examined through a sulphur suspension set free from a "hypo" solution by adding acid, the light becomes redder and weaker in the early stages, (forming an artificial setting sun), but later on, if observations are continued, the transmitted light grows again in intensity and is now deep blue, gradually changing to green, greenish-white, and white. No explanation was forthcoming from Rayleigh's calculations; nor does it seem likely that an increase in the value of the assumed index would materially alter the character of the results. Calculations which are now being made by Captain Talbot Paris (and which are not yet published) indicate that the explanation is to be found in another direction.

In No. 427 the remarkable colours, variable with the angle of observation, which are so frequent in beetles, butterflies, and feathers are stated to be due *probably* to a periodic structure in the diffracting particles, so that the dielectric constant has a different value at different distances from their centres. Reference may also be made to No. 438 on the optical character of some brilliant animal colours.

Of the papers indexed under the head of Miscellaneous, the one to which this title fits best is Rayleigh's Presidential Address to the Society of Psychical Research in 1919 (No. 443). The following extracts illustrate

his attitude to the questions discussed by that Society. Speaking of spirit-writing:

"A discouraging feature was that attempts to improve the conditions usually led to nothing. As an example, I may mention that after writing, supposed to be spirit-writing, had appeared, I arranged pencils and paper inside a large glass retort, of which the neck was then hermetically sealed. For safety this was placed in a wooden box, and stood under the table during several séances. The intention was to give opportunity for evidence that would be independent of close watching during the semi-darkness. It is perhaps unnecessary to say that, though scribbling appeared on the box, there was nothing inside the retort. Possibly this was too much to expect. I may add that, on recently inspecting the retort, I find that the opportunity has remained neglected for forty-five years."

"A real obstacle to a decision arises from the sporadic character of the phenomena, which cannot be reproduced at pleasure and submitted to experimental control. The difficulty is not limited to questions where occult influences may be involved."

He then goes on to give examples of similar difficulties in connection with scientific matters, such as meteorites, globe-lightning, will-o'-the-wisp, etc.:

"To my mind, telepathy with the dead would present comparatively little difficulty when it is admitted as regards the living. If the apparatus of the senses is not used in one case, why should it be needed in the other?"

"Some of the narratives that I have read suggest the possibility of prophecy. This is very difficult ground. But we live in times which are revolutionary in science as well as in politics. Perhaps some of those who accept extreme 'relativity' views, reducing time to merely one of the dimensions of a four-dimensional manifold, may regard the future as differing from the past no more than north differs from south. But here I am nearly out of my depth, and had better stop."

In this brief review only a few of the papers in the sixth volume have been referred to. Other writers would probably have made a different choice. The papers in this volume form a noble completion to Lord Rayleigh's life's labours. Although he founded no school in the same sense as Newton or Faraday or Fresnel or Maxwell, he has so enlarged our knowledge of physical science in all its branches as to stand out as one of the leaders in scientific achievement.

REVIEWS

MATHEMATICS

A Course of Modern Analysis. An Introduction to the General Theory of Infinite Processes and of Analytic Functions; with an account of the Principal Transcendental Functions. By E. T. WHITTAKER, Sc.D., F.R.S., Professor of Mathematics in the University of Edinburgh, and G. N. WATSON, Sc.D., F.R.S., Professor of Mathematics in the University of Birmingham. Third Edition. [Pp. 608.] (Cambridge: at the University Press, 1920. Price 40*s*. net.)

THERE are few changes in the new edition of this book. It still holds the field as the repository of information about the transcendental functions for the applied mathematicians, and it has come to be considered as a standard reference book of the methods of the theory of functions. It is at once plain that any book which achieves both these admirable objects only manages it through a perpetual struggle in the mind of the authors. There are evidences of this struggle throughout the book. It is not impossible that a further edition might most profitably introduce sweeping changes in the direction of redrafting the initial chapters (and in particular the very first, which is not up to the level of the rest of the book) in the interests of the mathematician, and developing in a somewhat gentler and less aristocratic manner the vagaries of the transcendental functions and integral equations, Fourier Series, and other mathematical phenomena which are so often a dark puzzle (and a painful one) to physicists who, for no fault of their own, sooner or later have to deal with them. The rearrangement of the chapter on Fourier Series, which is a step in this direction, is therefore to be welcomed.

An interesting chapter on Lamé's functions, which is entirely new and has not appeared in the previous editions, renders accessible much of the recent work on ellipsoidal harmonies which sooner or later must be of considerable importance in mathematical physics.

D. M. WRINCH.

ASTRONOMY

Easy Lessons in Einstein: A Discussion of the more Intelligible Features of the Theory of Relativity. By EDWIN E. SLOSSON, M.S., Ph.D. [Pp. vii + 128.] (London: George Routledge & Sons; New York: Harcourt, Brace & Howe, 1920. Price 5*s*. net.)

THIS book attempts the impossible. There is no easy path to the mastery of Einstein's theory, even for the skilled mathematician. For the layman, devoid of the training and equipment of the mathematician, an intelligible appreciation of the theory only can be obtained by hard study and careful thought. The present volume will not prove of much assistance: it talks round the theory in a very disjointed manner, and gives the impression that the author himself has not fully grasped its fundamental bases. It abounds with inaccuracies and loose statements, *e.g.* " Galileo showed, when he dropped, his big and little cannon-ball off the Leaning Tower of Pisa, all bodies fall with the same speed." Velocity and acceleration are here confused, yet the author considers himself competent to expound generalised relativity!

The printing of V–1 for $\sqrt{-1}$ on p. 59 will not prove very intelligible to a lay man. In the footnote to p. 74, *ds*2 is printed for ds^2, etc., and $1-\frac{2m}{v}$ for $1-\frac{2m}{r}$, the author appearing to have quoted formulæ which he does not understand. These are but a few examples, chosen from amongst many.

The style in which the book is written is irritating: such sentences as "Suppose yourself a worm—the Bible says you are, anyway"; "Astronomers, indeed, say that we are moving at tremendous speed towards Canis Major—in other words, that the world is going to the dogs," are typical.

Readers who require an elementary but accurate account of Einstein's theory should leave this book alone and study the English translation of Einstein's own elementary exposition (reviewed in a previous number) or one of the other available volumes by an author of repute. H. S. J.

From Newton to Einstein. Changing Conceptions of the Universe. By BENJAMIN HARROW, Ph. D. [Pp. x + 95.] (London: Constable & Co., 1920. Price 2*s.* 6*d.* net.)

THIS volume does not claim to be an exposition of Einstein's theories. It consists of a brief review of the conceptions of gravitation and the æther, from the time of Newton to the present day, described in a readable manner and free from serious inaccuracies. Newton's work is reviewed in some detail: an account is then given of Huyghen's wave theory of light and its triumph over the corpuscular theory. It involved the introduction of the conception of the æther, which was strengthened by the epoch-making electromagnetic theory of light, enunciated by Clerk-Maxwell. The modern developments—the electron structure of matter, and the connection between electrical and optical phenomena and the motion of electrons—are described. This leads to the question whether, if matter and light have the same origin, and if matter is subject to gravitation, why not light also? So the author comes to the 1919 eclipse expeditions and thence to Einstein's theory and some of its consequences. The essay should prove of interest to the lay-reader whose curiosity has been aroused by the prominence accorded in the Press and elsewhere to Einstein's theory. H. S. J.

Geodesy, including Astronomical Observations, Gravity Measurements, and Method of Least Squares. By GEORGE L. HOSMER, Associate Professor of Topographical Engineering, Massachusetts Institute of Technology. [Pp. xi + 368, with 115 figures in text.] (New York: John Wiley & Sons; London: Chapman & Hall, 1919. Price 18*s.* 6*d.* net.)

THIS introductory textbook on geodesy will prove valuable to English students of the subject. For some reason or other, the study of geodesy has been much neglected in this country, and there is no concise, complete and up-to-date textbook by an English author available. The neglect of the subject at the Universities in this country is probably responsible for the state of affairs, which the projected foundation of a Geodetic Institute at Cambridge—if realised—may do much to remedy.

The author has struck a happy compromise between theory and practice. Whilst the practical details are carefully explained (the methods used in the U.S. Coast and Geodetic Survey being generally followed), the underlying principles are clearly elucidated. The whole ground of geodesy is covered in a concise manner—triangulation, gravity measurements, precise levelling, and figure of the earth. The astronomical observations required for the determinations of longitudes, latitudes, and azimuths are explained in sufficient detail. In connection with this, no reference has been made to the use of wireless time signals for the determination of longitudes. This is a serious

omission, as it is undoubted that their employment by survey parties will soon become general, much time spent in observation and computation being saved.

One chapter is devoted to the properties of the spheroid, and the methods of calculation of triangulation and geodetic positions are dealt with. The problem of the adjustment of triangulation is dealt with in some detail, and is preceded by a brief account of the method of least squares. This is based upon the probability aspect and is clear and concise, though the proof of the error law is not rigid. It may be noted that there is an error in the second of equations (147), for v_2 in the denominators of the differentials should be read z_2; in addition, the right-hand sides of the equations have been omitted.

The chapter on map projections is the least satisfactory one in the book. The fundamental principles of projection are not clearly brought out, nor are the relative advantages of the several projections which are dealt with sufficiently explained. The chapter as it stands would not enlighten very much a student approaching the subject for the first time.

At the end of the volume is given a series of formulæ, numerical constants, and tables which will prove valuable for reference purposes.

H. S. J.

The Gyroscopic Compass. A Non-mathematical Treatment. By T. W. CHALMERS, B.Sc., A.M.I.Mech.E. [Pp. x + 167, with 51 figures.] (London: Constable & Co. Price 11*s*. net.)

THE contents of this book originally appeared as a series of articles in *The Engineer* in the early part of 1920. The articles were intended to give a clear account of the theory and construction of the gyroscopic compass, without the introduction of mathematics. The author has succeeded admirably, and the volume is a model of how an abstruse and difficult subject can be treated in an elementary manner without loss of accuracy or ignoring difficult points.

The gyro-compass has become a necessity on the modern battleship, owing to the difficulty of ensuring that a magnetic compass will be unaffected by the large masses of iron and steel around it. The three existing patterns of gyro-compass—the German Anschütz, the American Sperry, and the British Brown compass—were developed primarily for naval purposes. The gyro-compass is, however, certain to have a large future before it in the mercantile narine. The initial cost is somewhat heavy, but in the Preface to this volume it is stated that " remarkable figures were shown to the author recently which demonstrated that not only was navigation by the gyro-compass much more accurate than by the magnetic compass, but that the increased accuracy reduced the length of the voyage of a mercantile vessel to an extent that resulted in saving a quantity of fuel, the value of which on a single trip would go a considerable way towards meeting the extra first cost of a gyro-compass." This book will help the navigating officer to understand the principles upon which this type of compass is based.

The treatment throughout is sufficiently detailed for easy reading. The phenomena of the gyroscope and their application in the gyro-compass are described. The several errors to which the compass is liable (latitude error, north steaming error, ballistic reflection, and quadrantal error) are fully dealt with, and the manner in which they are eliminated in the three types of compass is explained. The principal features of the three patterns and the methods of damping the vibrations and eliminating fiction are described.

There was no detailed account previously available dealing with the gyro-compass in an elementary, non-mathematical manner, and the present volume is for this reason the more welcome. It should do much to remove the unjustifiable prejudice which exists in some quarters against the gyro-compass.

H. S. J.

PHYSICS

Ions, Electrons, and Ionising Radiations. By J. A. Crowther, Sc.D. [Pp. xii + 276, with 95 diagrams.] (London: Edward Arnold, 1919. Price 12*s*. 6*d*. net.)

As it is stated in the preface, the book "is not a popular exposition of the 'new physics,' nor is it a compendious synopsis of the whole subject," but a textbook for the student grounded in the more elementary portions of physics.

The subject-matter begins with the passage of a current through an ionised gas. Methods of measuring the current are clearly described, a discussion on the relative sensitiveness of the electrometer and the electroscope being particularly useful and practical. The classical researches of Sir J. J. Thomson, C. T. R. Wilson, Millikan, and Perrin, on the charge carried by an ion, are fully considered. The spark discharge is dealt with somewhat mathematically, but the subsequent chapter on the phenomena of the discharge tube is fascinating reading. Cathode and positive rays receive full deliberation, as also the emission of electricity by hot bodies and photo-electricity.

In a chapter on X-rays one finds that their penetrating power "depends principally upon density," which is rather a loose statement. Further, the modern demand for the "radiator" Coolidge tube rather contradicts the statement that "no cooling device is necessary" with this type of tube. To maintain the historical sequence one would have thought that the work of Barkla on the absorption of X-rays and the production of the characteristic radiations would have preceded the work of Lane and Sir W. H. Bragg and his son. A fuller description of Moseley's experiments would have been appreciated.

Nearly a quarter of the book deals with radio-activity. The classical experiments illustrating the nature of the L-rays are clearly described, though Becquerel's experiment disclosing the nature of the B particles has not received the same lucid treatment. Gamma-rays, and the secondary B-rays produced by their impact with matter, are reviewed somewhat rapidly, but the theory of radio-active change is given at length, and a chapter dealing with some problems in radio-activity is interesting.

The last section of the book, dealing with the electron theory of matter, begins with a résumé of the results of the experiments throwing light on the nature of matter. The Zeeman effect, metallic conduction, and the laws of Wiedemann and Franz, are discussed and explained by the theory.

Throughout the book the diagrams are numerous, simple, and clearly drawn. The two plates showing C. T. R. Wilson's "cloud" photographs and positive ray parabolas are a valuable addition to the text. Graphs illustrating experimental and theoretical results are abundant, though one regrets that the curves, for the K and for the L series, showing the proportionality of the atomic number to the square root of the frequency, are missing. In the main, the book justifies the claim of the author in that it is a "reasonably complete account of the present state of the subject," and is a valuable addition to physical literature.

L. H. C.

CHEMISTRY

A Treatise on Chemistry, Vol. I, The Non-metallic Elements. By the Rt. Hon. Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. Fifth Edition, revised by Dr. J. C. Cain. [Pp. xv + 968, with 226 illustrations and a portrait of John Dalton.] (London: Macmillan & Co., 1920. Price, 30*s*. net.)

Since the appearance of the fourth edition of Roscoe and Schorlemmer's *Treatise*, chemistry has suffered a severe loss by the death of Sir Henry Roscoe, and by a sad chance the issue of the present (fifth) edition of vol. i

has coincided with the death of Dr. J. C. Cain, to whose labours the revised edition is due; it represents therefore a last service rendered to his science by one who in many capacities, not least as editor of the Chemical Society's *Journal*, gave all his life and energy to the welfare and progress of the profession to which he had devoted himself.

It was perhaps a fortunate matter that Dr. Cain was already associated with Prof. Roscoe in the earlier edition of the book, so that it was possible to preserve the general character and style, and chemists will feel they have in the present work a memento both of author and reviser.

For many reasons one hesitates to criticise so old a friend as "Roscoe and Schorlemmer," particularly at the present moment; it will suffice to say that the reviser has obviously given much care to bringing the book up to date by the insertion of suitable references to recent work, but one is nevertheless left with the impression that it is perhaps a case of trying to put new wine into old bottles.

Possibly what is wanted in a future edition is a group of chemists working under the guidance of a general editor so that less important details may be removed to give place for more modern matter, otherwise there is a considerable danger of the book becoming more a history of twentieth-century progress in chemistry than a reliable textbook of inorganic chemistry; this applies particularly to such matters as ammonia, nitric acid, sulphuric acid, and to many of the illustrations.

The printing and general arrangement of the treatise continue, as might be expected, to be a model for chemical publications, though the purpose of the "unopened" pages is not very clear; they are more favoured by the bibliophile than by the chemist.

The fact that the book has reached a fifth edition is a proof that there is always a demand for a really readable textbook of inorganic chemistry, and chemists will acknowledge with gratitude and respect the debt they owe both to the late author and the reviser.

F. A. Mason.

Practical Chemistry: Fundamental Facts and Applications to Modern Life. By N. H. Black, A.M., and James B. Conant, Ph.D., Assistant Professor of Chemistry, Harvard University. [Pp. x + 474, with 251 figures and a coloured plate.] (New York: The Macmillan Co., 1920. Price 11*s*. net.)

The title of this book is somewhat of a misnomer, as one might be led to expect a practical laboratory manual; and although a large number of experiments are described in a general way, the scope of the book is considerably wider than this.

The essential purpose of the book is to provide an introductory textbook of chemistry with a view to interesting the student in the subject by showing its wide ramifications and its infinite number of points of contact with daily life.

The authors have found two ways of awakening the interest of students: first, by appealing to their love of the miraculous and the dramatic; and second, by touching their curiosity concerning the things of everyday life. With this in view they have outlined many lecture-table experiments, often of a spectacular nature, and wherever possible examples of chemical phenomena have been drawn from the student's daily experience.

There can be no doubt that by this means the subject is made essentially "live," and the interest can be fixed of many a student who otherwise would take but a perfunctory interest in chemistry.

The illustrations, which are clear and well chosen, vary from diagrams of laboratory appliances to an aerial photograph of a poison-gas attack or the photo of a Permutite water-softening plant, so that all tastes are catered for.

One cannot but think that books of this type will do much more for the encouragement and popularisation of chemistry among the general population than the more academic textbooks of a decade or two ago, and will thus help to create and perpetuate that atmosphere of respect and sympathy for chemistry in which alone a development of pure science can be hoped for.

The book is well printed and illustrated, and it is flattering to our national self-esteem to notice that among the nineteen portraits of distinguished chemists, six are of British nationality, Germany coming second with three, whilst France, Sweden, America, and Russia have two each.

Teachers of chemistry in search of a new textbook will doubtless welcome the present work.

F. A. M.

An Intermediate Textbook of Chemistry. By Alexander Smith, Head of the Department of Chemistry, Columbia University. [Pp. vi + 520, with numerous diagrams and 3 plates.] (London: G. Bell & Sons, 1920. Price 8*s*. 6*d*. net.)

Prof. Smith is well known as the author of several textbooks of chemistry, and the present work follows more or less the usual lines.

It is described as Intermediate because it is longer than the earlier "Elementary Chemistry" and shorter than the "College Chemistry."

The book covers most of the fundamental facts of the science and contains a well-balanced mixture of descriptive matter and theory, so that the student who assimilates its contents should have a very fair knowledge of general and inorganic chemistry.

The attempt has been made to bring the subject-matter up to date, so that we have sections dealing with Jean Perrin's work on the Brownian movement, with Moseley's atomic numbers, the fixation of nitrogen and so on. In connection with the last-named, however, it is somewhat curious that no mention is made of the synthetic production of ammonia from nitrogen and hydrogen, nor to the preparation of nitric acid by the oxidation of ammonia. Surely both processes have long reached a stage at which their incorporation, even in an elementary textbook, should go without saying.

There are one or two errors here and there, such as the equation on p. 239, and "Dr. Luke" on p. 296 should surely be "Dr. Lane"?

The use of the expression "inhomogeneous" is not in accordance with the usual nomenclature which favours the word "heterogeneous."

F. A. M.

Nucleic Acids: their Chemical Properties and Physiological Conduct. (Second edition.) By Walter Jones, Ph.D., Professor of Physiological Chemistry in the John Hopkins Medical School. (*Monographs on Biochemistry*: edited by B. H. A. Plummer, D.Sc., and F. G. Hopkins, D.Sc., F.R.S. [Pp. viii + 150.] (London: Longmans, Green & Co., 1920. Price 9*s*. net.)

In the history of the development of biochemistry from a purely empirical study to an exact science the work of Prof. W. Jones on the constitution of the nucleic acids will occupy a very high place.

It is very fortunate, therefore, for biochemists and others that Prof. Jones has compiled such an excellent summary of the researches carried out by various investigators, including himself, in this very intricate subject so as to make the results readily available for others.

The progress which has been made from the days of Miescher's work on the nucleic acid of salmon spermatozoa to the latest physico-chemical investigations of Jones on the constitution and hydrolysis of the nucleotides is quite remarkable when account is taken of the extraordinary experimental difficulties in the way.

At the present moment it would seem that little more remains to be done on the purely analytical side of the problem, and future work will no doubt be in the direction of synthetic experiments.

On reading Prof. Jones's account of the history of the subject, one is impressed more powerfully than ever with the fundamental importance of the work of "pure" organic chemists, notably Emil Fischer and his work both on the sugars and on the pyrimidine group, for, without his pioneer work on subjects apparently far removed from nucleic acid, the results of later experimenters would certainly not have been possible.

The book remains much as in the former edition, being divided into two parts, dealing respectively with the chemical properties of the nucleic acids and with their physiological conduct, together with an appendix on the preparation of various compounds derived from the nucleic acids, methods of analysis, and a bibliography.

The book has been brought up to date in the light of the latest work, and the issue of a second edition shows that the work is valued by all who interest themselves in the borderland of chemistry, physics, and biology.

F. A. M.

Fuel Production and Utilisation. By HUGH S. TAYLOR, D.Sc., Assistant Professor of Physical Chemistry, Princeton University, U.S.A. (Industrial Chemistry Series. Edited by S. Rideal, D.Sc.) [Pp. 297 + xiv, with diagrams.] (London: Baillière, Tindall & Cox, 1920. Price 10*s*. 6*d*. net.)

A SMALL work on a big subject. The volume is intended more especially for the young graduate in order to supplement his academic training with the broad facts of the problem of fuel production and conservation, but, owing to the somewhat sketchy nature of some of the chapters, the work strikes one as a trifle disappointing. The chapters deal with the Direct Combustion and the Destructive Distillation of Coal, the Utilisation of Coke, Gasification of Coal, Use of Coal By-products as Fuel, and the Utilisation of Lignite, Peat and Wood as Fuel, and a section on Synthetic Fuels, together with an introductory chapter on the fundamentals of the subject.

On the whole, one is left with the feeling, after reading Dr. Taylor's book, that to do justice to the subject much more space is needed, and that an eminently "practical" subject has been dealt with in too academic a manner; whilst if it is intended merely as a short introduction to the subject for students, the essentials could have been given more concisely.

F. A. M.

Practical Biological Chemistry. By GABRIEL BERTRAND, and PIERRE THOMAS, translated by HECTOR A. COLWELL. [Pp. xxxii + 348, with illustrations.] (London: G. Bell & Sons, Ltd., 1920. Price 10*s*. 6*d*. net.)

THE translation of the third edition of the *Guide pour les manipulations de chimie biologique*, by Prof. Bertrand and M. Thomas, will be welcomed by all biochemists in search of a book which does not slavishly follow the well-trodden path pursued by most other books dealing with this subject. The book is divided into two parts, entitled Statics and Dynamics respectively. In the first part the authors describe the preparation and properties of substances taken from all the various groups and compounds of importance in biochemistry, but the particular representatives chosen are usually rather less familiar than those ordinarily described in a book of this size; thus we find, among the carbohydrates, the preparation of the hydrazone and mannose and the ozazones of galactose, xylose, and arabinose, the oxidation of xylose to xylonic acid and the preparation of saccharic and mucic acids from glucose and galactose respectively, while a short chapter is devoted to mannitol and

the preparation of two of its derivatives; and so with the other groups of compounds such as proteins and pigments, several rather out of the ordinary preparations will be found here. The book has, however, to some extent "le défaut de ses qualités" inasmuch as a good many of the more familiar though none the less important facts have of necessity been omitted, and it is difficult to avoid the impression that the book may perhaps be of more use to the teacher than to the student. Part II is devoted to a study of enzymes, microbiology, and fermentations, and likewise contains many instructive and original experiments.

It is to be regretted that the revision of the translation was not more thorough, for such terms as iodhydrate of choline iodide, cadmium xylonobromide, iodine index, etc., grate on the ears of a chemist, while the inclusion of oxalic, succinic, lactic, malic, tartaric, and citric acids under the heading of "acids of the fatty series" is distinctly misleading. Moreover, the amount of caustic potash required for making a normal solution of alcoholic potash is given on page 181 as 70 grams, while a decimal point has been omitted in the figure for a N/5 solution of sodium thiosulphate on page 132!

P. H.

Third Report on Colloid Chemistry, published on behalf of the Department of Scientific and Industrial Research for the British Association for the Advancement of Science. [Pp. 154.] (London: H.M. Stationery Office, 1920. Price 2*s*. 6*d*. net.)

FOLLOWING the plan adopted in the two preceding reports, the subject matter is arranged under two heads, viz: (1) Classification according to scientific subject, and (2) classification according to industrial process. The subjects dealt with are Colloid Chemistry of Soap, Ultra-microscopy, Electrical Charges in Colloids, Solubility of Gases in Colloidal Solutions, Imbibation of Gas, Colloid Problems in Bread-making, Colloid Chemistry in Photography, Collodion in Photography, Cellulose Esters, Colloid Chemistry of Petroleum, Asphalt, Varnishes, Paints, and Pigments, Clays and Clay Products.

P. H.

The Manufacture of Sugar from the Cane and Beet. By T. H. P. HERIOT, F.I.C. [Pp. x + 426, with illustrations.] (London: Longmans, Green & Co., 1920. Price 24*s*. net.)

THE author of this monograph is to be congratulated on having produced a book with just that blending of theory with practice which will enable the general reader to acquire a very excellent all-round acquaintance with the subject of sugar production. As stated in the Introduction, detailed description of sugar machinery have been omitted, and this will enhance the value of the book to the general reader, since there are already a number of books devoted to the detailed structure of sugar manufacturing plant. We know of no other book of corresponding size written and produced in this country which deals with the manufacture of sugar both from the cane and from the beet, and the appearance of such a book should be welcomed at this time when there is a revival of interest in the production of cane sugar within the Empire, and of beet sugar in this country. The two industries are in this book treated in separate parts or chapters, enabling the reader, if he so wishes, to follow the consecutive operations in one industry. Part I, entitled Raw Materials, is divided into four chapters, of which the first is devoted to the morphology of the cane and the beet and the physiology of sugar production within them; chapters ii and iii deal with the sugar-beet and sugar-cane respectively from the point of view of their reproduction and contain a brief account of the methods employed in producing beet-seeds; chapter iv is devoted to a brief review of other sugar-producing plants. The remaining

parts of the book are devoted to a description of the technical production of sugar in all its various stages of purity and also to its refining. The principle of the Diffusion Process is very clearly described in chapter ix, although the last paragraph on page 76 might perhaps be modified somewhat in a future edition. One of the most interesting parts of the book is the one devoted to the by-products of the two industries in which the various suggestions for the utilisation of these materials are set forth.

The last paragraph sounds a reminder which cannot be too often repeated—"The remarkable technical progress made within the past century has not been effected by the so-called practical man or sugar-producer, but by a comparatively small number of men who built the theoretical foundations upon which modern practice stands."

P. H.

Chemistry for Public Health Students. By E. Gabriel Jones, M.Sc., F.I.C. [Pp. x + 244.] (London: Methuen & Co., 1920.)

One of the main difficulties experienced by the student of Public Health Chemistry is that of disentangling the essential details of any chemical operation from the mass of detail presented to him in most of the ordinary textbooks, with the result that much time is lost. In this little book the author has, however, adopted the excellent plan of breaking up his subject-matter under headings so that all operations are treated on a uniform plan of first briefly describing the object, then giving a complete list of apparatus and chemicals required, and finally giving full and clear instructions as to the exact method of procedure. The somewhat troublesome business of interpreting the results of an analysis is very well handled throughout the book, but nowhere more ably than in the section on water, where the subject is illustrated by a number of typical analyses. The whole subject is very attractively presented, and, besides giving references to and extracts from a number of original papers and reports such as those of the Royal Commission on Sewage Disposal and the Committee for the Investigation of Atmospheric Pollution, the author has included descriptions of the processes employed in manufacturing margarine and for preserving food materials. The book deserves to be widely read by both students of public health and analysts.

P. H.

GEOLOGY

The Evolution of the Earth and its Inhabitants. By Joseph Barrell, Charles Schuchert, Lorande L. Woodruff, Richard Swann Lull, Ellsworth Huntingdon. [Pp. xi + 208, with 38 figures.] (New Haven: Yale University Press; London: Oxford University Press, 1919. Price 10s. 6d. net.)

A series of able and interesting lectures delivered before the Yale Chapter of the Honorary Scientific Society of the Sigma Xi, edited by Prof. R. S. Lull. The book is up to date, and is eminently suitable for the education of young people (and probably also of many older people) in general science. The essays deal in order with The Origin of the Earth, The Earth's Changing Surface and Climate, The Origin of Life, The Pulse of Life, Climate and Civilisation. We can find little to disagree with in these pages. Perhaps it is somewhat too much of an assumption to suppose that the gorilla has in any way degenerated from a previous type; and perhaps Dr. Ellsworth Huntingdon presses somewhat unduly his well-known theory that changes of climate have influenced progress very largely—though we are by no means in favour of the view that climate has had very little such effect. The possibility of change of climate by afforestation and deforestation appears to have been well proved both in France and in Mauritius, and there is no

reason to suppose that the climate of the Mediterranean has not been affected in a similar manner since the time of Homer, let us say. All the lectures are coloured with broad washes rather than by means of undue stippling of detail. Young people would like to have seen more drawings of extinct animals. The book is certainly an achievement on behalf of scientific education.

Coal in Great Britain. The Composition, Structure and Resources of the Coal-fields, Visible and Concealed, of Great Britain. By Walcot Gibson, D.Sc., F.G.S. [Pp. viii + 311, with 8 plates and 50 illustrations.] (London: Edward Arnold, 1920. Price 21*s*. net.)

One wonders who will really benefit by reading this book. In the Preface the author says that it " is intended to supply mining engineers, mine-owners, and mining students with a concise account of the more important facts relating to the geology of coal generally, and to the composition, structure, and resources of the coal-fields of Great Britain in particular." But a book containing innumerable facts, many of doubtful validity, which does not give a single reference to the sources of these facts cannot be truly useful to students. It is, on the contrary, a type of book injurious to the mentality of students, for it leaves them either with the idea that the author is the authoritative source of all the statements, or with vague and baseless concepts in no way connected with their real source. While it is neither necessary nor would it be advisable to quote full references for most facts mentioned, yet, in order to give students a solid ground-work, it is essential that the authority should be given for the most important facts. The references must, moreover, be more than mere names, and sufficiently complete bibliographic quotations for an intelligent student to lay his hands on the originals in a good library. To refer every now and then to papers in the *Transactions of the Royal Society*, in the *Quarterly Journal* of the Geological Society, and the memoirs of our own and foreign Geological Surveys, and the great academies, and so on, is indeed a most valuable part of the student's training. It is true that the average mining student will never himself conduct a research; but, unless he is introduced to research methods, or is, at least, made aware of the type of work on which all his knowledge is based, he will never have a thorough grasp of his subject, nor will he be able to take advantage of any exceptional opportunity which may arise by which he may himself assist in the accumulation of scientific knowledge.

This book, moreover, is far from being an immaculate guide; indeed, it is full of obscurities and inaccuracies, particularly in the earlier chapters. The later chapters dealing with the distribution of the actual coal-basins in England, the series of the seams, etc., depending more on the personal experience of Mr. Walcot Gibson (who is well known to be an experienced geologist), are much more reliable than the earlier chapters dealing with the generalities and scientific aspects of the subject, in which there is an immense amount to criticise. At the very beginning of the book, for instance, the author reveals the fact that he has not kept abreast of recent knowledge even regarding the actual appearance and outward form of coal. One could find half a dozen faults by the second page of the book. On the third page the significance of the presence of alumina in the ash of coals is minimised by the author's example—that a species of *Lycopodium* " contains as much as 6·7 per cent. of ash "—made without stating that, of the ash in some Lycopodiums, more than 40 per cent. consists of alumina salts, while the greater part of all other families in the vegetable kingdom contain no alumina at all in their ash, and the rest, with the exception of the Lycopodiaceæ, only a small percentage, generally less than two.

Then, again, the book suffers from lack of precision: *e.g.*, in the chapter dealing with " Coal as a Rock," while the volume is an account of the " Coal

in Great Britain," the sentence "coal forms a distinct layer or bed varying from a few inches to as much as 300 feet in thickness" is not annotated to explain to the presumably ignorant student that no coal in Britain approaches anything like 300 feet in thickness.

Occasionally the name of someone distinguished in coal research is mentioned, but never with any reference to indicate to a reader where to find his work. As a rule, names which were famous half a century ago rather than those of any recent workers are cited. For instance, in Chapter III, the old (and one thought now quite defunct) controversy over *in situ* and "drift" origin of coal is rehashed, and the statement which hardly anyone of authority or experience would agree with is made that, "In this country . . . . due mainly to the teaching of Logan, de la Bêche, and Principal Dawson, a coal-seam is considered to represent an original peat-bog." Dawson, who was most active in the sixties of last century, should, at least, have had coupled with his name some palæobotanist who has done more recent research.

It seems a pity to rattle old bones over this so foolish controversy, but if it *must* be disinterred (one thought even its ghost was laid by the clear and simple fact that some seams were grown *in situ* and the accumulations forming others were drifted) the arguments used on either side should be expressed precisely or the student will be left in a fog.

Such a sentence as that "the condensed vegetable pulp quickly hardened or passed into coal . . . is shown by the not infrequent occurrence of angular or rounded pebbles of coal embedded in the coal-measure sandstones of most coal-fields" occurs on page 25. What is the student, uninstructed save by this book, and not referred to more precise authority, to assume is meant by the word "quickly"—a season? a decade? a century? a hundred thousand or a million years? In relation to the vast extent of the coal measure deposits half a million years might very well elapse between the formation of one accumulation and its break-up to form actual pebbles for the higher measures. While, of course, no one can say what periods of time are represented between such beds, still the teacher should offer some guide to the student regarding the possible limits of the meaning of the word "quickly" used in such context.

Looseness of thought degenerates into absolute inaccuracy in the sentence: "The increase in weight due to the conversion of wood into coal, etc." During the conversion of wood into coal by the various organic processes which took place, wood actually *lost* gaseous vapour and portions of its organic molecules; hence there can be no *increase in weight* when wood is converted into coal, but, on the contrary, a very material decrease in weight. Owing to the pressure on the mass, there is an increase in specific gravity; but the accurate discrimination between "weight" and "specific gravity" is a lesson for elementary school-children, and mining students should not be involved in their confusion.

After these criticisms we must close with a few words of appreciation of the chapter dealing with the practical prospecting and boring, which is quite useful, and of the later part of the book in which the clear maps and diagrams and the correlation of the various seams will be useful to many.

BOTANY

General Botany for Universities and Colleges. By Prof. H. D. Densmore, M.A. [Pp. xii + 459, with 289 illustrations.] (London and New York: Ginn & Co., 1920. Price 12*s*. 6*d*. net.)

As in most textbooks on Botany that have recently appeared, the author has adopted a functional treatment of structure. Starting with the relation of plants to their environment, the student is led to consider their structure

and anatomy, and this in turn is followed by sections dealing with physiology and reproduction.

The chief plant groups are illustrated on the type system, which portion of the work occupies about a quarter of the text. The last section treats of various families of flowering plants, and there is an excellent chapter on plant-breeding. There is a recognition of the economic aspects of the subject that one would wish to be emphasised in all elementary textbooks.

Having regard to the length of the book, the field covered is a wide one, though this has resulted in a somewhat cursory treatment of certain aspects, of which morphology is perhaps the most noticeable. The mere statement that the terminal buds of *Ulmus* are replaced by lateral ones is scarcely adequate guidance in what the student usually finds a difficult conception, and the treatment of rhizomes, bulbs, etc., which can be made to teach important lessons, is reduced to a minimum. In describing the differentiation of the cell wall, the impression created of a uniform deposition of cellulose is only corrected at a much later stage, and the statement that vacuoles in the cytoplasm are water-drops (p. 47), though amplified some four pages farther on, is calculated to perpetuate an all too common error.

If the method of presentation in parts leaves something to be desired, the matter is written in an interesting style, with a freshness of expression which is particularly pleasing. The illustrations, too, are good, and the diagrams especially exhibit a quite unusual standard of conception.

E. J. S.

Water Plants : A Study of Aquatic Angiosperms. By Agnes Arber, D.Sc., F.L.S. [Pp. xvi + 436, with frontispiece and 171 text figures.] (Cambridge: at the University Press, 1920. Price 31*s*. 6*d*. net.)

Whilst dealing with an ecological group of plants, Mrs. Arber has employed them, not for study as a plant community, but as a conveniently limited field for the study of morphological and evolutionary problems.

Like Mrs. Arber's previous publications, the presentation is characterised by marked lucidity and felicity of expression, which should go far to commend it even to amateur botanists.

In the first section the chief biological features of aquatic plants are exemplified by means of a selection of typical life-histories, which prepares the way for the subsequent matter on the vegetative and reproductive organs. In this latter the authoress upholds the view that the submerged leaf type is the juvenile and more primitive one, capable of being induced, however, by conditions of malnutrition. Such an interpretation is in harmony with the views of de Candolle and Henslow regarding the nature of the monocotyledonous leaf, views which have been elaborated by Mrs. Arber and which are set forth on these pages.

In considering the anatomical structure of the leaf, it may be pointed out that the facts adduced with regard to the straight and wavy epidermal walls of the submerged and aerial leaves of *Callitriche*, though not explicable on the hypothesis quoted, are quite consistent, as also those of sun and shade plants, with the mechanical hypothesis of Haberlandt, to which, however, no reference is made.

Stress is pertinently laid on the necessity for recognising that lack of lignification in the xylem vessels is no index of their incapacity to conduct water, and a useful summary is furnished of work on the transpiration stream in submerged species.

It is noted as a curious fact, " as yet unexplained," that the water pores of aquatics are often highly ephemeral. This feature is by no means confined to aquatics, and it may be suggested that there, as in terrestrial plants, the water pores probably have as their chief function the prevention of excessive pressure in the juvenile stages of leaf development.

The third section treats of the physiological relations of water plants, and includes a chapter of eight pages on their ecology. This latter is indeed the least satisfactory in the whole work. We cannot but deprecate the suggestion that research in the exploitation of any field of knowledge is the exclusive prerogative of any investigators having one particular view-point. That branch of knowledge is greatest which is most truly the servant of others, and the function of each is to supply inspiration to add to the sum of human learning. When, therefore, Mrs. Arber writes that the contributions of ecology to the study of aquatics have not been of first-rate importance, one cannot but note the large number of references in the less strictly ecological matter, to work such as that of Gluck, which had its inspiration in the ecological view-point. We note, too, no mention of Pearsall's important contribution to the rôle of sedimentation in determining the distribution of aquatics, though the paper itself is quoted in another minor connection.

In treating of the distribution of aquatics in this country, we are surprised to find no mention of the influence of our elaborate canal system, which formed an unbroken connection between the waterways of the north and those of the extreme south. Also, in considering the effect of buoyancy and its absence, the transporting action of currents during periods of spate must surely be of importance even to seeds of high specific gravity.

Mrs. Arber repudiates the view that aquatics are the remnants of a decadent and defeated race retreating before the pressure of competition; rather are they the pioneers suited by their constitution to this particular habitat.

Emphasis is laid on the important part which the Helobieæ play in giving an apparently prevalent aquatic habit to the monocotyledons as a whole. The primitive character of this cohort and its characteristically swollen hypocotyl are held to be the determinants of its marked hydrophytism.

In summarising the bearing of aquatics on evolutionary problems the inheritance of acquired characters is held to be "an almost inevitable article of belief if it is understood *in a broad and general sense*."

There is appended an extensive bibliography in which a novel feature is the notes indicative of the chief contents of the papers cited.

Botanists and naturalists generally should feel indebted to the authoress for having brought together a large mass of scattered information, compiled in a manner which renders the dullest subjects interesting.

E. J. SALISBURY.

British Plants. By J. F. BEVIS, M.A., B.Sc., and H. J. JEFFEREY, A.R.C.S., F.L.S. 2nd Edition. [Pp. xii + 346, with 115 figures.] (London: Methuen & Co., 1920. Price 7*s*. 6*d*. net.)

As indicated by the sub-title, the aim of the authors has been to treat of British plants from the standpoint of their ecology and biology. Beginning with a general treatment of the factors of the environment, occupying about one-third of the text, the authors pass on to consider the morphology, physiology, and biology of plants in general, thus leading up to the final chapters, in which the plant communities of the British Isles are briefly dealt with.

The chief modification of the original edition is the addition of about twelve pages of appendix devoted to Weismann's law of heredity, the Mendelian theory, botanical provinces, and a short bibliography. The chapters on ecological matters could with advantage have been drastically revised. Most field botanists will learn with surprise that *Iris fœtidissima* is commonly found on shifting dunes, whilst the inclusion of *Ulex europæus* or *Teucrium*

scorodonia in the vegetation of a shingle beach gives quite a wrong impression of its characteristic flora.

In the general conception there is much to be commended, as, for instance, the inclusion of sections dealing with the soil, and the origin of the British flora; but in the execution there is much to be desired.

E. J. S.

Symbiosis. A Socio-Physiological Study of Evolution. By H. Reinheimer. (Pp. xii. + 295.) (London: Headley Brothers, 1920.)

Mr. Reinheimer's work is frankly unorthodox, both in subject and treatment, and therefore in evaluating the thesis which he presents one must be careful to avoid prejudice, due to a not unnatural irritation at his methods of presentation.

The value of such works mainly depends on the fresh view-point from which ascertained facts are considered, but is necessarily determined by their proper and complete comprehension.

The title itself is somewhat misleading since the term Symbiosis is given a much wider meaning beyond relations involving organic connection.

The author presents us with a biological philosophy based on the assumption that success in the evolutionary process is dependent on the measure of the organism's contribution to the biological community as a whole. There is much that might be advanced in support of certain aspects of this view, and in so far as the author lays stress on the reciprocal relations of organisms, the warp and woof of the organic world which is so frequently overlooked, we are at one with him. When, however, we come to the basis of Mr. Reinheimer's hypothesis it resolves itself into a question of the superiority, if any, of *Cystococcus* symbiotic, over *Cystococcus* growing freely.

When we turn to the evidence adduced we find that the facts are often only partially apprehended. For example, though it is apparently true of the great majority of species that cross-fertilisation is beneficial, and often essential, the fact is ignored that there is a minority, for which self-fertilisation is equally necessary, whilst some of the most successful species and genera at the present day are even apogamous. Again, the statement that "Mendelism has shown that under domestication factor after factor is lost," is no argument for the ill effects of cultivation since wild races, equally, show variations of the same type as those here referred to.

The view that predatory animals or plants are more subject to parasitism than others will scarcely bear a moment's investigation, whilst it is an equal perversion of the facts to state that all plants whose pollen causes hay-fever are useless weeds.

It is largely on such half-truths, with their pernicious verisimilitude, that facile generalisations are based, which the author claims are thereby well substantiated.

E. J. Salisbury.

An Introduction to the Structure and Reproduction of Plants. By F. E. Fritch, D.Sc., Ph.D., F.L.S., Professor of Botany, East London College, University of London, and E. J. Salisbury, D.Sc., F.L.S., Lecturer in Botany and Fellow of University College, University of London. [Pp. viii + 458, with 230 figures and illustrations.] (London: G. Bell & Sons, 1920. Price 15*s*. net.)

This volume is a sequel to the authors' well-known *Introduction to the Study of Plants*. The latter work did not treat of the minute structure of plants nor of the details of their life-history. The book before us may be considered supplementary in that it treats more particularly of these aspects of the subject. It is divided into two parts, "The Anatomy of Plants" and "The

Life-histories and Reproduction of Plants." In the latter the type system has been purposely abandoned, the authors preferring to give the learner a broader and more general account of the groups by describing a comparatively large number of forms selected with a view of giving an idea of the "range of form and reproductive methods within each group." Evolution and heredity are considered at greater length and from a wider view-point than is usual in a book of its scope; the exposition of this difficult subject, moreover, is admirable. There is also a special chapter devoted to the ecological anatomy of plants. The physiological aspects of structure are continually placed before the reader, both in the special and in the more general part, a chapter, for example, being devoted to a consideration of the special physiology of the Fungi, Lichens, and Bacteria. Another and a very commendable feature is that the economic uses of plants have been touched on at appropriate places. The work is admirably illustrated with a number of reproductions from photographs and drawings taken from specimens of well-known and easily obtainable British plants. An Appendix on the Microscope and its uses, and on elementary technique, a Bibliography of the most important works on various branches of the subject, and a very full Index are provided. The authors are to be heartily congratulated on their achievement. The book can be strongly recommended.

E. M. C.

A Textbook of Plant Biology. By W. Neilson Jones, M.A., F..S., Professor of Botany in the University of London and Bedford College, and M. C. Rayner, D.Sc., late Lecturer in Charge, Department of Botany University College, Reading. [Pp. viii + 262, with 36 figures and 6 plates.] (London: Methuen & Co., 1920. Price 7*s*. net.)

This book is put forward as a supplement to the general elementary textbook. The authors point out that the more extended study for which the elementary work prepares the student is often not pursued; and it is their hope to provide the student "with a general grounding in biology" and "biological principles." The three main divisions treat of the plant as a machine, reproduction, and the plant in relation to the outside world respectively. The first section is practically the physiology of nutrition; the second, the methods of reproduction, outlines of classification, and genetics. The third part is concerned with plant movements, ecology, and the soil. Each chapter is provided with a series of illustrative practical exercises.

The book, in fact, seems to be practically a textbook of plant physiology with a biological basis. Considerations of expense and the need for curtailing size have reduced the number of illustrations to a minimum. In view of the fact that different students using this book will in all probability not be provided with the same general textbook on Botany, and will, therefore, have different requirements in the way of extra illustrations, it seems a pity that the work could not have been self-contained as far as illustrations were concerned: this would have added greatly to the utility of the book.

E. M. C.

ZOOLOGY

The Influence of Man on Animal Life in Scotland: A Study in Faunal Evolution. By James Ritchie, M.A., D.Sc., F.R.S.E., Assistant Keeper in the Natural History Department of the Royal Scottish Museum. [Pp. xvi + 550, with 90 figures and 8 maps.] (Cambridge: at the University Press, 1920. Price 28*s*. net.)

The title of this volume is somewhat deceptive. The book, true enough, deals with animal life in Scotland, and very comprehensively, but in effect its scope is much farther reaching than its title suggests. It is stimulating

in its treatment, and not only cites many instances of animal development and repression gleaned from other countries, but throughout impresses the reader with the fact that the author is dealing with world-wide principles and problems.

The author is to be congratulated on successfully having undertaken an enormous task. The general plan of the volume is best illustrated by a citation of the chapter headings, of which there are nine, the first introductory, the last conclusive, and the remainder being grouped, four and three respectively, in still larger divisions, Part I dealing with man's deliberate interference, and Part II with his indirect interference with animal life. The four chapters of Part I bear the following legends—" The Domestication of Animals "; " Deliberate Destruction of Animal Life "; " Protection of Animal Life "; " Deliberate Introduction of New Animals ": those of Part II—" The Destruction of the Forest "; " Influence of Cultivation and Civilisation "; " Animals Introduced Unawares." All of these are subdivided still further into sections, Chapter V, for instance, including three under the headings " Animals Introduced for Utility," " for Sport," " for Amenity." It will thus be readily seen that the book deals with all possible phases of the main subject in a comprehensive, scholarly, and able manner. There is, moreover, an excellent and complete index.

The maps form an important feature of the book, graphically illustrating such features as the shrinkage in the distribution of the red deer, the spread of the squirrel, or the influence of man-made obstructions on salmon rivers. Of other illustrations many are good, but some are so poor that they might well have been omitted.

This authoritative treatise should find a place in every reference library.

W. R.

Wasp Studies Afield. By Phil and Nellie Rau. [Pp. 372, with 68 figures.] (Princeton: University Press, N.J. Price $2.00 net.)

This is an interesting little book, dealing mainly with the field habits of some of the solitary wasps. There is a single chapter on certain sociable species. Altogether some sixty species are dealt with, the habits of the majority being here described for the first time.

A large portion of the book goes to the description of experiments carried out to test the homing instincts, etc., of various selected wasps. In conclusion, there is a chapter, entitled " General Considerations," devoted to summarising and discussing the results obtained. There is a short but fairly complete index.

The book is written in a rambling style, but makes very interesting reading and supplies the wasp specialist with a great deal of new information.

W. R.

ARCHÆOLOGY

Pre-Palæolithic Man. By J. Reid Moir, F.R.A.I. [Pp. 67, with 29 plates.] (Ipswich: W. E. Harrison.)

Of the original work in prehistoric anthropology which has been carried on during the last ten years, none has been more important than that of the author of this book. As is now well known to all those who follow the advances of natural science, Mr. Reid Moir has been studying for years certain flints which are found below the Red Crag of East Anglia, which are therefore of Pliocene date, and are well known as Rostro-Carinate implements. Mr. Reid Moir has always believed that these peculiar flints were artificially chipped, and he has succeeded in convincing the majority of those competent to form an opinion that some, at least, of the specimens

really are artefacts. The author has published a large number of highly interesting scientific papers on these particular flints, and also on cognate matters, and he has now published his chief arguments and conclusions, which are of a most far-reaching character, in book form. The greater part of the matter contained in the present volume has been published previously in scattered articles, and the book bears the obvious marks of its origin from separate essays. The chapters do not fit very well into any connected plan, and there is a certain amount of repetition. Moreover, as the book was presumably intended for the educated public generally as well as for professed anthropologists, we think it would have been made more useful and instructive to the former class of reader if a certain amount of preliminary explanatory and non-controversial matter had been included, perhaps in the shape of an introduction. The book, as a book, is not good ; but any shortcomings which it may have in this respect are quite overshadowed by the importance and extraordinary interest of the subjects with which the separate chapters deal.

The first chapter deals with flint fracture and flint implements, the second with the oldest flint implements, the third with the relationship of the most ancient flint implements to the later river-drift palæoliths, the fourth with the ancestry of the Mousterian flint implements, the fifth with " A Rostro-Carinate flint implement," and the sixth (and last) with pre-palæolithic man in England. There is much in nearly all these chapters which will be instructive even to those who have followed the advances of prehistoric anthropology, and by far the greater part of the work is entirely original; and indeed the only places where the author does not hold the reader's attention so closely are where he is dealing with discoveries which are not his own, as, for instance, with the Piltdown discovery. It is perhaps about the first chapter that the chief controversy must rage ; for it is in this chapter that the author explains his criteria for distinguishing artificial from fortuitous chipping of flints. And we must say that, so far as this chapter is concerned, the criticism made at the outset does not apply, for the technical terms used are fully explained, and by making a reasonable effort the uninitiated could follow the technical arguments involved. The reader is told the meaning of such terms as " striking-platform," " cone of percussion " (positive and negative), " Éraillure," " Ripple-marks," " truncated flake-scars," " opposing cones of percussion," etc., etc. And the author then sets out a detailed and absorbing argument in regard to the criteria of artificial flaking. Thus he finds, firstly, that there is a constant angle in human flaking, and divergent angles in fortuitous flaking; also that there is a certain squatness in fortuitous flakes; also that fortuitous flakes tend to go deep into the flint, thus leaving a step at their point of final separation; also that there are numerous prominent ripple-marks in fortuitous flaking; and also that truncated flake-scars are more numerous in fortuitous flaking. It is in this discussion that we have the crux of the whole matter. The reader will observe that we have here not so much any absolute criterion (if there had been an absolute criterion the great discussion would not have been so long as it has been) but a series of converging indications or proofs. To follow the argument properly the chapter must be read in full, and probably read twice. But it is perhaps not out of place to point out that in an argument of this particular character the law of probability has to be considered. If, for instance, we take the first criterion, that of a constant angle in the flakes, we must admit that, whilst the majority of flints showing (to take the simplest case of all) two contiguous chips, would not have those chips at a constant angle if the chipping were fortuitous, yet a certain minority of flints, thus fortuitously chipped, would show the required constant angle. If, for instance, we take the case of two chips, perhaps one out of every eighteen specimens would show the required approximation to the necessary constancy of angle.

But it is obvious that the weight of this argument diminishes at an enormously rapid rate as the total number of chips is increased. Thus, making the same assumption as before, where there are three chips the constant angle would only exist according to the same law of probability in 1 out of 18^2, and where there are four chips the required constant angle would only exist in 1 out of 18^3; and so on, until the minority becomes excessively small. This mathematical aspect of the matter is ignored by the author, although it is obviously germane and essential to the discussion. Moreover, as was pointed out some years ago by Professor Sollas, under certain circumstances, as, for instance, when a flint is firmly embedded in a matrix and subjected to percussion or pressure acting only in one direction, the likelihood of fortuitous flaking showing the required angle would be greatly increased. We think that this aspect of the matter ought to have been discussed in the present work, for it is obviously relevant to the criteria which are brought forward. And we think that the argument could be used with force against many of the eoliths, including some of the Kentian eoliths figured in this work. But, after a careful consideration of the flaking of the Rostro-Carinate implements so beautifully illustrated in the plates, we think that the mathematical argument can scarcely be used effectively against them.

The book is full of other interesting points, especially perhaps in relation to the evolution of the early palæolithic implements themselves. In regard to this matter, the existence of the so-called "side-choppers" appears to be a strong point in favour of Mr. Moir's argument. We notice a slip on page 25. The specimen represented by fig. 6 is said to be of the same provenance as those of figs. 1 and 2; but, according to previous statements, the provenance of the flints of figs. 1 and 2 respectively is not identical. The passage on page 42 would appear to be somewhat unnecessary. The reader is hardly likely to suppose that the makers of palæoliths were conversant with Darwinian philosophy! But Mr. Moir is scrupulously anxious to meet false impressions half-way.

A. G. T.

An Introduction to Palæontology. By A. Morley Davies, D.Sc., A.R.C.S. [Pp. xi + 414, with 100 figures.] (London: T. Murby & Co. Price 12*s*. 6*d*. net.)

Dr. Morley Davies's little book makes much more interesting reading than the majority of textbooks on palæontology. It is not a mere handbook for reference, but is arranged on a plan which is in many respects original, and there are pages which read more like a palæontological essay than a textbook. The first chapter, for instance, which is on the Brachiopoda and is one of the best sections of the book, strikes one at once as possessing some of the charm which distinguished the writings of so many of the leisured naturalists of fifty or a hundred years ago. Whilst the author gives a great many necessary details, and, so far as we can see, gives them usually with scrupulous accuracy, he constantly refers to general questions, which the details he is discussing serve to illustrate. Another excellent feature is that he tells the student more about the living forms of life than geologists usually do. The book has twelve chapters, of which the first four deal respectively with the Brachiopoda, Lamellibranchia, Gastropoda, and Cephalopoda. Chapter VI deals with the Vertebrata, and Chapter X with the Vegetable Kingdom. In the chapter on the Vertebrata one finds that the birds have been reduced to a mere "order" of the Reptilia, an innovation which would, we think, have been more defensible if the Aves (as also the other divisions) had been given the rank of what is technically known as a "sub-class." The classification is in certain other respects unusual; thus, in the Mammalia we find not only "sub-classes" but "super-orders," the Insectivora, Rodentia, Carnivora, Edentata, and Cheiroptera being united into a super-order Unguiculata. The Sirenia are included in the Ungulata, which is raised to the rank of a "super-order."

In the classification of the Reptilia the student might have been told where that important and well-known reptile the *Mosasaurus* is placed; for there is no order Pythonomorpha given. We also notice a slip on page 222, where, speaking of the Ostracodermi, the author says that jaws are found in all other Vertebrata. This slip is corrected later when the Cyclostomata are referred to, but as it stands it might be misleading to the student. The little book is one of the best introductions to a science that we have seen. A. G. T.

Primitive Time-reckoning. By MARTIN P. NILSSON, Professor in the University of Lund. [Pp. xiii + 384.] (Lund: E. W. K. Gleerup; London: Oxford University Press, 1920. Price 21*s*. net.)

WITHIN the last few decades studies in Classical Antiquity have received a new and strong impulse from the comparative investigations of savage and barbarous races. It is enough to remember that we owe such masterpieces of modern humanism as Frazer's *Pausanias* and *Golden Bough* to the cross-fertilisation of these two branches of learning in order to realise its importance. The book under review is also the outcome of the same tendency, its special aim being to throw light on the initial stages of the Greek Calendar by the study of primitive time-reckoning.

A monograph like this one must, in the first place, give an adequate picture of its special subject, that is, a clear exposition of the problem, a well-catalogued and analysed collection of data and sound theoretical conclusions. In all these respects Prof. Nilsson's book is thoroughly satisfactory. The author, in the Introduction, begins with a detailed analysis of the elements of the problem, of the "limited and indeed small number of phenomena which are the same for all peoples all over the globe, and can be combined only in a certain quite small number of ways" in order to provide a people with a system of time-reckoning. These are the periodic changes of the heavenly bodies—sun, moon, and stars—the changes in climatic and meteorological conditions, and the seasonal change of animal and plant life. After a short sketch of how these data must have appeared to crude and unaided observers, Prof. Nilsson proceeds in the first four chapters to describe one after the other the various elementary time divisions—the day, the seasons, the year—as they are found in the customs and lore of primitive peoples. The counting of the moons occupies the next four chapters, the rest but one being devoted to the calendar arrangements of the ancient civilisations—Babylonian, Jewish, Egyptian, to popular time-reckoning of the European nations, to artificial time-divisions, and to the institution of calendar-makers.

The last chapter contains some theoretical conclusions, mainly a summary of various generalisations arrived at by induction in the course of the previous chapters. Thus, the author submits that primitive time-reckoning is concrete—the day is named after the sun, the night after sleep, the year is conceived in terms of the yearly products, chiefly the yield of the harvest. Further, it is remarked that primitive time-indications are "discontinuous and aoristic." They are not conceived of "as divisions of time of definite length; they do not appear as parts of a large whole, limited on both sides by their connection with other divisions of time." The time is conceived as characterised by certain salient points, like definite positions of the sun, definite phases of the moon, which serve as descriptions for the whole contiguous time-period. This is what the author calls the "*pars pro toto* counting of periods." Finally, Prof. Nilsson gives an interesting theory of the origins of Greek time-reckoning, showing that "it was the necessity for the regulation of the religious cult that first created the calendar of Greece," and that this was done with the aid of Babylonian influences.

The theoretical conclusions are modest, and well within the sound inductive inferences of the abundant material collected by the author, who makes a point of his empirical sobriety. "The present work . . . is based upon facts

and their interpretation," in contrast to some "books composed throughout in the spirit of the neo-scholastic school of Durckheim."

A highly specialised monograph runs one risk in particular, that, namely, of isolating its subject too much, of treating it as a self-contained entity, which exists for its own sake. Prof. Nilsson has, perhaps, not altogether succeeded in steering clear of this danger in his otherwise excellent monograph. In describing the primitive systems of time-reckoning, he has not paid sufficient attention to what could be called the *functional* aspect of these systems. By this I mean that he does not make it sufficiently clear to what uses the natives put their systems of moon-naming and moon-fixing, for instance. It is obvious that for ordinary placing of events, past or future, a general indication of the season, or of a typical seasonal occupation, is sufficient. The Melanesians of the Trobriand Islands, among whom I have spent some time in doing field-work, place all past or future events in time by reference to gardening activities. None the less, these natives have a definite and clear system of naming moons, a system which runs side by side with the usual, less definite seasonal divisions. This system of moon-reckoning is of great importance, as it fulfils several vital functions. These natives have elaborate intertribal arrangements, connected with trading, with ceremonial oversea visits, and with big festivals. In order to synchronise the preparations, movements, and meetings of the various tribal sections, a precise system of determining the sequence of moons is necessary. As a rule, the full-moon time of a given month is fixed as the date of the coming event, sometimes a year or two beforehand. Besides this, they have to calculate the date of certain magical and religious ceremonies by means of moon-reckoning. How far and by what means the natives succeed in placing their months always in the same sequence, and more or less within the same place in the solar year, is too complex a question to be dealt with here.

Only the functional study of such arrangements as the various systems of primitive time-reckoning can give us a correct idea of its essential qualities. If we find that a system of time-reckoning fulfils a vital function, and for practical reasons has to be definite and consistent, we gain quite a different and much deeper understanding of its nature. Again, it is clear that among a group of tribes who for one reason or another need to synchronise big intertribal movements, we have to expect there a consistent system of moon-reckoning, as this is the only means they have of defining the dates of distant future events.

I have, therefore, to disagree with Prof. Nilsson when, in speaking of the stage of the Melanesian culture, he says: "We see that at this stage the number of months is indifferent: the question how many months the year has simply does not exist, and consequently there is no need to make the series of moon-months fit into the solar year." I have to disagree, in spite of the high authority of Codrington, whose statements corroborate Prof. Nilsson's views. So far as I can judge, Codrington also failed to account for the functional value of moon-reckoning among his Melanesian tribes.

In fact, it must be emphasised that, if Prof. Nilsson's work is incomplete in the direction here stressed, it is owing to the incompleteness of ethnographic field-work in this respect. And it is perhaps hardly fair to expect that a theoretical ethnologist should go so far as to pick holes in his sources and to try to go further than they go. Being a field-worker myself, however, I may be allowed to emphasise the fact that such books as Prof. Nilsson's are of the utmost value to any observer going to do research among native tribes. An author who, in his study, has the leisure to compare and compile a great number of sources, and who achieves a synthesis of the information available, has a full right to point out the defects in the sources, and to suggest to field-workers new points of view and new methods.

B. MALINOWSKI.

MEDICINE

Physiology and National Needs. Edited by W. D. HALLIBURTON, M.D., LL.D., F.R.C.P., F.R.S. Professor of Physiology, King's College, London. [Pp. vii + 162.] (London: Constable & Co., 1919. Price 8*s*. 6*d* net.)

IN *Physiology and National Needs* Prof. Halliburton edits an attractive series of lectures on the relation of physiology to various aspects of human life.

In the first of these the editor himself deals broadly with the problem of food. Prof. Halliburton, not unnaturally, extols the virtues of the "Handmaid of Medicine" and the valour of those who woo her. On the other hand, he is not fair to the clinicians—the "exceptions" are pretty numerous.

In the second and third Profs. Hopkins and Harden respectively discourse on the accessory factors of diet. We note with satisfaction the reference to Cheadle's pioneer work on scurvy and rickets; and would have seen, with equal pleasure, allusion to Bland Sutton in connection with the latter disease.

Prof. Noel Paton, in the fourth lecture, discusses physiology in relation to gas warfare, and to wound shock, concluding with an account of his work on parathyroid tetany.

Prof. Dendy (fifth lecture) deals with the difficulties incidental to the storage of grain and with the means of surmounting them.

Physicial training is the subject of the sixth and last discourse. Prof. Pembrey, emphatic as ever, urges the disadvantage of drill as compared with spontaneous exercise of the muscular system.

These lectures, of great and varied interest, are addressed to the general reader rather than to the student of academic physiology, and are to be recommended to all as a valuable and enjoyable collection of physiological knowledge.

W. L. S.

Functional Nerve Disease. Edited by H. CRICHTON MILLER, M.A., M.D. [Pp. xi + 205.] (London: Henry Frowde, Hodder & Stoughton, Ltd., 1920. Price 8*s*. 6*d*. net.)

THIS is a useful volume because it is the record of the experience of eleven well-known physicians (including the editor) who have treated functional nervous states in the soldier during the war and since. The editor contributes two chapters, one upon the "Mother-complex," in which he suggests a relationship between neuropathic symptoms and inebriety in the father.

With the possible exception of Drs. George Riddock and Edwin Bramwell and certainly of Dr. William McDougall, who is emphatic as to his position, the contributors follow the teachings of Freud, or the Zurich School, and relate their views in harmony with the doctrines of this school, stress being therefore laid upon the mental rather than upon the physical origin of functional disturbances, especially their origin through emotional conflicts and their relation to the sexual instinct, using in their description the nomenclature and terminology of their teacher. The editor has no use for the materialist, who fails to regard emotion as the cause of disease, and Dr. Riddock asserts that disordered function does not necessarily imply disordered structure, for "in disease alteration of function precedes destruction of tissue," hence the significance of subjective phenomena, whilst Dr. Bramwell lays stress upon the psychical effect of the physical measures of relief.

Dr. E. Prideaux, in a clear and well-reasoned chapter, gives the different views of the origin of neuroses and psychoses, but he asserts that it is in the domain of sex that in peace-time the key to the origin of hysteria may be found.

He repeats the indefinite article "an" before hysterical symptom, which familiarity with "an humble spirit" does not condone. Dr. J. A. Hadfield contributes a very practical chapter upon suggestion and persuasion; he lays much stress upon the reassociation of a repressed emotion with a new and healthy link in the normal cognitive dispositions. Dr. W. H. R. Rivers distinguishes between unconscious suppression and conscious repression, and Dr. Maurice Nicoll adds a technical paper upon Regression, which explains the phrase "I really can't" in Freudian terms; also another upon Psycho-analysis, in which he claims to explain the conflicts of the unconscious mind. Dr. W. H. Bryce contributes a useful paper upon Institution Treatment for shell-shock cases, and Dr. Millais Culpin a thoughtful record of personal examination which is one of the most useful in the book. A helpful summary by Dr. W. McDougall ends the work, and although he is tolerant of Freudian doctrines he describes them as highly speculative Esoteric views. He enters a claim for a compromise between the different schools, e.g. the school which tends to limit its activity to mental exploration and the other which concentrates upon a readjustment by suggestion, persuasion, and re-education with suitable vocational training. He lays great stress upon the practical point that at the root of all the neuroses is amnesia, the great end of treatment being not only to recover the lost memory but to re-establish it and to join it up with the rest of the memory continuum when recovery becomes permanent. This is an interesting volume, and the editor has done a useful service to students of mental and nervous disorders by thus bringing the contributors together.

ROBERT ARMSTRONG-JONES.

Psycho-neuroses of War and Peace. By MILLAIS CULPIN, M.D., F.R.C.S. [Pp 127.] (Cambridge University Press, 1920. Price 10*s*. net.)

DR. CULPIN is widely known as a distinguished surgeon and a capable student of psycho-pathological conditions. He is the Lecturer upon the Psycho-neuroses to the London Hospital, and during the war, both overseas and at home, he has had an unusually extensive experience of the war neuroses, and his practice has been eminently successful in this department. He quotes the views offered to explain the "driving force" of human endeavour, which when shaken by an emotional shock or thwarted, appears after a period of meditation as a form of hysteria, an anxiety state or a psychosis, and the view entertained by Charcot, or that it is a summation of all the instincts (*libido*), as expressed by Jung, or that it is the instinct for power and ambition as maintained by Adler, the psycho-neurosis resulting from a dissociation (Janet), or as the result of suggestion (Babiuski), or an overpowering emotion (Dejerine), or a sexual motive (Freud), or a conflict with the personality (McDougall). If the energy postulated by some of these authorities is thwarted by repression and a conflict ensues, the surviving group of ideas associated with an emotion may result in a psychosis or a psycho-neurosis. The author's views are supported by numerous examples, and the whole volume is easily written. It will form a reliable and readable guide to the study of abnormal mental and nervous disorders.

ROBERT ARMSTRONG-JONES.

Industrial Colonies and Village Settlements for the Consumptive. By SIR GERMAN WOODHEAD, K.B.E., V.D., M.A., M.D., LL.D., and P. C. VARRIER JONES, M.A., M.R.C.S., L.R.C.P. [Pp. xii + 152.] (Cambridge: at the University Press, 1920. Price, paper covers 9*s*., and cloth 10*s*. 6*d*. net.)

THE importance of the correct after-care of the "middle" or still infectious type of consumptive is well brought out in this book. The arguments set forward by the authors as to the general management of the lives of con-

sumptive members of the community are convincing, and their whole treatment of this difficult problem is obviously based on sound lines.

One point in particular is strongly emphasised, and that is that on no account must one allow such patients to foster the idea that they are useless members of the community, and the main theme running throughout the whole conception of colonies and settlements for consumptives is that such institutions should afford the dwellers therein a solid opportunity of proving themselves useful members, able to do a good day's work and obtain a remuneration commensurate with the work done.

With regard to the actual character of the employment, it appears that members skilled in any particular branch of work should, if possible, be given a chance to continue in that branch, although at times a change of occupation might be beneficial: also that, whilst working, plenty of fresh air and sunshine should be an important consideration, and that in the event of suspicious physical signs arising, a rest, under special treatment, should be available in a hospital attached to the colony.

A condition of mental repose is insisted upon by the authors as an important factor in treatment; and it is claimed, and rightly so, that to convert these patients into useful members of society, and afford them an opportunity of earning a living wage in pleasant surroundings and under conditions where strain and anxiety are removed, certainly conduces to a mental state which contributes materially to the success of such treatment.

The business and financial aspects of the colony problem are also gone into, and the authors hold that any expense incurred in the running of such settlements, on the lines they put forward, would be amply repaid by the lasting results obtained.

By means of such a scheme as described it is possible in numerous instances to use the colony as a link between the sanatorium and the patient's former conditions of existence, although it must be acknowledged that in many cases it is undesirable for members to return to such conditions, especially where overcrowding and other vitiating circumstances obtain.

It cannot be denied that the general scheme outlined in this volume is one which goes a long way towards solving a problem of immense national importance.

H. A. H.

MISCELLANEOUS

Australian Meteorology. By Griffith Taylor, D.Sc., B.E., B.A., F.G.S., F.R.G.S. [Pp. xi + 312, with 229 figures.] (Oxford: at the Clarendon Press, 1920. Price 12*s*. 6*d*. net.)

The student of Meteorology in Australia needs a special textbook, for Australia is so very differently situated from England, Germany, and America that the standard works of these countries have only a limited application there. It is only the southern coasts that experience the prevailing westerly winds and frequent depressions so familiar to us, and over a great part of the country heat and drought exist to a degree exceeded in but few parts of the world. In addition, the relation between the direction of the wind and the horizontal pressure gradient is reversed for the Southern Hemisphere.

Dr. Griffith Taylor deals attractively with the climatological portions of his work, and shows much ingenuity in his method of conveying information diagrammatically. In the physics of the free atmosphere, however, he does not appear to be so much at home. For instance, as a picture of conditions aloft, his comparison of the troposphere and stratosphere (p. 229) to the inside and outside of a greenhouse which is heated by a stove is useful; but he goes further than this, and would have it that the clouds at the Cirrus level

act the part of the glass of the greenhouse and are actually the cause of the vertical distribution of temperature. The continued existence of the "tropopause" during cloudless weather is hard to explain if his hypothesis is true, and even more so the fact that in North-West Europe it is during cloudy cyclonic weather and not fine anticyclonic conditions that the stratosphere tends to lose its sharply defined under edge.

But this is a minor point, and the known facts about the free atmosphere have been clearly and accurately set out.

Although the book is written to meet Australian needs, the European reader should find much of interest, especially when reading about the "Southerly Burster" and the violence of the tropical revolving-storms. He is not likely, however, to accept the author's theories concerning the origin of the shallow "lows" of Northern Australia.

The book is a curious mixture of accurate information in many branches of meteorology and daring speculation in a few, and deserves a wide circulation.

E. V. Newnham.

Secretum Secretorum. Cum Glossis et Notulis Tractatus Brevis et Utilis ad Declarandum Quedam Obscura Dicta Fratris Rogeri nunc primum edidit Robert Steele Accedunt Versio Anglicana ex Arabico Edita per A. S. Fulton Versio Vetusta Anglo-Normanica nunc Primum Edita. (Opera Hactenus Inedita Rogeri Baconi, Fasc. V.) [Pp. lxiv + 318, with 7 illustrations]. (Oxonia: E. Typographeo Clarendoniano. Price 28*s*. net.)

The publication of Roger Bacon's works, interrupted by the war, has been resumed, and Mr. Robert Steele and those associated with him are to be congratulated on the appearance of the volume before us, which is the fifth instalment of Mr. Steele's edition of the hitherto unpublished works of Bacon. The preparation of the volume, with its mass of learned information, must have entailed much labour; and we have nothing but praise for the way in which the work has been carried out.

The chief interest of the work before us lies in its revelation of the outlook of Roger Bacon and his contemporaries. Science, like every other human achievement, is of slow growth; and to some people its past history can be as interesting almost as are its present results, for it reveals the epic struggle of the human mind in its endeavour to come to an understanding with the forces of nature and the mysteries of existence. As an historical document, the *Secretum Secretorum*, with Bacon's Introduction and Glosses, is certainly of interest, although regarded objectively and without an historic sense it may appear to be a rather weird hotch-potch of odds and ends. What is perhaps most striking is the extent to which an intellectual giant like Roger Bacon remained a child of his age, steeped in theological ways of looking at things, and, while denouncing some superstitions of his day, yet clinging to many others. The flame is mingled with the smoke, and disentangles itself but slowly.

The *Secretum Secretorum* professes to have been written by Aristotle in his old age as a secret manual for the guidance of his former pupil, Alexander the Great. It claims to give the quintessence of knowledge and wisdom in a concise form suitable for a busy man of affairs who wants "wisdom while you wait." Kingship and state-craft, the wise choice of counsellors and ambassadors, the administration of justice and the collection of taxes, the organisation of armies and the conduct of war, physiology and physiognomy, medicine and magic, astrology and alchemy—such are the topics of instruction contained in this manual. There is much wisdom in some of its doctrines.

The best medicine, we are told, is moderation in all things. Drinking hot water each morning is prescribed as a cure for many ills. Combing the hair, brushing the teeth, and walking exercise are strongly recommended as part of the regular morning routine; and there is a touch of modernity about the praise of baths and late dinners. But some of the prescriptions are decidedly quaint, though not quite so quaint as those of ancient Egypt. Pearls, rubies, emeralds, and gold-dust are good things no doubt, but hardly as ingredients in physic! And the extent to which complex astrological calculations enter into medicinal practice compares unfavourably with the more sober methods of Arabian medicine of the time. Knowledge must have been esteemed power indeed when it was deemed possible to make the stars in their courses fight the battles of man!

As a matter of fact, the *Secretum Secretorum* is not Aristotelian at all. It contains some Greek ideas, but was probably written originally in Syriac between the seventh and ninth centuries A.D. It was translated into Arabic early in the ninth century, thence into Hebrew in the twelfth century, and from Hebrew into Italian, German, and the Romance languages. Even metrical versions were made of it—one such, the Anglo-Norman version of Piere d'Abernun, is included in the present volume. By the beginning of the thirteenth century there were current many MSS. of the *Secretum Secretorum* differing to some extent in substance and in arrangement. Roger Bacon, believing it to be of genuine Aristotelian origin, edited the treatise and provided it with an Introduction and Notes. The editing appears to have been carried out before 1257, but the Introduction was written only about 1270.

Bacon's Introduction is really a complete little treatise on astrology. It begins with a vindication of astrology as opposed to magic. Bacon distinguishes between and true false mathematicians. (Mathematics, he says, includes four sciences—geometry, arithmetic, music, and astrology, which last contains judiciary and operative astronomy.) The false mathematicians are mere magicians who resort to witches' charms, talismans, sacrifices, and demons, and believe that the power of the stars is irresistible, leaving no room for the exercise of free-will. True mathematicians, on the other hand, presume no such necessity; they do not attempt to foretell all events, nor any with certainty. They foresee possibilities, but allow for free-will. Their art consists in knowing the positions of the planets in the signs of the zodiac and their relation to each other and the fixed stars, so that they can estimate their influence on mundane matters. But God, or even a good Christian, may alter impending events. Such alteration may also be effected in animate bodies. The human body is disposed, though not forced, to various actions by various constellations. Just as a man may do things in the company of his friends which he would not have done otherwise, so the constellations may influence his disposition without coercing him. Having vindicated astrology to his own satisfaction, Bacon proceeds to give a clear exposition of the turbid subject: he describes the movements of the ten heavens round the earth, and expounds the lore of the planets and the signs of the zodiac—their "properties," "aspects," "dignities," etc. There is some soul of goodness in things evil. Even astrology may have done some good. The restrictions which it imposed on the times for blood-letting and taking medicine must have been a blessing in days of rough surgery and noisome medicine. But the extent to which Christendom generally was addicted to astrology in the thirteenth century is certainly remarkable, considering the way in which Arabian thinkers managed to keep clear of it. More remarkable still is it to observe a genius like Roger Bacon badly smitten with magic and alchemy. But *Natura non agit per saltum*: even man does not advance by leaps.

A. Wolf.

The History of Social Development. By Dr. F. Müller-Lyer. Translated by E. C. and H. A. Lake. [Pp. 362.] (London: George Allen & Unwin, 1920. Price 18*s*. net.)

Dr. Müller-Lyer's *Phasen der Kultur*, of which this book is a translation, is the first of a series of six volumes in which the author gives a comprehensive treatment of the whole course of man's social development. The title of this volume belongs rather to the work as a whole than to this first part, which deals primarily with the economic stages of culture. The first book (63 pp.), however, forms a general introduction to the whole series, dealing briefly with the nature of the science of culture and with the origin of the human race. At the end of the volume there is an even more general disquisition on the nature of progress, the age and probable duration of the earth, and the relation of culture and happiness. This is characteristic of the author's whole treatment of his subject, which tends to excessive generalisation. Moreover, the ethnological and geographical aspects of culture are unduly neglected, and sometimes forcibly subordinated to the author's economic schematisation. What real value can there be in a classification (p. 95) which places in a single economic-cultural group such diverse peoples as the Bedouin, the naked Dinka of the Nile swamps, and the Galla of the well-watered Abyssinian highlands? A similar disregard of modern ethnology is shown by the author when he describes the peoples of the Mediterranean area as consisting of three races, Semitic, Hamitic, and Aryan; and the same passage contains a truly surprising reference to the Assyrio-Babylonians of Asia Minor.

However, the central thesis of the book is an economic one. Dr. Müller-Lyer divides the economic history of the world into three epochs: that of class organisation, which includes all primitive peoples; that of industrial organisation, which includes the societies of early classical times and of mediæval Europe; and that of capitalistic organisation, which includes in its earlier stage the highest development of the classical world, and Western Europe from the sixteenth to the eighteenth centuries, and in its later stage the modern world only. Finally, he regards 1881 as marking the beginning of a new epoch—that of world economy and socialised industry.

The book contains a very full analysis of contents but no index. The references to authorities quoted in the text are insufficient, and contain numerous misprints. It requires, for instance, some effort of mind to realise that the reference at the foot of p. 186, Conrado Handw. u. Staatsw., is intended to refer to an unspecified volume and article in Conrad's Handwörterbuch der Staatswissenschaften. The translation also is not free from mistakes, such as Metoken for Metics, and "the Third Position" instead of "the Third Estate."

H. O. Dawson.

Optical Projection: A Treatise on the Use of the Lantern in Exhibitions and Scientific Demonstrations. By Lewis Wright. Fifth Edition. Rewritten and brought up to date by Russell S. Wright, M.I.E.E. In two parts. Part I, The Projection of Lantern Slides. [Pp. viii + 87.] (London: Longmans, Green & Co., 1920. Price 4*s*. 6*d*. net.)

The first edition of this work was written by Mr. Lewis Wright as long ago as 1890. It passed through three successive editions, and has rightly been regarded as the standard work on lantern projection apparatus during that period. The original work covered practically the whole range of optical projection, but this edition covers that dealing with the projection of lantern slides only; the second part of the demonstration of opaque and microscopic objects and other scientific phenomena is to be dealt with at a later date. There is considerable need for a book of this type, as lantern demonstrations

are only too often but poorly carried out, and very often detract somewhat seriously from the value of an otherwise good lecture. The most important part of the book is that devoted to illuminants, and it is obvious that that is, after all, the crux of the whole question, as on the illuminant available depends in large part the type of the other apparatus that must be used. Oil lamps, acetylene, and limelight are referred to and described ; but it is to electrical illuminants that the main part of this section is devoted. The electric arc is the most important of these, and the description given and the instructions for working it are exceedingly good.

There are minor inaccuracies, such, for instance, as where the passing of luminous carbon particles from one carbon to the other is referred to as if it were the actual source of light. This, of course, is not the case, the source being, in the case of continuous current, the crater on the positive carbon. The length of arc is also arbitrarily laid down as being one-eighth of an inch ; but this again would vary with the amount of current and the size of the carbons.

The types of arc lamps described and figured are apparently by one maker only, and it cannot be said that these are without any good rivals. Whether, too, the right-angled type of electric is only satisfactory when used with small currents is questionable ; in fact, they do prove exceedingly satisfactory with currents as large as twenty and thirty amperes.

The actual instruction in the use of the arc and directions for calculating the necessary resistance are very lucid, and exactly the sort of information that a lantern user requires.

The optical part of the equipment is also described, and sufficient of its principles elucidated to enable those who have to use the apparatus to understand what they need do. It is to be regretted that only the old type of lantern body is figured, and no suggestion is made as to the much newer and more convenient type in which the apparatus partakes of the character of an optical bench.

In general the book may be recommended to all those who have occasion to use the lantern for educational or any other purpose.

J. E. B.

BOOKS RECEIVED

(Publishers are requested to notify prices)

The Early Mathematical Manuscripts of Leibniz. Translated from the Latin texts published by Emmanuel Gerhardt, with critical and historical notes. By J. M. Child. London and Chicago: The Open Court Publishing Co., 1920. (Pp. v + 238.) Price \$1·50.

Girolamo Saccheri's Euclides Vindicatus. Edited and translated by George Bruce Halsted, A.M. (Princeton), Ph.D. (Johns Hopkins). London and Chicago : The Open Court Publishing Company, 1920. (Pp. xxx + 246.) Price \$2.

Algèbre à Deux Dimensions. Par M. Stuyvaert, Docteur Spécial en Mathématiques, Correspondant de l'Académie Royale de Belgique, Professeur à l'Université de Gand. Gand : Van Rysselberghe et Rombaut, Editeurs (Ancient Librairie E. van Geothem), Rue des Foulons, 1, 1920. (Pp. 223.) Price 12.50 fcs. net.

The Mathematical Theory of Electricity and Magnetism. By J. H. Jeans, M.A., F.R.S. Formerly Stokes Lecturer in Applied Mathematics in the University of Cambridge, sometime Professor of Applied Mathematics in Princeton University. Fourth Edition. Cambridge : at the University Press, 1920. (Pp. vii + 627.) Price 24*s*. net.

The Dynamical Theory of Cases. By J. H. Jeans, LL.D., F.R.S. Formerly Stokes Lecturer in Applied Mathematics in the University of Cambridge, sometime Professor of Applied Mathematics in Princeton University. Third Edition. Cambridge: at the University Press, 1921. (Pp. vii + 442.) Price 30*s*. net.

Bibliotheca Chemico-Mathematica. Catalogue of Works in many Tongues on Exact and Applied Science, with a Subject-Index. Compiled and Annotated by H. Z. and H. C. S. London: Henry Sotheran & Co., 140 Strand, W.C.2, 1921. (Pp. xii + 964, with 127 plates.) Price £3 3*s*. net.

Clouds: A Descriptive Illustrated Guide-Book to the Observation and Classification of Clouds. By George Aubourne Clarke, F.R.P.S., F.R. Met. Soc. With a Preface by Sir Napier Shaw, LL.D., Sc.D., F.R.S., Director of the Meteorological Office. London: Constable & Co., 10, Orange Street, W.C., 1920. (Pp. xvi + 136, with 40 plates and 17 figures.) Price 21*s*. net.

The World of Sound. Six Lectures delivered before a Juvenile Auditory at the Royal Institution, Christmas, 1919. By Sir William Bragg, K.B.E., D.Sc., F.R.S., Hon. Fellow of Trinity College, Cambridge, Quain Professor of Physics in the University of London, London: G. Bell & Sons, 1920. (Pp. viii + 196.) Price 6*s*. net.

The New Physics. By Albert C. Crehore, Ph.D. San Francisco: Journal of Electricity. (Pp. viii + 111.)

Properties of Steam and Thermodynamic Theory of Turbines. By H. L. Callendar, M.A., LL.D., F.R.S., Late Fellow of Trinity College, Cambridge, Professor of Physics in the Imperial College of Science and Technology. London: Edward Arnold, 1920. (Pp. xi + 531.) Price 40*s*. net.

Statique Dynamique. Par M. Stuyvaert, Correspondant de l'Académie Royale de Belgique, Professeur à l'Université de Gand. Gand: Van Rysselberghe et Rombaut, Editeurs (Ancient Librairie E. van Goethem), Rue des Foulons, 1, 1920. (Pp. 205.) Price 20 fcs. net.

A Textbook of Physics. Including a Collection of Examples and Questions. By W. Watson, C.M.G., A.R.C.S., D.Sc. (London), F.R.S. Seventh Edition, Revised by Herbert Moss, M.Sc. (Lond.) A.R.C.S., D.I.C. London: Longmans, Green & Co., 39, Paternoster Row, 1920. (Pp. xxvi + 976.) Price 21*s*. net.

The Principle of Relativity. Original Papers by A. Einstein and H. Minkowski. Translated into English by M. N. Saha and S. N. Bose. Lectures on Physics and Applied Mathematics, University College of Science, Calcutta University. With an Historical Introduction by P. C. Mahalanobis, Professor of Physics, Presidency College, Calcutta. Calcutta: Published by the University, 1920. (Pp. xxiii + 186.)

The Absolute Relations of Time and Space. By Alfred A. Robb, Sc.D. D.Sc., Ph.D. Cambridge: at the University Press, 1921. (Pp. viii + 60.) Price 5*s*. net.

The Theory of Relativity. By Robert D. Carmichael, Professor of Mathematics in the University of Illinois, No. 12. Second Edition. Mathematical Monographs. Edited by Mansfield Merriman and Robert S. Woodward. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. 112.) Price 8*s*. 6*d*. net.

The Scientific Papers of Bertram Hopkinson, C.M.G., M.A., F.R.S., Fellow of King's College and Professor of Mechanism and Applied Mechanics in the University of Cambridge, collected and arranged by Sir J. Alfred Ewing, K.C.B., F.R.S., Vice-Chancellor and Principal of the University of Edinburgh, and Sir Joseph Larmor, F.R.S., M.P., Fellow of St. John's College and Lucasian Professor of Mathematics. Cambridge: at the University Press, 1921. (Pp. xxv + 480.) Price 63*s*. net.

Laboratory Projects in Physics. A Manual of Practical Experiments for Beginners. By Frederick F. Good, A.M., Instructor in the School of Practical Arts and in the School of Education, Columbia University, New York City. New York: The Macmillan Company, 1920. (Pp. xiii + 267.) Price 9*s*. net.

A System of Physical Chemistry. By William McC. Lewis, M.A., D.Sc., Brunner Professor of Physical Chemistry in the University of Liverpool. In Three Volumes. Vol. II. Thermodynamics. Third Edition. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. xiii + 454, with diagrams.) Price 15*s*. net.

Principles of Biochemistry for Students of Medicine, Agriculture, and Related Sciences. By T. Brailsford Robertson, Ph.D., D.Sc., Professor of Physiology and Biochemistry in the University of Adelaide, South Australia. Philadelphia and New York: Lea & Febiger, 1920. (Pp. xii + 633, with 49 engravings.)

Practical Physiological Chemistry. By Sydney W. Cole, M.A., University Lecturer in Medical Chemistry, Cambridge, with an Introduction by F. G. Hopkins, M.B., D.Sc., F.R.C.P., F.R.S., Professor of Biochemistry. Sixth Edition. Cambridge: W. Heffer & Sons, 1920. (Pp. xvi + 405, with 58 figures.) Price 16*s*. net.

Recent Advances in Organic Chemistry. By Alfred W. Stewart, D.Sc., with an Introduction by J. Norman Collie, LL.D., F.R.S., Fourth Edition. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. xvi + 359.) Price 21*s*. net.

La Chemie et la Vie. Par Georges Bohn, Directeur de Laboratoire à la Sorbonne, et Anna Drzewina, Docteur ès Sciences. Paris: Ernest Flammarion, Éditeur, 26 Rue Racine, 1920. (Pp. 271, and 4 figures.) Price 7.50 fcs. net.

A Textbook of Organic Chemistry. By A. F. Holleman, Ph.D., F.R.A. Amst. Professor Ordinarius in the University of Amsterdam. Edited by A. Jamieson Walker, Ph.L., B.A., F.I.C. Assisted by Owen E. Mott, O.B.E., Ph.D., F.I.C., with the co-operation of the Author. Fifth Edition, completely revised. New York: John Wiley & Sons. London: Chapman & Hall, 1920. (Pp. xviii + 642.)

Organic Medicinal Chemicals (Synthetic and Natural). By M. Barrowcliff, M.B.E., F.I.C., and Francis B. Carr, C.B.E., F.I.C. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1921. (Pp. xiii + 331, with 35 figures.) Price 15*s*. net.

An Introduction to the Chemistry of Plant Products. Vol. I. On the Nature and Significance of the Commoner Organic Compounds of Plants. By Paul Haas, D.Sc., Ph.D., and T. G. Hill, A.R.C.S., F.L.S. Third Edition. London: Longmans, Green & Co., 39, Paternoster Row, 1921. (Pp. xiii + 414.) Price 16*s*. net.

A Textbook of Practical Chemistry. By G. F. Hood, M.A., B.Sc., Master, Nottingham High School, and J. A. Carpenter, M.A. London: J. and A. Churchill, 7 Great Marlborough Street, 1921. (Pp. xii + 527, with 162 illustrations.) Price 21*s*. net.

A Textbook of Inorganic Chemistry for University Students. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry at the East London College, University of London. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xii + 1063.) Price 25*s*. net.

A Dictionary of Applied Chemistry. By Sir Edward Thorpe, C.B., LL.D., F.R.S. Assisted by Eminent Contributors. Vol. I. Revised and Enlarged. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. x + 752.) Price 60*s*. net.

Inorganic Chemistry. By E. I. Lewis, M.A., B.Sc., with Introductory Note by Sir Richard Threlfall, K.B.E., M.A., D.Sc., F.R.S. Cambridge: at the University Press, 1920. (Pp. xv + 443.) Price 9*s*. net.

Mineralogy. An Introduction to the Study of Minerals and Crystals. By Edward Henry Kraus, Ph.D., Sc.D., and Walter Fred Hunt, Ph.D. New York: McGraw-Hill Book Co., 239, West 39th Street. London: 6 and 8 Bouverie Street, E.C.4, 1920. (Pp. xiv + 561, with 696 figures.)

Geology of the Non-Metallic Mineral Deposits other than Silicates. Vol. I. Principles of Salt Deposition. By Amadeus W. Grabau, S.M., S.D. New York: McGraw-Hill Book Co., 239 West 39th Street; London: 6 and 8 Bouverie Street, E.C.4, 1920. (Pp. xvi + 435, with 125 figures.)

Handbook of Metallurgy. By Dr. Carl Schnabel. Illustrated by Henry Louis, M.A., D.Sc. Third Edition. Revised by the Translator. Vol. I. Copper-Lead-Silver-Gold. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xxi + 1171, with 705 figures.) Price 40*s*. net.

The Thermionic Vacuum Tube and its Applications. By H. J. van der Bijl, M.A., Ph.L., M.Am.I.E.E., M.I.R.S., Mem. Am. Phys. Soc., Scientific and Technical Adviser, Dept. of Mines and Industries, Union of South Africa. New York: McGraw-Hill Book Co., 239 West 39th Street; London: 8 Bouverie Street, E.C.4, 1920. (Pp. xix + 391.)

Soil Alkali: its Origin, Nature, and Treatment. By Franklin Stewart Harris, Ph.D., Director and Agronomist, Utah Agricultural Experiment Station, and Professor of Agronomy, Utah Agricultural College. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. xvi + 258, with 33 figures.) Price 13*s*. 6*d*. net.

Devonian Floras: a Study of the Origin of Cormophyta. By E. A. Newall Arber, M.A., Sc.D. Trinity College, Cambridge, University Demonstrator in Palæobotany. With a Preface by D. H. Scott, M.A., LL.D., Ph.D., F.R.S. Cambridge: at the University Press, 1921. (Pp. xiv + 100, with a frontispiece and 47 figures.) Price 17*s*. 6*d*. net.

The Bases of Agricultural Practice and Economics in the United Provinces, India. By H. Martin Leake, M.A., Sc.D., F.L.S., Director of Agriculture, United Provinces. With a Foreword by J. Mackenna, C.I.E., I.C.S. Cambridge: W. Heffer & Sons, 1921. (Pp. xiii + 277.) Price 15*s*. net.

Botany with Agricultural Applications. By John N. Martin, Ph.D., Professor in Botany at the Iowa State College of Agriculture and Mechanic Arts. Second Edition, revised. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. xii + 604.) Price 21*s*. net.

Zoology. An Elementary Textbook. By Sir A. E. Shipley, G.C.B.E., Sc.D., F.R.S., Master of Christ's College, Cambridge, and E. W. MacBride, M.A., D.Sc., F.R.S., Professor of Zoology in the Imperial College of Science and Technology. Fourth Edition. London. Cambridge: at the University Press, 1920. (Pp. xx + 752, with 360 figures.) Price 20*s*. net.

A Textbook of Biology for Students in General Medical and Technical Courses. By William Martin Smallwood, Ph.D. Fourth Edition, thoroughly revised. Philadelphia and New York: Lea & Febiger, 1920. (Pp. xvi + 308, with 229 figures.) Price $3·50.

A Naturalist in Himalaya. By R. W. G. Hingston, M.C., M.B., Fellow of the Royal Geographical and Zoological Societies. London: H. F. and G. Witherby, 326, High Holborn, 1920. (Pp. xii + 300, with 25 illustrations.) Price 18*s*. net.

An Introduction to Zoology for Medical Students. By C. H. O'Donoghue, D.Sc., F.Z.S., Professor of Zoology, University of Manitoba; Fellow of University College, London. London: G. Bell & Sons, 1920. (Pp. x + 501.) Price 16*s*. net.

The Origin of Man and of his Superstitions. By Carveth Read, M.A., Lecturer on Comparative Psychology in University College, London. Cambridge: at the University Press, 1920. (Pp. xii + 350.) Price 18*s*. net.

Standard Method of Testing Juvenile Mentality by the Binet-Simon Scale and the Porteus Scale of Performance Tests. A Uniform Procedure and Analysis. By Norbert J. Melville. With an Introduction by William Healy, M.D. Second Edition, enlarged. Philadelphia and London: J. B. Lippincott Company. (Pp. xi + 162.) Price 12*s*. 6*d*. net.

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INDEX TO VOL. XV (1920—1921)

1. ARTICLES, ESSAYS, ESSAY-REVIEWS, CORRESPONDENCE, AND NOTES

II. AUTHORS OF ARTICLES

III. AUTHORS OF BOOKS REVIEWED